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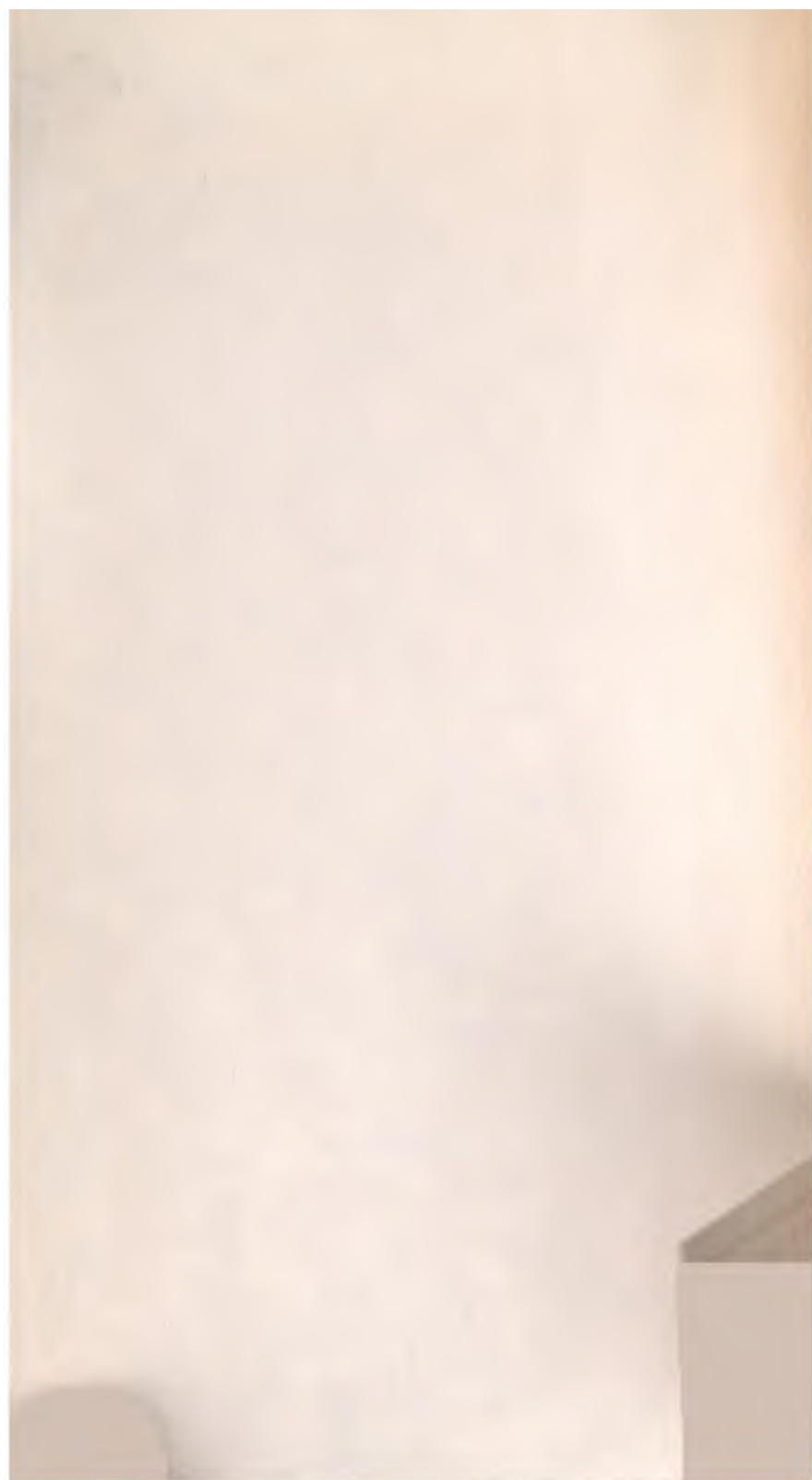
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No. 1.

SYLLABUS OF LECTURES ON THE PSYCHOLOGY
OF PAIN AND PLEASURE.

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This syllabus principally consists of the substance of a course of lectures on the Psychology of Feeling delivered at Clark University during the past academic year.

§ 1. The department of study which it is here proposed to pass in review has for its subject-matter the facts of mind known as agreeableness and disagreeableness.

While common usage is apt to confine the terms pain and pleasure to particular varieties and marked degrees of agreeableness and disagreeableness, that the words at least denote species of the latter genera, will hardly be denied; and without doing much violence to the habitudes of English speech, their acceptation may be extended to cover any kind and any grade of the agreeable and disagreeable respectively. (The corresponding words in French, German and Italian have been used of late by many writers in this widest possible sense. Cf. Fechner: "Vorschule der Ästhetik," 1876, Vol. I. p. 11.)

§ 2. In uniting the investigation of these two mental facts into one branch of inquiry, it is tacitly assumed that they are species in a psychological genus. This assumption becomes explicit in the use of terms like the French "Sensibilité,"

the German "Gefühl," and the English "Feeling." Yet these words are of ambiguous import, having at least two other possible meanings besides that of a psychological genus including pain and pleasure as species. "Sensibilité" might, from its etymology, mean that which is presented to us in the sense; and "Feeling" is sometimes used for presentation in any form. Again, all three of the words can be taken to mean emotion, a phenomenon of mind which, although it may always be either pleasant or unpleasant, is different from the facts of pleasantness and unpleasantness themselves. For the generic fact of which these latter are species, there exists in English no unambiguous name. Moreover, were there such a term, its use to describe the domain of inquiry here had in mind would involve the assumption that pain and pleasure are the only species in the genus. While the existence of the latter is not, at present, often questioned, it cannot be regarded as settled that this dichotomy is an exhaustive division of it. May there not, or must there not, exist, it has been asked, a "feeling" of Indifference as well as "feelings" of Pain and Pleasure? (Cf. the discussion on Feeling as Indifference between Professors Bain, Sully and others: *Mind*, Vols. XII., XIII. and XIV.) Yet the doubt itself as to the existence of the former species of the genus here called "feeling," gives justification for taking the latter pair as the object of a separate inquiry.

§ 3. A fundamental psychological doctrine, to which the writer holds, affirms the ultimate categories of psychical fact to be three in number: (1) presentation, or mental content in general, whether sensory, emotional or representative; (2) will, or the fact of resolve; (3) the generic fact of which agreeableness and disagreeableness are special forms. The mental life may, on this view, be looked at in three aspects: as the theatre of thought (in the widest sense), of act (whether the psychic facts known as desire and impulse involve this element or not, becomes a question of interest), and of pain and pleasure (possibly also, indifference). The domain of inquiry here to be reviewed assumes, in consequence, the importance of at least the main element in one of three grand divisions of psychology. We are led to look for manifestations of the pain and pleasure genus in any manifestations of mind, to think of the sphere of our inquiry into it as co-extensive with the psychic life itself. (In the "Vorschule der Ästhetik," Vol. I. p. 36, Fechner remarks that the term Hedonik has been proposed to designate the general doctrine of pleasure and pain, "a doctrine which traces out all bearings, inner and outer, of pleasure

and pain in the universe; in their relations, whether of notion or of principle, enchainments, modes of origin and of application.")

§ 4. Although the worth of things is admitted to depend in good measure upon fact of the pain and pleasure genus with which they are complicated in experience, the doctrine of this element of mind cannot be said to have reached either the volume or the solidity of other doctrines of psychology less intimately connected with values. There are unusual difficulties surrounding the investigation of the agreeable and disagreeable which go far to explain this fact. It may be claimed that no state of pain or pleasure of higher intensity can immediately become the object of introspective examination: it must be scrutinized from a distance in time. Further, if the agreeableness and disagreeableness themselves "are not presented, they cannot be represented," and "any knowledge we have of them must be in some way constructive or mediate." (Professor J. Ward: *Modern Psychology, Mind*, N. S. 5.) In a word, the habitude of self-consciousness keeps the psychologist on lower levels of pleasure, while his powers of observation fail him on higher levels of pain, and even the lower degrees he is incapable of studying as he can the facts of presentation. Yet from the application in this field of the varied methods of observation and experiment which have in recent years given so strong an impulse to other branches of psychology, results of value may be expected.

§ 5. The following pages contain a review of some principal topics in the psychology of pain and pleasure, with references to writers. The accompanying scheme is adhered to in the exposition:

I.—GENERAL QUESTIONS.

1. *Psychological*:

- (1) Nature. § 6.
- (2) Logical relations (of resemblance, difference, inherence)
 - (a) to other psychic fact: pain and pleasure are—
 - (a) species of presentation. § 7.
 - or (β) characters of presentation. § 8.
 - or (γ) the original form of consciousness. § 9.
 - or (δ) fundamentally one with the fact of will. § 10.
 - or (ϵ) independent manifestations of mind. § 11.
 - (b) among themselves. § 12.

(3) Actual relations (of accompaniment, in quality quantity)

(a) to presentation—

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(β) presentational conditions of pain and pleasu

Quantitative. § 14.

Psychical aid and conflict. § 15.

Genesis of conditions. § 16.

(b) to will. § 17.

(c) among themselves. §§ 18-20.

2. *Psychophysical*:

Theories of vital hindrance and furtherance. § 21.

Quantitative theories. § 22.

Fechner's theory of stability. § 23.

3. *Philosophical*:

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(b) Bodily cravings. §§ 29-31.

(c) Lower senses. § 32.

(d) Hearing. §§ 33-36.

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2. *Special Conditions*:

(1) Morbid:

(a) Melancholia and Mania. §§ 45-46.

(b) Impulsive Insanity. § 47.

(c) The relation of Genius and Insanity. § 48.

(2) Onirotic (*ὄνειρωτικός*, consisting in dreaming). §

(a) Dreaming. § 50.

(b) Natural Somnambulism. § 51.

(c) Hypnotism. § 52.

(d) Shock. § 53.

(e) Narcosis. § 54.

(f) Emotion. § 55.

(α) Wonder, Horror, etc. § 56.

(β) Love. § 57.

(γ) Religion. § 58.

(δ) Beauty. §§ 59-61.

The Fine Arts.	§ 62.
Picture.	§ 63.
Sculpture.	§ 64.
Architecture.	§ 65.
Ornament.	§ 66.
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Drama.	§ 69.
Literature.	§ 70.
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III.—THEORY OF HABIT.

§§ 72-80.

I.—GENERAL QUESTIONS.

Psychological:

6. (1) Nature. Pain and pleasure are generally recognized as ultimate facts of mind. ("Pain and Pleasure themselves taken pure and apart from all subsidiary determinations are simple, not further analyzable determinations of our soul." Fechner: "Vorschule," I. p. 8.) Yet the question is debated whether each does not exist in different kinds. (Cf. J. S. Mill: "Some *kinds* of Pleasure are more valuable and valuable than others." "Utilitarianism," Chap.

Professor Ward contra, to whom feeling itself varies only in intensity and duration." Art. "Psychology," *Encyc. Britannica*, p. 71.) It is customary to speak of them as having a subjective reference in opposition to presentation which is referred to an object. (Cf. Kant: "Kritik of Judgment," Section 3, Bernard's translation: "The green color of the meadows belongs to *objective* sensation, as a perception of an object of sense: the pleasantness of this belongs to *subjective* sensation, by which no object is represented, *i. e.*, feeling . . .")

(2) Logical relations: (a) to other psychic fact.

7. (a). Pain and pleasure are species of presentation. Locke calls them "simple ideas." ("Human Understanding," Book II. Chap. XX.) The modern assumption of the physical nature of pain leads to a classification of physical sensations among sensations; but according to Prof. Ward, by a consequence consequent upon "the use of one word pain for certain physical sensations and the purely subjective state." (Art. "Psychology," p. 40.) Pleasure and pain are placed on a footing with other senses by Dr. H. Nichols in "The Origin of

Pleasure and Pain." (*Philosophical Review*, Nos. 4 and 5; criticism by Mr. H. R. Marshall in No. 6 of same review.)

§ 8. (β). Pain and pleasure are characters of presentation. Utterances of Descartes, Spinoza and Leibnitz may be thus interpreted. According to Descartes (letter to the Princess Elizabeth), "All our pleasure is nothing more than the consciousness of some one or other of our perfections." Spinoza defines pleasure as "a passive state wherein the mind passes to a greater perfection;" and pain as "a passive state wherein the mind passes to a lesser perfection." (*Ethics*, III. Prop. XI.) Leibnitz writes: "Pleasure is the feeling of a perfection or excellence whether in ourselves or in something else. . . . One does not always notice wherein the perfection of agreeable things lies, . . . meanwhile it is felt by the soul if not by the understanding." ("Von der Glückseligkeit," *Opera Phil.* Ed. Erdmann LXXVIII.) These quotations may be conceived to express the doctrine that presentation of a certain form, viz., inadequate idea, exhibits the characters known as pleasure or pain, according as it involves or tends toward one or the other of two internal determinations, viz., perfection or imperfection. (Yet Wolff bases pleasure on the presentation of perfection, and not on the perfection of presentation—"Pleasure is the intuitive cognition of some perfection or other, whether true or false."—"Psychologia Empirica," 1738, Section 511,—ascribing his definition to Descartes.) The opinion that pain and pleasure are "*quales* of all presentations comprising our psychic life as we know it" has been recently maintained by Mr. H. R. Marshall. *Mind*, No. 56, Oct. 1889.)

§ 9. (γ). Pain and pleasure are the original content of consciousness, presentation their derivative. This theory has been advanced by Horwicz in his "*Psychologische Analysen*," Vol. I. 1872, Vol. II. 1875-78.) Defining Feeling as "the condition of pleasure or pain with which we accompany various soul processes," he writes, "Feeling is, according to our view, the earliest, most elementary product of our soul-life; it is the earliest and sole content of the consciousness, and the main-spring of the entire soul-development. (II. 1, p. 177.) "Inasmuch as all ideation rests on sensation, it must be traced back entirely to the feeling of the agreeable and the disagreeable. For all sensation is . . . originally feeling; it is only through a set of processes that we have called habituation (active and passive), memory and localization, projection and apperception, that the agreeable and disagreeable, originally only subjective, has become objective perception." (II. 1, p. 4.)

§ 10. (δ). Pain and pleasure are fundamentally one with the fact of will. This is the view of Brentano, who maintains it in Chapter VIII. of his *Psychology*, (1874): "Einheit der Grundklasse für Gefühl und Willen." According to Wundt, feeling is "the subjective completion of objective sensations and ideas," and its examination leads us back to "the original activity of apperception." The pain and the pleasure of sensation is, he writes, "the manner of reaction of apperception upon sensory stimulus." Apperception, further, we know as an "inner activity, and, "this inner activity finally is to be posited as entirely identical with the efficiency of the will." . . . ("Phys. Psych." X. Chap. Section 4.)

§ 11. (ε). Pain and pleasure are independent manifestations of mind, not to be referred to any other psychical category. According to the analysis of mind assumed by Kant, in the "Kritik of Judgment," " . . . all faculties or capacities of the soul can be reduced to three, which cannot be any further derived from one common ground: the *faculty of knowledge*, the *feeling of pleasure and pain*, and the *faculty of desire*." (Introduction III. Bernard's tr.) In like manner Lotze recognizes three primitive faculties: of presentation, of pain and pleasure, and of effort. In order to explain the facts, we must conceive that "the capacity to feel pleasure and pain inheres originally in the soul, and that the events of the stream of presentation reacting upon the nature of the soul waken them to utterance instead of engendering them out of themselves; further, whatever feelings may dominate the soul, they do not bring forth an effort,—they become only motives for a faculty of willing already present, which they find already in the soul, without ever being able to put it there had it been lacking." ("Microcosmus," Book II. Chap. II.) This belief in the essential independence of the pain and pleasure genus would seem to be shared by many contemporary psychologists, although Prof. Ward, in maintaining it, writes: "To say that feeling and attention are not presentations will seem to many an extravagant paradox." (Art. "Psychology," p. 44.)

§ 12. (b). Logical relations among themselves. As already noted, pain and pleasure are commonly conceived as species in a psychological genus, which may or may not be thought to include, beside them, the element called indifference. (Yet, cf. Ch. Richet: "Comparer le plaisir et la douleur, c'est déjà presque une hypothèse." "L'Homme et l'Intelligence," Chap. I.) They are further commonly spoken of as opposites, or, at least, contraries; but, in default of further light upon the possible meaning of these terms as applied to the ele-

ments pain and pleasure themselves, we may conclude that their application is to pains and pleasures, the states of consciousness in which these elements occur. (v. § 18.)

(3) Actual relations (a) to presentation: (a). Nature of the relation.

§ 13. It is a very common if not a predominant opinion that any element of presentation may be found in a unique way implicated with pain or pleasure, and that the latter exist only in such implication. The view of Lehmann thus expressed, "A state of consciousness consisting of pure feeling does not exist: pleasure and pain are always attached to a presentational content" ("Hauptgesetze des menschlichen Gefühlslebens." 1892, Section 18), is called by him the Kantian theory, on the ground that it was Kant who, in the "Kritik of Judgment," first "emphasized at once the opposition and the close connection between feeling and presentation." The term epi-phenomenon, which is sometimes applied to the pain and pleasure genus, implies etymologically a theory of the nature of the connection between the genus and presentation. Yet in the mind of some who use it, the word may express simply the opinion that while pain or pleasure is always the pain or pleasure of some presentation, not every presentation affords either. The hypothesis that pain and pleasure are species of presentation would lead to a denial of the unique character of their connection therewith, and to its explanation as contiguity (in sensation) and association (in idea).

(β). Presentational conditions of pain and pleasure:

§ 14. That there is any invariable connection between qualities of presentation and pain or pleasure is commonly denied. (*E. g.*, for sensation by Wundt, who concludes "that there are no qualities of sensation that are absolutely pleasant or unpleasant." . . . "Phys. Psych." Chap. X. Section 2; and for presentation in general by Lehmann on theoretic grounds: "Hauptgesetze," Section 216f.) A general principle of the dependence of sensational pain and pleasure upon the intensity of the sensation is exhibited by Wundt (same chapter, Section 1), in a diagram of the following content: as the intensity of any sensation is increased from its lowest point, at which it will neither be perceptibly painful nor perceptibly pleasurable, it will at first be increasingly and later decreasingly pleasurable, then neither pleasurable nor painful, and thereafter increasingly painful to a maximum. Yet the principle does not seem to hold universally. (H. Spencer, "Psychology," I. Section 123, "Such a taste as cod-liver oil is disagreeable, even though slight . . . sweetness is not rendered disagreeable by any degree of intensity.")

Fechner's principle of the æsthetic threshold ("Vorschule," IV.) takes into consideration not only the intensity of a presentation, but the degree of receptivity and of attention to it in the subject. "It is, to wit, a general law holding not only for feelings of pleasure and pain, but also for them, that in order that they should come to consciousness, a certain degree of that upon which the inwardly and outwardly depend is requisite." Any presentation unaccompanied by pain or pleasure, but which needs only an increase of one or other of the above elements to awaken it, he speaks of as "in the direction of pain or pleasure."

§ 15. In the theory of Herbart, pain and pleasure are functions of the interaction of presentations in the soul. ("It is the feeling of pleasure and pain that depends upon the manner in which our presentations occur in consciousness, and are excited to orderly flow." Werke Hartenstein Edn., Vol. VI. Section 108.) The theory conceives of one presentation as either aiding or hindering the rise of another into consciousness, the conditions of pleasure involving the former effect only, of pain the latter also. (See the exposition of Mr. G. F. Stout, "The Herbartian Psychology," *Mind*, No. 52, Oct., 1888, Section 31.) The notions of aid and hindrance may be taken to involve that of a particular kind of difference made in the outcome γ of circumstances a by the presence of circumstances β . If γ is lessened, retarded, prevented or rendered less likely when β occurs with a , β is called a hindrance to γ , or to a ; if γ is increased, quickened or rendered more likely, β is called an aid to it or to a . To affirm aid or hindrance it must then be possible to predict a certain outcome to given circumstances; if the basis of this prediction for the course of presentation is experience, its aid and hindrance (in which this theory finds the basis of pain and pleasure), become in general the aid and hindrance of habitudes of the soul. In order to explain the pain and pleasure implicated in simple sensation, Herbart assumes their essential though undiscoverable complexity. (They are to be considered as "arising out of presentations that cannot be specified separately, that are even perhaps from physiological reasons incapable of separate perception." Werke, V. Section 34, VI. Section 108. Cf. Kant in regard to the air-vibrations which are the basis of sensations of tone: "If we assume . . . that the mind not only perceives by sense the effect of these in exciting the organ, but also perceives by reflection the regular play of impressions . . . which I very much doubt . . ." "Kritik of Judgment," Section 14, Bernard's tr.) Nahlowsky, who takes in general the Herbartian standpoint, abandons this explanation of the

pain and pleasure implicated in simple sensation, his substitution for it of a physiological theory implying the opinion that presentational conditions cannot be given. ("Das Gefühlsleben," 2d Edn., 1884. Einleitung III.: cf. Waitz. "Grundlegung der Psychologie," 1849, p. 306.) The theory of Volkmann ("Psychologie," 3d Edn., 1884, Sections 35 and 127) is another modification of the Herbartian view. Prof. Lipps may also be claimed as a representative of the general doctrine basing pain and pleasure upon presentational aid and hindrance. ("Pleasure is everywhere the result of soul-furtherment. Pain the reflex of soul-hinderment . . .") "Grundthatsachen des Seelen lebens, 1883, p. 696.)

§ 16. A genetic theory of the presentational conditions of pain and pleasure is given by Mr. Spencer ("Psychology," Chap. IX., Data of Ethics, Chap. X.), the implication of which is that the painfulness or pleasurable of presentations is ultimately a matter of chance, the existing distribution of pain and pleasure over our experiences being the result of the co-operation of the principle of natural selection with a general law that pleasures tend to realize themselves in animal life and pains to exclude themselves therefrom. To those to whom the conception of chance as the origin of things more commends itself than definite formation, these two principles afford a means by which any observed connection between experiences advantageous to the animal which is their subject and its pleasure, and between experiences disadvantageous to it and its pain, can be explained as originally fortuitous. For such a connection as the first leaves us free to assume that another organic type has existed in the past, differing from that of the present only in the fact that this experience was not pleasurable to it; since, if such a type had existed, the advantageous experience not being pleasurable would have been less realized in its life, and it would have been therefore at a disadvantage as compared with the type we know. That is, had the hypothetical type existed, it would not now exist. In like manner an observed connection between an experience detrimental to an animal and its pain, leaves us free to assume that a type has existed in the past, like it in all respects, excepting in not finding this experience painful. For, being painful, the detrimental experience would have been less realized in the life of the existing type than in that of the other, which would therefore have succumbed to it. That is, had the hypothetical type existed, it would not now exist. The hypothesis of the existence of these suppressed forms is the hypothesis that the connection between pleasure and pain and presentations is to a certain extent a matter of chance. (Cf. C. S. Peirce: "Illus-

trations of the Logic of Science." *Popular Science Monthly*, 1887-88, fifth paper. A chance world contains "every combination involving either the positive or the negative of every character.") To use the customary term, this hypothesis regards the existing distribution of pain and pleasure over our experiences as in some measure an evolution. On the other hand, evidence against the fortuitous nature of the connection between pain and pleasure and presentation is afforded by any cases in which pains are advantageous and pleasures detrimental to the organism experiencing them. These instances offer evidence that to a certain extent pains and pleasures are not matters of chance, have not been evolved. (On limitations of the theory, cf. Schneider, *Freud und Leid des Menschengeschlechts*, 1883, Chap. I.) A question fundamental to the hypothesis is whether the time and space through and over which the various forms of life may be supposed to have existed on the globe is sufficiently great to make possible the production by chance of such relationship as exists between pain and bodily detriment and pleasure and bodily advantage. Further, no evolutionary theory will commend itself to any mind incapable of resting in the conception of chance as the origin of things: (Cf. Lotze: Art. "Leben. Lebens Kraft." Wagner's *Hd. w. b.* 1843, who finds "that absolute law has just as much right to be conceived eternal as absolute lawlessness.")

(b). Actual relations to will:

§ 17. The proposition that the will is a function of pain and pleasure seems to have appeared axiomatic to Schopenhauer: "Was den Willen bewegt ist allein Wohl und Wehe überhaupt und im weitesten Sinne des Wortes genommen." ("Grundlage der Moral," Section 16: Upon this opinion cf. the remark of Amiel: "Pour Schopenhauer le caractère s'identifie avec le naturel, comme la volonté avec la passion. En un mot, il simplifie trop" "Journal Intime," Vol. II. p. 68.) Many psychologists at present assume the same principle. Mr. Spencer writes: "If we substitute for pleasure the equivalent phrase—a feeling which we seek to bring into consciousness and retain there," etc., etc. ("Psychology," I. Section 125.) Lipps holds that "only that theory is justifiable according to which effort and that which lies at the basis of pleasure and pain, are related as different sides of the same psychic fact." ("Bemerkungen z. Theorie der Gefühle." *Vierteljahrsschrift für wissenschaftliche Philosophie*, 1889, p. 176.) Mr. Hodgson writes: "All choice is a balance and determination between pleasures." ("Theory of Practice," 1870, Vol. I. Section 56.) On the other hand, according to Meynert, the simplest theory

will always be that "in the most complicated, enigmatic and incomprehensible actions of man, the guiding motive is the avoidance of the greater pain." ("Psychiatrie," 1st part, 1884, p. 163.) Prof. James, according to whom "effort of attention" is "the essential phenomenon of will," concludes that "believers in the pleasure and pain theory must, if they are candid, make large exceptions in the application of their creed. . . . Pleasure and pain are motives of only part of our activity." ("Psychology," Chap. XXVI.) According to Lotze, most human behavior is impulsive in nature, involving no exercise of volition. "We are sure of having to do with an act of will only in a case in which the impulses that urge to an action are perceived in clear consciousness, and yet the decision whether they shall be yielded to or not is in abeyance, and falls, not to the power itself of those urgent motives, but to the free determining choice of the spirit independent of them." ("Microcosmus," Book II. Chap. V.) A delicate observation of impulsive as distinguished from volitional avoidance of pain is given in the "Journal des Goncourt" (I. p. 314): "C'est étonnant le matin, quand il faut passer du sommeil à une certitude douloureuse, comme machinalement la pensée retourne au sommeil ou elle se réfugie et semble se pelotonner, pour ainsi dire, dans ses bras." A question fundamental in the subject is that as to the sense in which the will can be said to be a function at all: (Cf. the distinction of Kant between empiric and intelligible character. In "The Dilemma of Determinism," *Unitarian Review*, Sept., 1884, Prof. James gives grounds for a decision in favor of volitional freedom).

(c). Actual relations among themselves :

§ 18. Pain and pleasure are commonly spoken of as opposites or at least contraries. Doubtless it is intended by the use of the latter term to express a generalization akin to the following: If pain and pleasure are together in the mind, the presentation which, upon analysis of the mental state in question, appears as that which we take pleasure in, is always other than the presentation which gives us pain. (Yet this would have been disputed by Herbart, according to Mr. Stout, who gives the conditions which, if simultaneously present, would cause a presentation to be at once pleasurable and painful. *Mind*, No. 52, October, 1888, p. 490.) The source of the predicate of opposition is apparently the common complication of pain and pleasure with bodily movements in opposite directions (flight and approach, contraction and expansion). The idea leads readily, though without warrant, to a theory of the psychical

nature of the pain and pleasure genus, viz., that it constitutes a one dimensional manifold containing, besides different intensities of pain and pleasure, an intermediate element called the "feeling of indifference." (While sensational pain and pleasure are spoken of by Wundt as opposites which "pass over into each other through an indifference point," he interprets the latter as indicating not a form of feeling, but its absence. "Phys. Psych." Chap. X.)

§ 19. The intricate subject of the coalescence of pains and pleasures is studied at length by Lehmann, ("Hauptgesetze," Sections 315-336.) For complex states where both enter, it appears unquestionable "that they do not cancel one another as positive and negative quantities." (Section 336.) Among principles of the combination of pain and pleasure in experience formulated by Fechner, that of *Æsthetic Aid* is to the effect that the amount of pleasure produced by the union of several pleasurable elements is greater than the sum of the pleasures they individually give ("Vorschule," Vol. I. p. 50); that of *Blunting* is to the effect that the pleasurable-ness (and to a certain extent also the painfulness) of a presentation is lessened through the continued or frequent experience of it ("Vorschule," Vol. II. p. 240). According to Lehmann, the phenomenon of Blunting is not a real decrease of pleasurable-ness or painfulness, but the result either of a diminution in strength of the presentation itself, or (1) in the case of a pleasure of a gradual accretion of disagreeable elements; (2) in the case of a pain the production of a need of the (originally) painful presentation. ("Hauptgesetze," Sections 248, 253, 254.) The principle of Contrast (of which those of Sequence and Compensation are consequences) Fechner states as follows: contrast occurring when elements of experience are so given in consciousness that their difference is perceived, one can say in general, "the pleasurable gives the more pleasure the more it comes into contrast with the disagreeable, or the less pleasurable; a corresponding principle holding for the disagreeable." ("Vorschule," Vol. II. p. 232.)

§ 20. The possibility of the application of measurement to pain and pleasure, conceiving this as finding the number of times a given unit of either feeling is contained in a certain pain or a certain pleasure, may be denied on the ground that it is impossible to figure to one's self a process of the subtraction of one pain or one pleasure from another. (Cf. the remark of Professor Stumpf as to the impossibility of analyzing a degree of intensity into a lower degree and a remainder: "As far as I can see it is impossible to imagine separately the amount of intensity which must be added to the lower degree to give the higher." "Tonpsychologie," I.

1883, p. 121.) What meaning quantitative judgments, judgments of greater or less, may have in the domain of pain or pleasure, and to what extent they are possible as between pains, pleasures, or a pain and a pleasure, are questions of importance and difficulty. Such exclamations as that of Heine (Lazarus 37),

"Doch wer von Wonne trunken ist
Der rechnet nicht nach eitel Stunden,
Wo Seligkeit ist Ewigkeit."

or Massimo d'Azeglio ("I miei Ricordi," p. 37): "There are moments in life that would compensate for an eternity of torments"—are not to be regarded as pure rhetoric. Lehmann finds all more delicate estimates of the intensity of pleasures and pains impossible ("Hauptgesetze," Section 332).

2. *Psychophysical:*

§ 21. The propriety of postulating in the case of pain and pleasure a correlation with phenomena of body like that generally assumed at present for presentation, is open to question from the standpoint of those who recognize a fundamental distinction between these two forms of mental fact. Professor Lipps denies "that feelings of pleasure and pain spring immediately from relations and respects of the nervous stimuli to bodily well-being and ill-being," although he does not deny "that such relations and respects exist and run parallel to the corresponding psychic fact." ("Grundthat-sachen des Seelenlebens," 1883, p. 199.) The opinion here referred to, according to which the pleasure of any soul is the sign of some event in the body it inhabits which is favorable to the life of that body, and pain the sign of bodily detriment, is a very general belief, both popular and scientific. In the presence of human suffering the impression is instinctive that "something ought to be done;" we conceive that something is happening in the bodily economy that threatens its integrity. The instinctive impression in regard to pleasures is, on the other hand, that they "do one good," in the sense of contributing to bodily well-being. Professor Bain writes regarding the bodily accompaniments of pain and pleasure: "A very considerable number of the facts may be brought under the following principle, namely, that states of Pleasure are connected with an increase and states of Pain with an abatement of some or all of the vital functions." ("Senses and Intellect," division on Movement, Sense and Instinct, Chap. IV. Section 18.) Lotze extends the application of the ideas of hindrance and furtherance beyond the

bodily life: while the "agreeable in sensation" is that which is adapted to the capacities of the sensitive mechanism, the "pleasant in idea" is that which is adapted to the functional conditions of the psychic mechanism; and æsthetic approval or disapproval is conditioned upon a furtherance or hindrance not personal, but of the universal spirit within us. ("Geschichte der Ästhetik in Deutschland," p. 262. "Grundzüge der Psychologie," Sections 47-49; cf. "Microcosmus," Book II. Chap. V. Similar principles are assumed by Lehmann, "Hauptgesetze," Section 202.) Yet these formulæ are the unequivocal expression of only very general ideas, however important. By Professor Bain's words, pain and pleasure are connected with the degree of certain forms of bodily functioning called vital, a certain intensity being perhaps assumed for each, above which pleasure and below which pain is the result to the accompanying consciousness; or, if no such dividing degree is posited, it is the intensification of function that is connected with pleasure, and its weakening with pain. Lotze is, perhaps, to be understood as connecting pain and pleasure with any functions of any structure, psychic or bodily (when correlated at all with mind), which fulfill the qualitative condition of adaptation to the powers of the structure. The criterion of adaptation is not stated; perhaps that of the longevity or vigor of the organism may have been in Lotze's mind; or it may be that he assumed no independent criterion, the doctrine he intended to express being that for every sentient being there exists a certain normal life history (physical, perhaps, as well as psychical), the perfect or mutilated realization of which is respectively pleasure and pain to it.

§ 22. It is frequently sought to express the bodily conditions of pain and pleasure in quantitative terms, either in connection with a theory of aid and hindrance to life, or independently of such a hypothesis. Professor Bain suggests that some principle of stimulation connecting pleasure with a consumption of nervous force which, though considerable, is not in excess of nutrition, may eventually be found completely to account for the facts. ("Senses and Intellect," *ibid.* Section 22.) Lotze proposes a similar theory, in which pain is conceived as the accompaniment of a consumption of nerve force in excess of the ordinary reparative powers. ("Medizinische Psychologie," Section 23.) These two principles are included in the theory of Lehmann ("Hauptgesetze," Section 208), who regards his hypothesis as, in the main, the same as that proposed by Grant Allen. ("Physiological Æsthetics," 1877, Chaps. II. and III.; criticised by E. Gurney, "Power of Sound," Appendix C.) Another hypothesis, stating the bodily

conditions of pain and pleasure in terms of energy received and given out by the organ concerned, has recently been proposed by Mr. H. R. Marshall, "Pleasure is produced by the use of stored force in the organ determining the content; and Pain is determined by the reception of a stimulus to which the organ is incapable of reacting completely." (*Mind*, Nos. 63 and 64, July and October, 1891. The theory is discussed in Professor Bain's article on "Pleasure and Pain," *Mind*, N. S. No. 2, April, 1892. Criticisms of other psychophysical hypotheses on this subject are contained in Mr. Marshall's article, and in those of Cesca: "Die Lehre von der Nature der Gefühle," *Vierteljahrsschrift für Wissenschaftliche Philosophie*, X. 2, 1886, and of Külpe, "Zur theorie der Sinnlichen Gefühle," in the same periodical, XI. 4, and XII. 1, 1887-88.)

§ 23. Fechner inclines to the opinion that only quantitative relations of the psychic can rightly be made to depend on quantitative relations of the correlated physical process (a like remark is made by Volkmann, "Psych.," Section 35, Note), and that pain and pleasure, as qualitative determinations, are to be conceived as depending on a form or form-relation of this process. He proposes, as a possible view, a principle of psychophysics stability. A condition of movement being stable "which involves the conditions of its own return," he suggests that "the actual relation subsisting in the realm of consciousness between effort, pleasure and pain, might be of such a kind that, beyond a certain degree of approximation to the stable condition, pleasure should be the result and, beyond a certain degree of withdrawal from the stable condition, pain should be the result, while between the two there should exist a condition of indifference of a certain breadth." ("Vorschule," XLIII. "Einige Ideen zur Schöpfungs und Entwicklungsgeschichte der Organismen," 1873, XI. Zusatz.)

3. *Philosophical:*

§ 24. Pleasure and value. That pleasure is one of the forms of value, is generally assumed: ("Nothing affirms itself to be valuable so unconditionally and so immediately as pleasure." Lotze: "Grundzüge der Ästhetik," Section 13); that it is the only form, is not an uncommon doctrine. The contention of Pessimism, as that term is commonly understood, is that since pain over-balances pleasure in experience, life is of no value (E. Von Hartmann: *Philosophische Monatshefte*, Vol. 19, 1883, "In what sense was Kant a pessimist?" p. 464; "i. e., whether he assumed a negative balance of pleasure in the totality of all existence . . ."). The literal implication

of this proposition, that pleasure is of no value unless it exist in greater amount than pain, is doubtless not the sense intended. This may, perhaps, be formulated in (1) a statement about the relation of pain and pleasure to volition, viz., sentient beings make no choices which they think are to lead to greater amounts of pain than of pleasure; and (2) a definition of value as that which is willed. Real life, according to this opinion, would never be chosen; the content of volition is always an impossible ideal of life. (Second Part of K. Henry IV. Act 3, Scene 1.) Independently of the question as to the commensurability of pain and pleasure, the truth of both statements in the analysis just given is to be disputed. In the doctrine of Utilitarianism, pleasure is again assumed as the only value ("Pleasure and freedom from Pain are the only things desirable as ends," J. S. Mill: "Utilitarianism," Chap. II.); righteousness, it is true, has the appearance of intrinsic worth, yet analysis shows it to be a function of pleasure (virtue is a good originally indifferent: "There was no original desire of it, or motive to it, save its conduciveness to Pleasure and especially to protection from Pain," *ibid.* Chap. IV.). By the aid of the principle, lately brought into question, of the inheritance of acquired characters, Darwin and Mr. Spencer have sought to show how an illusion in regard to the intrinsic value of virtue might have arisen. The sense of obligation is a precipitate of pains and pleasures from waters of oblivion. (Darwin: "Descent of Man," Chap. IV.)

§ 25. Defining the valuable as that which ought to exist, the fundamental doctrine of the threefold nature of mind heretofore posited, suggests in opposition to these tendencies of opinion that its determination as pleasure is only one of three forms which value may assume; that righteousness is the determination of value in the volitional aspect of the soul, and knowledge value in presentation. Intrinsic worth is as commonly denied to knowledge as to virtue. The spirit that cannot conceive of it as an end in itself, is illustrated in the argument with which Omar justified the burning of the Alexandrian library. The query of the Turkish *cadi*, quoted from Layard by Professor James ("Psychology," II. 641), "Will much knowledge create thee a double belly, or wilt thou enter Paradise with thine eyes?" implies that knowledge has value only as a means of pleasure or a help to virtue. "So that knowledge increases on us, if that be a good," remarks a personage in Wm. Morris's *Utopia* ("News from Nowhere," p. 46). (On the general doctrine of value, cf. Professor Royce's "Spirit of Modern Philosophy," 1892, Lect. XII., "The world of description and the world of appreciation.")

II.—PAIN AND PLEASURE IN PRESENTATION.

1. *Normal Consciousness:*

(1) Sensational:

§ 26. (a). Physical pain. A working definition might be—markedly disagreeable sensation localized in the body. (Cf. Kroner: "Gemeingefühl und Sinnliches Gefühl," *Vierteljahrsschrift für Wissenschaftliche Philosophie*, XI. 2, 1887, and "Das Körperliche Gefühl," 1887, p. 167, where pain is defined as the feeling-tone of the skin and muscle sense.) This localization may be general or special, vague or precise. Pains localized throughout the whole body, or one cannot say where, in it, are called by Beaunis ("Sensations Internes," 1889), *malaises* or uneasinesses, *e. g.*, the feeling of languor (inability), exhaustion (used up ability), restlessness (insomnia or waking "nervousness") or feverishness. In vertigo and præcordial oppression there is an indistinct special localization of the pain. Physical pain located definitely may have the tinge of a special bodily sense (painful touches, temperatures), and often assumes the form in time or space of mechanical interferences with the bodily economy (cutting, tearing, darting, etc., pains).

§ 27. On the question as to the physiological basis of physical pain there is much difference of opinion. (Kroner: *Körp. Gefühl*, Chap. XXV.) (a). Theory of special nerves and a special centre for pain. Richet ("Récherches sur la Sensibilité," Chap. V.) assumes a centre for pain, and Brückeseveral ("Physiologie," 1884, p. 266). The discovery of certain points on the skin, insensitive and others hypersensitive to certain painful forms of stimulation (researches of Blix, Goldscheider and Donaldson, described by the latter in *Mind*, for July and October, 1885), has given support to the hypothesis of special nerves of pain. (Lehmann contra; "Hauptgesetze," Section 48.) The facts of analgesia (loss of sensibility to pain, with preservation of other sensibility) point the same way (yet the proof is not positive, according to Wundt, who offers another explanation, "Phys. Psych." Chap. IV. Section 3; cf. Lehmann: "Hauptgesetze," Section 58f.). This condition may be produced by drugs (*e. g.*, cocaine), or by partial section of the spinal cord (Schiff's experiment), or by disease, *e. g.*, progressive muscular atrophy. Another item of evidence for special avenues of pain-conduction is the fact that in a painful contact, the pain is not felt with, but after the sensations of touch involved (according to Lehmann, Section 52f., because it is not the pain of that touch, but of certain massive sensation following the touch). Vulpian finds the hypothesis of special nerves of pain unten-

able (Deschambe's Dict. des Sciences Médicales Art. Phys. de la Moelle Epinière, p. 420; cf. Mantegazza: "Fisiologia del Dolore," 1880, Chap. X.); and the evidence of their existence is at least insufficient according to Beaunis ("Sensations Internes," p. 210f.; cf. Külpe: "Zur theorie der Sinnlichen Gefühle," *Vierteljahrsschrift für Wiss. Phil.* XI. 4, 1887, Chap. II. Section 2). Further, the close relation between pain and pleasure would seem to make it necessary to assume in addition to pain nerves a system of nerves for pleasure. (β). Theory that the physiological correlate of physical pain is intensity of the nervous irritation. (Richet: "L'Homme et l'Intelligence," I.; Wundt: "Phys. Psych." Chap. IX. Section 1 ad finem postulates also irradiation.) The existence of faint pains (*e. g.*, neuralgias, qualmishness) seems to make it necessary to posit instead of simple intensity some factor involved in intense irritation, but which may occur without it; *e. g.*, (perhaps widespread) interference with cortical functioning: Meynert ("Psychiatrie," 1884, p. 176) speaks of pain as a *Hemmungs-gefühl*, basing it on inhibitions not only of nervous irradiation from the painful stimulus, but of other performances of the cortex. The facts of the separation of pain and other sensation in analgesia and anæsthesia may in good part be explained by the supposition (γ) that extensity of irritation is essential to its production. (Cf. E. H. Weber in Wagner's *H. w. b. der Physiologie*, and Groninger: "Ueber den Shock," 1885.)

§ 28. The physiological effect of pain is in general a depression of the functions. Mantegazza (Fis. del Dolore) found a diminished pulse rate, temperature, nutrition and secretion (excepting tears and sweat). Darwin's discussion of the manifestations of pain is given in his "Expression of the Emotions," Chap. VI. On a certain pleasure in tears cf. the remark of Leopardi, "Epistolario," I. p. 292, "Could not one come from America only to enjoy the pleasure of tears for the space of two minutes?" Lehmann's experiments upon the manifestation of feeling ("Hauptgesetze," Section 95f.) have, in part, a reference to pain.

§ 29. (b). Bodily cravings. These are forms of appetency in which the object is sensation localized bodily. When the character of appetency (which may be one of the ultimate forms of presentation) is present, a certain more or less disagreeable state of consciousness exists, which can normally be put an end to with comparative permanence and completeness only when a certain other supervenes. The former experience is then called a craving for the latter; the latter the satisfaction of the first. Whatever fact of the pain and pleasure genus may be implicated with the satisfaction

in independent occurrence, in sequence upon a craving for it, it tends to be a pleasure. Cravings are connected with many bodily functions; *e. g.*, to breathe (feeling of suffocation); to move the muscles (for exercise; morbid forms are tics and chorea); to eat (hunger); to drink (thirst); to copulate (desire); to gape, sneeze, cough, chew, swallow, defecate, urinate, vomit (nausea?), wink, rest, sleep (drowsiness). A discussion of some of the more important is given by Beaunis ("Sens. Internes," Chap. II. Besoins). It is an interesting question, according to him, whether the activity of unstriped (organic) muscles may not become an object of craving, *e. g.*, in hunger. (Reference to Sir James Paget's "Clinical Lectures on Surgery:" chapter on "Stammering with other organs than those of speech.") The theory of Beaunis, that craving for muscular activity is due to a high degree of tension in the motor centres, would seem to correlate a nervous state and not a nervous activity with consciousness.

§ 30. With the craving of sex, the special pleasurable sensation which constitutes gratification is complicated; although the craving is no exception to the rule according to which appetency is in the direction of pain. Lust, which in earlier usage meant pleasure in general, has, in later English, come to signify the venereal desire principally. Comparable with sex are the two cravings involved in sneezing and itching: the satisfactions of all three are progressively intensifying and more or less abruptly closing courses of sensation, the quality of which does not seem altogether different. Sneezing and sex each leads up to a motor discharge; the satisfaction quality in each is complicated with the craving: they are further complicated together in experience, and sex, at least, with tickling, if not itching. All three cravings have their disagreeable side: this shows itself in massive form in the familiar experience of being balked of a sneeze which has gathered good headway. Snuff-taking may be conceived as an exploitation of the pleasure of the sneeze-craving, as lewd dances (*e. g.*, in the Orient) are of that of the sex-craving. The experience of satisfying an itching may (*e. g.*, in the scratching of an eczema) attain the volume and intensity of coitus. These similarities suggest a similarity of physical basis for the three forms of craving. The writer offers the suggestion that the satisfactions in all these cases are the result of the massage of muscles of organic life (unstriped muscles): a molecular form of the same experience we have in a motor way when voluntary (striped) muscles are stretched actively or by massage. The cravings might, then, be viewed as results of fatigues of unstriped muscles. The extraordinarily widespread connections of the sex-craving in the psychic

life give rise to the amorous passion. (On the part taken by imagination in the experience of being tickled, cf. Kroner: "Körp. Gefühl," p. 163f.) The cravings spoken of as "for repose" and "for sleep" appear to be inaccurately named: they are rather cravings for certain sensational content, normally complicated with the experience of the abandonment of effort, and with the state of drowsiness.

§ 31. Assuming that appetency (blind=craving: intelligent=desire, expectation) is essentially disagreeable and its satisfaction essentially agreeable, the following hypothesis may be offered in explanation of the fact. The physiological basis of a state of appetency consists of an anterior stage of the process whose psychic correlate is the experience craved, performed with another psychophysis process than the posterior stage as its successor. The pain of appetency is thus explained as the pain of thwarted psychophysis functioning. A process at last carried through after repeated inceptions, is performed with exceptional intensity, and this livelier success of psychophysis functioning is the ground of the pleasure of satisfaction. According to this hypothesis appetency is not exclusively a motor phenomenon (as Mr. Spencer seems to imply, "Psychology," I. Section 213; cf. Dumas: "L'Association des Idées dans les Passions," *Revue Philosophique*, No. 185, 1891); but one whose physiological correlate may be any form whatever of psychophysis happening; nor is this correlate a "nascence" of the correlate of the object of desire (cf. Mr. Spencer), but the occurrence of a preceding portion of it before another form of excitement than the succeeding portion. (Cf. Prof. Sully on Desire: "Pessimism," 1877.)

§ 32. (c). Lower senses. Neither the psychological nor psychophysical study of the lower sensations is as yet far advanced. Whether the pain of the voluntary muscles known as fatigue is caused by mechanical stimulus, or chemically by products of decomposition in the blood, is yet undecided. (Hermann: "Handbuch der Physiologie," Vol. III. Funke: "Tastsinn und Gemeingefühle.") A connection between loss of muscular power and a gloomy view of life is posited by Féré. ("Pessimisme et Impuissance," *Revue Philosophique*, July, 1886.) The experiments of Prof. Haycraft on touch indicate that the disagreeableness of the sensation of roughness is due to its pulsating intensity. ("An experimental inquiry into the nature of the objective cause of sensation," *Brain*, July, 1885.) In smooth touches the sensation is constant. These latter may be vividly pleasurable. (The hand in mercury. Joy of the Marquesan lady over the touch of plush, described in R. L. Stevenson's letters from the South Seas.) Is, then, the sensation of contact

in itself complicated with strong pleasure, or is this effect in a measure associative? A possible explanation of the strong pain often accompanying a light touch (*e. g.*, a fly walking over the face), might ascribe it to the fact that this form of irritation involves an intense state of suspense in the nervous tracts subserving the sensation of contact. The light touch awakens anticipations of contacts of ordinary volume and intensity, which left without satisfaction sum themselves to a painful height. (On the question of the irradiation of weak stimuli, cf. Funke in "Hermann's Handbuch.") In the cases of heat and cold the question has interest as to the extent in which the pain and pleasure involved are complicated with the sensations themselves, or are the result of the effect on the system of the stimuli concerned, or are matter of association. Of the physiological conditions of the various tastes of substances, we are as yet entirely ignorant. (Cf. Vintschgau: "Hermann's Handbuch," Vol. III.) The question as to how many there are is not settled. They appear to exhibit the working of contrast, and a certain rivalry when simultaneous. The study of sensation of smell is no further advanced. Yet in sweet and bitter we have two representatives of the agreeable and disagreeable pronounced enough to give their names (in many languages) to many other forms of pain and pleasure; and in smell the agreeable and disagreeable is no less marked. The arts of the kitchen and bar-room have reached a high degree of complexity, and offer rich material, as yet hardly touched, for the study of refinements and harmonies of flavors and odors: (Cf. Brillat Savarin: "Physiologie du Gout," 1825. The existence of a scale of odors is assumed by Dr. S. Piesse in his "Art of Perfumery," 1880); *e. g.*, what is the part played by association, and what is intrinsic effect in the satisfactions and repulsions of the gourmand and gourmet: what is the basis of the habitude of serving potatoes with fish: how does cheese enhance the taste of wine: are these harmonies a positive addition to the charm of the components, or an absence only of interference between them?

§ 33. (d). Hearing. Of the two kinds of sensations of hearing, noise is in general not intrinsically pleasant, while tone is markedly agreeable in itself. This difference is naturally referred to the fact that the stimulus in the case of tone is regularly periodic vibration of the air. (Cf. Leibnitz: the pleasurable of tone proceeds from the "unsichtbare Ordnung" of the air vibration. Op. Phil. Ed. Erdmann, LXXVIII. Wundt, "Phys. Psych." I. Chap. VII. Section 4, finds it possible to assert "that the sensation of tone depends upon a regularly periodic course of excitement in the fibres of the auditory nerve.") The combination of two tones of differ-

ent pitch, either simultaneous or successive, is in almost all cases disagreeable, the exceptions being in general cases where the combination has positive charm. In these combinations, called consonant, the vibration ratios of the component tones can be expressed by the use of only the first few integers. (This remarkable connection between consonance and the first few integers formed the basis of the Greek conception of the harmony of the spheres. Ancient Chinese speculation upon it quoted by Père Amiot: "Memories concernant l'histoire, etc., etc., des Chinois," Vol. VI. Paris, 1780.)

§ 34. Euler suggested that the perception of order (in the combination of series of impulses) is the basis of the charm of consonant intervals (vibration ratios). ("Tentamen novæ theoriæ Musicæ," 1739.) A theory applying a like principle to a hypothetical physiological process concerned in consonance, is that of Preyer ("Akustische Untersuchungen," 1879, III., "Zur Theorie der Consonanz"), according to whom the order is perceived in the proportions of the segments marked out on the basilar membrane of the inner ear by the fibres concerned in hearing an interval. A view which bases the pleasure of consonance not on any grasp by the mind of proportions exhibited in the physiological event concerned in the perception of interval, but on a physiological result of these proportions (in this case proportions of duration), is taken by Prof. Lipps, according to whom auditory periodicities of simple ratio help, of complicated ratio hinder one another ("Psychologische Studien," 1885, II. "Das Wesen der Musitalischen Harmonie und Disharmonie." Of the suggestion of Lotze: "Medicinische Psychologie," Section 22).

§ 35. In the theory of Helmholtz the disagreeableness of dissonant intervals is ascribed to the presence in them of rapid pulsations of sound (called beats), while the agreeableness of consonant intervals is left to be accounted for by the intrinsic charm of the component tones. ("Die Lehre von den Tonempfindungen," IV. 2d Edition, 10th and 11th Abschnitte, p. 320 and 335.) The pain of rapid beats is, according to Helmholtz, an instance of the general principle that sensations of quickly pulsating intensity are disagreeable. Of this fact his explanation is that "there is produced thereby a much more intense and more unpleasant excitement of the organ than by a tone that persists equably." ("Tonempfindung," 8th Abschnitt, p. 283.) E. Gurney finds this explanation unsatisfactory, and writes: "We seem thus driven to assume the existence of some other kind of nervous disturbance, connected specially with interruptions supervening on a mode of motion which has been sufficiently established to

become, so to speak, familiar." ("Power of Sound," p. 557. Cf. v. Hensen "Hermann's Handbuch," Vol. III. Or it may be claimed that irritation of a pulsating intensity is an exceptional form of sensory process, running counter to the habits of the sensorium.) The conclusions of Helmholtz on dissonance and consonance have since been called in question. A. von Oettingen presented two arguments against the theory of beats: (1) It does not account for the unchanged harmonic character of an interval in notes of different quality; (2) It does not account for the positive charm of harmony. ("Harmonie System in dualer Entwicklung," Leipzig, 1866, p. 30.) Preyer notes (3) that the theory of beats does not account for consonance and dissonance in successive notes, except through the aid of a complex hypothesis as to the part played by memory in judgments of tone. ("Akustische Untersuchungen," 1879, p. 60. E. Mach, "Beiträge zur analyse der Empfindungen," 1886, p. 119, recognizes the force of these criticisms. C. Stumpf writes: "The theory must, indeed, according to my conviction, be given up." "Vierteljahrsschrift für Musikwissenschaft," 1885, p. 345.)

§ 36. In explanation of the phenomena of harmony, v. Oettingen suggested two principles, one of which has found much acceptance since; viz., the reason why the intervals whose ratios are expressible by the first few integers are pleasant, is that it is these intervals that are exemplified between the partial tones of the standard musical note. The question suggested by this hypothesis: viz., why is this note the standard? has various possible answers: (1) Perhaps because among all possible notes this alone is free of beats (between partials or difference tones): (2) Perhaps because no one form of note is so common: (3) Perhaps because this is the note of the human voice. (This latter is the suggestion of O. Hostinsky. "Die Lehre von den Musikalischen Klängen," 1879, p. 55. "The musical sense has its foundation in an adaptation of the organ of hearing to the vocal organs.")

§ 37. (e). Sight. (a). Color. Notwithstanding the study that has been given questions regarding the pain and pleasure phenomena of the sense of sight, they are still in large measure without well defined and well established solutions. Sensations from light waves are in general pleasant, and it is natural to assign as the cause of this fact the uniformity of the physical process in which they originate. The inferior charm of the sensation of green has been attributed to the unusual intensity with which the light waves concerned in its production attack the visual mechanism: (Professor O. N. Rood: "Student's Text-book of Color," 1890, p. 295.) The same fact has been explained on evolu-

tionary principles: red and yellow are comparatively rare in nature, and are, moreover, the colors of fruits; hence the eye is not only fresh to, but strengthened for their perception: (Grant Allen: "The Color Sense," 1879, Chap. XII.) Associations with colors; their emotional effect; warm and cold color: (Goethe: "Zur Farbenlehre." Didaktische Theil, sixth Abtheilung. Wundt: "Phys. Psych." I. Chap. X. Section 2. Fechner: "Vorschule," I. p. 100f.; also direct effects of color, II. 212f.) Effects of combination of color and of light and shade; enrichment and impoverishment of colors by contrast: (Chevreul: "Harmony and Contrast of Colors," 1839. Brücke and Helmholtz: "Principes Scientifiques des Beaux Arts," 1881, section on harmony of colors. A. Kirschmann: "Die Physiologische-ästhetische Bedeutung des Licht und Farben Contrastes." *Philosophische Studien*, 1891, p. 382f.) Contribution of intellectual elements: (Sully: "Harmony of Colors," *Mind*, XIV.) Theory of optical balance: this is not necessarily æsthetic balance; theories relating color to tone lack a sufficient basis of fact: (Rood: "Student's Text-book," Chap. XVII.) (On the general subject, cf. Helmholtz's "Physiologische Optik;" Brücke: "Physiologie der Farben," 1866; A. Lehmann: "Farvernes Elementare Æsthetik," 1884.)

§ 38. (β). Form. (Cf. Lipps: "Æsthetische Faktoren der Raumanschauung," 1891.) The æsthetic superiority of curved outlines has been ascribed to feelings of the movements of the eye involved in following them; on the principle that unless executed with intenser degrees of effort such movements are agreeable. (Wundt: "Phys. Psych." Chap. XIV. Section 2. Horwicz: "Psych. Analysen," Vol. II. p. 146. G. Allen: "Phys. Æsthetics," VII. Section 7. Lotze contra; these feelings too insignificant. "Gesch. der Æsthetik in Deutschland," p. 310. On the principle of least energy in æsthetics, cf. Fechner: "Vorschule," XLIII., and H. Jäger: "Das Princip des Kleinsten Kraftmasses in der Æsthetik," *Vierteljahrsschrift für Wiss. Phil.* 1881. p. 415.) The agreeableness of curves may be explained also on evolutionary principles, and further by perception of relations of direction and its change, and by association: (Sully: "Pleasures of Visual Form," *Mind*, 1880, p. 180.) A figure is symmetrical when it can be divided into halves, of which one can be conceived as the reflection of the other in a mirror; the symmetry being called vertical (arch) or horizontal (landscape and its reflection in water) according to the position of the supposed mirror. Our pleasure in symmetry and preference of the vertical to the horizontal form has been explained (Mach: "Die Symmetrie," 1872) by the resemblance in the

former case only between the experiences of observing the two halves of the figure, due to the vertical symmetry of the organs of perception, the assumption being that in general repetition of such experience is pleasurable. Theory of the habituation of the eye to vertical symmetry through observation of animals and plants: (Grant Allen: "Origin of Sense of Symmetry," *Mind*, XV., and "Phys. Æsthetics," VII. Section 8; also Professor Sully in article just cited.)

§ 39. (*r*). Proportion. Preference in proportion is sometimes explained on a principle of the easy grasp of ratio (e. g., Wundt: "Phys. Psych." Chap. XIV. Section 2). Zeising (in "Neue Lehre von den Proportionen des menschlichen Körpers," 1854, and other works) claimed that the Golden Section is the normal æsthetic proportion, and sought illustrations of it in the human body and in architecture. The line A C is divided in the Golden Section by the point B, when $AB : BC :: BC : AC$. Fechner finds Zeising's principle a real discovery in æsthetics, though not of the importance attributed to it by its author. ("Vorschule," I. p. 184f.; cf. Pfeifer: "Der goldene Schnitt," 1885.) The elements of the proportion being incommensurable the theory of an easy grasp of ratio does not seem to apply; but since it involves two applications of the same ratio, the explanation above quoted for symmetry (a repetition of visual experience) may be conceived to account for the charm of the Golden Section also.

2. *Ideal*:

§ 40. A principle according to which the success of psychic functioning is a cause of pleasure and its hindrance a cause of pain ("psychic euphoria" and its opposite, Meynert: "Psychiatrie," I. p. 180), serves to account for many of the more important phenomena of pain and pleasure in the sphere of idea. (In support of the Herbartian hypothesis basing feeling on the interaction of presentations, acute and detailed analyses of pain and pleasure phenomena in this field are given by Waitz: "Lehrbuch der Psychologie," 1849, III. Abschnitt. Nahlowsky: "Das Gefühlsleben," Sections 8-13, Section 16. Volkman: "Psychologie," Section 117f.; cf. also on this subject, Horwicz: "Psychologische Analysen," II. Pt. II. p. 181f. Mr. Hodgson: "Theory of Practice," Section 19, Section 55. Professor James: "Sentiment of Rationality," *Mind*, 1879, p. 317, and "Psychology," Chaps. IX. and XXVIII. Fechner: "Vorschule," VII. Lehmann: "Hauptgesetze," Section 297f.) Familiar manifestations of the pain and pleasure genus in ideation are the strong discomfort of imperfect recollection

(when empty images of idea make us "ronger et despiter après leur queste," Montaigne: "Liv." III. Chap. V.) and the glow of pleasure accompanying its final success. An allied discomfort is that of being prevented from following up some thought from which avenues of suggestion seem to be open in the mind. The regret we often feel at being interrupted in a dream, the content of which may be indifferent, seems to be of this kind.

§ 41. The vague feeling of discomfort with which we apprehend an unsound argument may be interpreted as a reflection of thwarted habitudes of intellection whose cognitive content belongs to the penumbra of consciousness. (Cf. Meynert: "Psychiatrie," I. p. 274.) The state of mind called doubt is a form of intellectual event in which incompatible contents of thought alternate in consciousness and the development of consequences is in each case prevented: here again it may be assumed that the interruption of mental process therein involved is the source of the pain of the state. The experience called Belief (= sense of reality, Professor James: "Psychology," Chap. XXI.) is in the direction of pleasure, but it may be questioned whether the pleasure is intrinsic, whether it is not rather one of relief from the pains of doubt. (The pleasure of certitude, or belief conscious of itself, is spoken of by Newman: "Grammar of Assent," Chap. VI., as consisting in "the triumphant repose of the mind after a struggle.") The special pleasure of the confirmation of a belief may be given a provisional explanation by referring to the aid offered one process of thought by another. The pleasure in a recognition of the "identities struck by science" is, according to Professor Bain ("Emotions and Will," Chap. XII.), not only the positive charm of "the identification of likeness in remote objects and under deep disguises," but contains the negative element of "the lightening of an intellectual burden."

§ 42. The state of bewilderment, inability to comprehend what is presented to the mind, has a disagreeable character which may vary from simple annoyance (cf. Jean Paul's description of the vexation caused a writer by uncomprehended domestic noises, "Siebenkäs," Chap. V.) up to a discomfort of an overwhelming kind. Bewilderment as a state of overfulness of experience is sometimes contrasted with tedium, or ennui, as a state of mental emptiness. Yet the pain of tedium is unquestionably not one of intellectual default. Animals, doubtless, feel little ennui (one comfort of domestic pets is their capacity not to be bored), nor do men when their minds are at their emptiest, *e. g.*, in going to sleep. (Leopardi, "Pensieri," LXVII. and LXVIII.: Ennui is

felt only by those of some intellectual power; it is "little known to men of no consequence and least of all or not at all to animals." Cf. Drobisch: "Emp. Psychologie," 1842, Section 61. Professor Sully: "Pessimism," 1877, p. 235. Sir W. Hamilton: "Metaphysics," Section XLIV.) In tedium the mind is occupied, it may be claimed, with processes of idea that are balked in their natural progress, contents whose germination is hindered, or whose germinal power is insufficient, and which yet return again and again to consciousness. The discomfort of the state is not one, according to this, of a lack of ideation, but a lack of the fruition of idea. The mind is employed in ennui, but unsuccessfully.

§ 43. In one of their principal functions *Games* and *Sports* are an apparatus for the awakenment of keen anticipation in order to the pleasure of its satisfaction: *e. g.*, events of little or no intrinsic interest become victories or tend toward an eventual victory (points in a game); again the rules or customs of the game or sport provide that the result shall be doubtful despite the best efforts of the participants (shooting a quail on the ground would be "unsportsmanlike"). Further, under the tension of suspense created in sports and games, there is a heightened bodily and mental functioning which is pleasurable in itself, often still further intensified by the sense of danger: and to this pleasure is to be added that of the facilitated play of the mental powers (sense of freedom) consequent upon the final satisfaction of an awakened anticipation. The interest of a game which is played for money, or of a sport where bets are made upon the result, being in general much greater than the sum of the pleasures to be received from the recreation in itself, and from the gain of money in itself, the charm of gambling affords an illustration of Fechner's principle of *Æsthetic Aid* ("Vorschule," V.). An indirect pleasure-yield of sports and games is due to the fact that they are more or less complete mental (and often physical) alternatives, taking us into a world of their own, and bringing us back refreshed to the real one: (Cf. Lazarus: "Die Reize des Spiels," 1883.)

§ 44. Taking a suggestion from Fechner's analysis of witty comparisons, plays upon words, etc. ("Vorschule," XVII.) *Riddles* may be conceived as an exploitation of the pleasure of the unification (by the solution) of a manifold (presented in the puzzling data). We are given complexes of presentation which do not, at first sight, offer any channels of idea enabling us to pass from the contemplation of one already prepared for what another is to offer us; there being, nevertheless, such a mediating idea called the answer of the riddle.

When this is found the passage from one element of the riddle to another affords a pleasure of expectation satisfied, intensified though the excitement of the previous (and perhaps still threatening) disappointments. (On unity in multiplicity as an æsthetic principle, cf. Fechner, "Vorschule," VI.)

2. *Special Conditions.* (1). Morbid.

§ 45. (a). Melancholia and mania are two types of mentally diseased condition, distinguished respectively by the misery and happiness of the subject. According to Krafft-Ebing ("Psychiatrie," II.) the "fundamental phenomena in melancholic insanity are the painful mood of depression and a general difficulty which may become inhibition of psychic movements. . . ." "The facts compel us to regard the mental pain and inhibition as coördinate phenomena . . ." On the other hand mania is "a change of the self-consciousness in the direction of a predominately pleasurable mood of feeling and an abnormally facilitated flow of psychic activities, amounting sometimes to entire uncontrollability of the psycho-motor side of the mental life." These two characteristics are likewise to be considered as coördinate. The assumption involved in this theory of melancholy and mania is the general principle according to which the hinderment of mental function is painful and its furtherment pleasurable to the subject.

§ 46. The psychic phenomena of alcoholic intoxication are regarded as presenting a close analogy to those of insanity. (Griesinger: "Mental Pathology and Therapeutics," Eng. tr. 1867, Section 144, p. 310. Krafft-Ebing, "Psychiatrie," I., among "Analogien des Irreseins," finds the most exact to be intoxication by alcohol. Maudsley, "Pathology of Mind," 1880, p. 194, compares the first stage of happiness with mania and the following maudlin condition with melancholy; cf. the earlier decrease and the later increase of reaction-times under the influence of alcohol observed by Kraepelin: *Philosophische Studien*, I. 1883, p. 573, "Ueber die Einwirkung einiger medicamentöser Stoffe auf die Dauer einfacher psychischer Vorgänge.")

§ 47. (b). Impulsive insanity is a form of morbid state, in which both the pain of craving (or desire) and the pleasure of satisfaction reach an exceptional intensity. (Pyromania, kleptomania, homicidal mania, etc., are varieties.) The appetency seems in these conditions to fill well nigh the whole consciousness. Cases of the kind are adduced by Lotze as evidence of the psychological principle that "many even very complicated actions are carried out without definite volition." (Art. "Instinct," Wagner's *Handwörterbuch*.)

§ 48. (c). *Insanity and genius.* It has been debated of late whether the type of mind recognized as genius is or is not in essential characteristics a morbid type, whether mental operations betraying what is known as inspiration are to be called unhealthy or not. The affirmative is maintained by Lombroso ("L'uomo di genio," 5th Edn. 1885), and in a modified form by Eadestock ("Genie and Wahnsein," 1884). Hagen remarks that the deviations from the normal observable in the insane are monotonous: they resemble one another more than do persons of sound mind: ("Ueber die Verwandtschaft des Genies mit dem Irresein," *Allg. Zeitschrift für Psychiatrie*, 1877. A remarkable case of artistic inspiration in mental disease has been described by Dr. W. Noyes: *AM. JOUR. PSYCHOLOGY*, Vol. I. No. 3. Vol. II. No. 3.) The frequency of a blood relationship between genius and unhealthy mental types is a striking fact, yet the state of mind itself of inspiration appears to betray a divergence from the normal consciousness opposed in an important respect to that commonly exemplified in mental disease. For the genius is a psychic type in which self-consciousness is easily weakened or extinguished ("... Genius is nothing but the most perfect objectivity . . ." Schopenhauer: "Welt als Wille," section 36), while in the insane it is in general abnormally intense and persistent. The characteristic condition of genius is *unself-consciousness*: that of insanity *over-self-consciousness*: the normal make of mind being intermediate. In Plato's *Phædrus*, Socrates distinguishes between "two kinds of mania, one arising from human diseases, the other from an inspired deviation from established customs." Brentano ("Das Genie," 1892) argues against a difference in kind between genius and talent. Yet genius is surely not the "capacity for infinite labor," but the *incapacity not* to labor infinitely: a case where two negatives do not make the affirmative.

(2) Onirotic conditions.

§ 49. Under this head are here grouped a number of mental conditions possessing in common some prominent characters of states of dreaming, viz., a certain scantiness of psychic functioning (commonly intensified) and a diminution of volitional power and of the consciousness of self. The mental results of either external stimuli or psychic event are in these conditions less abundant than they are in the rest of the psychic life: they are simplified psychoses. ("L'hypnotisme, polarisation étrange de l'âme, n'en est donc, comme le songe, qu'une simplification." G. Tarde: "Criminalité comparée," 1881, p. 141.) The fact of interest in these conditions for the

present inquiry is that of their unsymmetrical relation to pain and pleasure. It may be claimed that they include most of the maximal pleasures known to man, and that while maximal pains have a certain tendency to produce them, when formed they tend to exclude the pain: moreover that they are in general less apt to be predominantly painful than states of ordinary complication (called waking states). (In "The Scientific Basis of Delusions: a New Theory of Trance," 1877, Dr. G. M. Beard grouped under the latter term somnambulism—artificial and spontaneous—mesmerism, hypnotism, catalepsy, ecstasy and like conditions, proposing the hypothesis that trance, which differs essentially from sleep, is "a functional disease of the nervous system, in which the cerebral activity is concentrated in some limited region of the brain, with suspension of the activity of the rest of the brain and consequent loss of volition." He elsewhere remarks on the fact that "the insane are not easily entranced.")

§ 50. (a). Dreaming. Sleep itself, in strict acceptance, is subject matter for psychology only as the fact of the discontinuity of the individual consciousness in time. As phenomenon of mind the term denotes the conditions called reverie, drowsiness, dreaming. The dreams of ordinary sleep are perhaps in general neither agreeable nor disagreeable. In exceptional cases both characters occur, often under circumstances which lead to waking (erotic dreams, nightmare). The emotion of terror may be felt in a dream more intensely than it ever is in the waking states of an ordinary civilized life. In some persons these dreams of fear occur only when the dreamer lies upon his back. If this is generally the case an explanation is suggested, based on the (disputed) assumption of the inheritance of acquired characters, by the fact that in human and pre-human conflict, sensations in the back have been associated with the terrors of flight or of inability for defense. The pain of nightmare is complicated with an intense form of the pain of a thwarted nervous process, that involved in crying out.

§ 51. (b). Natural somnambulism. The characteristic of the state is excitement of the motor centres. Assuming that successful nervous functioning contributes pleasure to the correlated consciousness, this state should, in good measure, be agreeable, for in general there is a noticeable exactitude in the motor processes it involves. Somnambulists are apt to be sure-footed and sure-handed (cf. Mark Twain's anecdote of the Mississippi pilot who steered his boat through a difficult passage while asleep). In waking life this exactitude of movement is a pleasure: the surety of hand of a ball or tennis player, the surety of foot of a leaper is a source of

pleasure to him independent of the results of his activity. A state of agreeable revery developed during prolonged and regular physical labor (mowing) is described by Tolstoi in *Anna Karénina*: "Les bien heureux moments d'oubli revenaient toujours plus fréquents, et la faux semblait trainer à sa suite un corps plien de vie, et accomplir par enchantement sans le secours de la pensée le labeur le plus régulier. En revanche lorsqu'il fallait interrompre cette activité inconsciente, enlever une motte de terre, ou arracher un bouquet d'oseille sauvage, le retour à la réalité semblait pénible."

§ 52. (c). Hypnotism. Insensibility to certain painful stimuli applied to the skin is one of the regular accompaniments of hypnotism. On command, and even without it, perhaps through the restriction of the consciousness of the subject to matters concerned with the personality of the operator (*rapport*), other pains, though naturally intense, are apparently unfelt (surgical operations). The nature of these phenomena is a question of interest. Are the presentations involved stripped of their painful character or do they fall out themselves; and is this lapse a case of unconsciousness or of want of notice? Does the hypnotic consciousness, further, behave in like manner in respect to pleasurable experience? *e. g.*, are there forms of pleasure to which the hypnotic is spontaneously unsusceptible; and can experiences be stripped of their character of pleasure on command? (Cf. the erogenic zones of Chambard; Binet and Féré: "Animal Magnetism," p. 152.) In the emotional sphere the hypnotic consciousness is called hyperexcitable: patients easily weep and laugh; moreover their emotions state have the inertia of all their mental operations: they tend to persist abnormally. (For a certain undisturbedness of bodily functioning in hypnotism an item of evidence is supplied in the simplicity of the muscle and pulse tracings obtainable during the cataleptic condition. *Journal of Nervous and Mental Disease*, Vol. X. 1883, p. 1. Note by Charcot and Richet.) States of concentrated thought have, as the name and the accompanying sensory (and often motor) quiescence shows, onirotic characters, although apparently not cases of volitional abeyance. Yet it may be questioned whether activity, effort of will, is not a foreign element in these conditions in the sense that in so far as it is excited they tend to disappear, and that for the most part they are a condition of passive waiting for the subject-matter to unfold itself before our mental vision. They seem like alternations of dreaming and waking to secure what we have dreamed. ("La pensée est le labeur de l'intelligence: la reverie en est la volupté." V. Hugo: "Les Misérables.")

153. (d). Shock. A bodily state, the result of severe injury, presenting defined characters and accompanied by a psychic condition akin to hypnotism. A striking element in this is the diminished or extinguished sensibility to the pain of the lesion (*e. g.*, the wounded in battle or in a railway accident are often unaware of their injuries). The phenomenon has been explained as an extreme case of inattention. Gruninger makes the supposition that this painlessness of grave injury is due to the fact the stimulus has eaten up so much of the potential energy of the nervous tracts it has attacked that what is left is insufficient to bring about a sensation of pain, this demanding a wide extensivity of nervous origin, the coöperation of much nervous matter ("Über den Shock," 1885).

154. (e). Narcosis. The bodily effect of narcotics is described as a more or less complete paralysis of some part of the nervous mechanism. On the principle of localization the psychic correlative of this phenomenon should be a more or less pronounced psychic simplification. The fundamental element in the ineffable pleasures of opium, according to De Quincey (and other witnesses), is the calm in which it immerses the consciousness: "Here were the hopes which blossom in the paths of life reconciled with the peace which is in the grave; motions of the intellect as unwearied as the heavens, yet for all anxieties a halcyon calm; a tranquillity that seemed no product of inertia, but as if resulting from mighty and equal antagonisms, infinite activities, infinite repose." ("Confessions of an English Opium Eater," p. 81.) On the principle claiming a pleasure to the inhabitant Psyche from the successful functioning of the body it inhabits, the delight of opium can be conceived according to this description to proceed from the relief of the hemispheres from the mass of little conflicts of function which are the customary results of the complexity of waking states; states of psychic simplification being freer from hindrances as the branches of a tree may interfere less when few than when many. In narcosis we seem to get the natural joy of the fullness of life, pure for the time being (*e. g.*, in onirotic conditions hardships meet no contradictions; the voice of sagacity fails to shatter the dream world).

155. (f). Emotion. The question has been debated in recent years whether emotions are to be regarded as solely compounds of other mental facts, or whether there are independent psychic elements involved in them. Mr. Hodgson holds the latter opinion: "The emotions in my theory become a new kind or mode of feeling depending upon the constitution and operation of nervous matter, and in this respect

are similar to sensations:" ("Theory of Practice," Vol. I. p. 108.) Prof. James takes the former view ("What is an Emotion?" *Mind*, XXXIV. 1884, and "Psychology," Chap. XXV.), holding that an emotion is the sensation of bodily changes brought about by its exciting cause. According to Lange ("Ueber Gemüthsbewegungen," 1885) these bodily changes are originally vaso-motor phenomena. Prof. James' view is criticised by E. Gurney (*Mind*, 1884; cf. E. Kroner, "Das Körperliche Gefühl," Chap. XI.). Lange is criticised by Lehmann ("Hauptgesetze," Section 83f.), who gives (Section 95f.) an account of his own experiments on the bodily accompaniments of emotion. He concludes vs. Prof. James and Lange, that the sensations from these are not the only constituents of emotion (Section 150), which normally involves an introductory pleasurable or painful presentation (Section 151), whose agreeableness or disagreeableness is its own and cannot be regarded as borrowed from accompanying bodily feeling (Section 164. Cf. Worcester in *The Monist*, Vol. 3, No. 3. The derivative nature of emotion maintained by E. Regalia, "Sul errore nel concetto di Emozioni," *Riv. di Fil. Scientifica*, Oct., 1890). On the bodily theory of the emotions their content in pleasure and pain is to be explained by a reference to the bodily disturbances their exciting cause awakens: and on any derivative theory by a reference to their component elements, sensational or ideal. They are discussed in the present connection in order to lay emphasis upon onirotic characters that are apt to be conspicuous in them. A state of emotion is one of more or less absorption or entrancement: emotions carry us away, put us beside ourselves, and while they may give pain as well as pleasure, it seems possible to claim for them the trend toward the latter that distinguishes states of psychic simplification in general. They might be described as less pronounced onirotic conditions, originating in an imaginative content generally markedly pleasurable or markedly painful. That one's views of any matter differ greatly under emotion, and most noticeably by default, from the impressions of a dispassionate contemplation, is a commonplace of psychological moralizing. The suggestion here offered is that emotion consists in this partial mental vision when the narrowing of the consciousness has taken place round an ideal content ("representational framework," Hodgson), generally markedly painful or pleasurable, with its appurtenances of sensation. The glow of the emotion would then be the result of a hyperæsthesia of the subject of these elements and these afterward accruing to the consciousness. The psychosis might in general include what are called bodily sensations, whose contribution in volume or in-

ness of pain or pleasure would perhaps tend to blind us to that of other psychic elements: but their partial or complete absence (as in the case of many æsthetic or religious enthusiasms) would not destroy the emotional character of the state, i. e., the character of an onirotic condition formed on ideal elements for the most part markedly agreeable or disagreeable. A conception like this would serve to account for the small revivability of emotional states (and hence for the backward psychology of the subject), for this is a character of dream-states too. (Of his desertion of Frederike, Goethe writes, "Those were painful days whose memory has not remained with me." "Wahrheit und Dichtung," Book XI. ad finem; cf. Tourgénéff, "Smoke.") The emotional states which best lend themselves to such an interpretation as this are the following:

§ 56. (a). Wonder, horror, etc. The expressions, "petrified" with fright or astonishment, and "frozen" with horror, suggest a cataleptic condition. (The original application of the Greek word *ἐκστασις*, our ecstasy, seems to have been to these states.) Surprising or horrible experiences seem to arrest the psychic machinery as it is arrested in hypnotism by the word of the operator, or through the fixation of shining objects. Conceiving that the onirotic condition tends towards pleasure and away from pain, the attractiveness to coarser natures of gladiatorial shows and even of scenes of torture (in Racine's "Les Plaideurs," Isabelle is invited by Dandin to witness an examination by torture) and of accidents, funerals, executions, representations of the horrible in art, even to the more civilized, may be explained by the power the vivid imagination of pain possesses to hypnotize the percipient even without any suggestion of personal danger. Animal cataplexy is conceived as allied to hypnotism; yet it seems often to be a species of petrification by terror. (Cf. Preyer: "Die Kataplexie," 1878.) Fear may also cause sudden muscular relaxation: (cf. suggestions of sinking through the earth in shame: the word humility is derived from Latin *humus*, the ground.) Such expressions as "blind fury," "taub vor zorn," suggest hypnotic characters also in anger. (The anæsthesia therein is very evident according to Lange, although he doubts the value of such expressions as evidence: "Gemüthsbewegungen," 1887, p. 34.) The slave was put at the elbow of a Roman conqueror to break up now and again his emotion of pride by bringing him to self-consciousness. In pity "by means of the knowledge that I have of (another), i. e., the presentation of him in my head, I identify myself with him . . ." (Schopenhauer: "Grundzüge der Moral," Section 16.)

§ 57. (β). Love. The proverb, "Love is blind," asserts a form of psychic simplification as a characteristic of amorous passion. What the form is, H. Beyle has sought to express by his metaphor of crystallization. ("Physiologie de l'Amour," published under the pseudonym "de Stendhal.") The idea of the loved one or of her return of one's affection is here a pleasurable nucleus of presentation (a naked branch lying in an abandoned working of a salt mine), to which and to allied ideas and impressions in so far as they are agreeable (the atmosphere of the mine in so far as laden with salt can deposit crystals thereof upon the branch), the more vivid consciousness of the lover is mainly restricted. "Ce que j'appelle crystallization, c'est l'opération de l'esprit qui tire de tout ce qui se présente la découverte que l'objet aimé a de nouvelles perfections;" there is further a second crystallization "produisant pour diamants des confirmations à cette idée: elle m'aime." The phenomena suggest those of "*rapport*" between operator and subject in hypnotism: but a *rapport* that is hedonic in origin and outcome, the consciousness of the lover being mainly held to pleasant sensation and suggestion. (Cf. Molière's description of his passion for Amande Béjart: "Toutes les choses du monde ont du rapport avec elle dans mon cœur; . . . Quand je la vois . . . je n'ai plus d'yeux pour ses défauts, il n'en reste seulement pour tout ce qu'elle a d'aimable;" and the scene in the "Bourgeoise gentilhomme": *Covielle*. Elle a les yeux petits. *Cleonte*. Cela est vrai. Elle a les yeux petits, mais les a pleins de feu, les plus brillants, les plus perçants du monde, les plus touchants qu'on puisse voir. Etc., etc.) The abeyance of self-consciousness in passionate love is the burden of a fable of Jelalluddin quoted in Fitzgerald's translation of Omar Khayyam. The sufficient condition for the production of such hedonic rapport seems to be the insistence of the attacks of pleasure from the given person. The summation of many charms may either gradually or suddenly set up a hedonic rapport with the charmer in the soul of the charmed. (On personal charm, cf. Prof. Sully: "Sensation and Intuition." Chapter on "The æsthetic aspects of character.") One of the most charming of traits is the habitude of unselfconsciousness, as that of selfconsciousness is one of the least pleasing. Hence a certain lack of personal charm in those accustomed to being "lionized" socially, and even generally in people of intellectual attainments. The charm of unselfconsciousness may be that of a "natural joyousness of temperament" (Prof. Sully: *ibid.*; "All the world loves a lover," Emerson), happiness being the mother mood of self-forgetfulness: (Mach: "Beiträge zur Analyse der Empfindun-

gen," p. 18); while the companionship of a reflective (and hence less joyous) nature tends to destroy one's own naïveté, and with it one's capacity for pleasure. ("L'habitude d'analyser toute chose m'a rendu irremédiablement triste," Theophile Gautier.)

§ 58. (γ). Religion. The Spanish mystic, Santa Teresa, describes the intenser forms of meditation upon divine things as a "sleep of the powers," the senses being suspended, the will in abeyance, and the consciousness restricted to the contemplation of Deity alone. ("Castello Interior" and "Autobiography," quoted by Mantegazza; "Le Estase Umane," 1887, Chap. X., and by Ribot, "Psychologie de l'attention," 1889, p. 143f.) These states of religious absorption appear to have possessed the trend toward pleasure and away from pain here claimed for onirosis in general. Santa Teresa had terrible visions sometimes, but for the most part glorious ones, where beatitude was beyond all description. Again perceptions of brilliant light (the principal sensational source of the hypnotic condition: cf. infatuation of insects and birds with light) were prominent in her visions: "It is an overflowing splendor that gives unimaginable pleasure to the sight and does not weary it." Again her trances became more complete and more glorious as they were multiplied (as the perfection of hypnosis grows with the habituation of the subject): and the fact was a great mystery to her. Again the passivity of her state seems to have been an essential element of it, for activity broke it up. One apparition she had the greatest desire to make more clear, "but all my efforts served only to make the vision disappear more completely." (Cf. the fables of Cupid and Psyche, and of Lohengrin, where action is again a kill-joy.) The penances and hardships Santa Teresa inflicted upon herself as well as the self-tortures of all other religious enthusiasts and the constancy and even beatitude of martyrs may be taken as evidence of an onirotic insensibility to physical pain. According to Spitta ("Traumzustände der menschlichen Seele," 1882), ecstasies (*e. g.*, those of religious exaltation) are a disease of the emotional nature and not to be counted among dream-conditions which are healthy. Yet if by disease is here meant a state incompatible with an average duration and vigor of the bodily organism, an extraneous character is made the basis of a classification of mental conditions, to the neglect of intrinsic likenesses. Moreover the fact may be disputed.

§ 59. (δ). Beauty. The state of æsthetic contemplation is fundamentally a hypnotic condition (passivity: narrowed consciousness) according to Souriau ("La suggestion

dans l'art" 1893), who quotes to the same effect Bergson ("Essai sur les données immédiates de la conscience," 1889, p. 11), "Les procédés de l'art sont comme une forme atténuée, raffinée et spiritualisée en quelque sorte des procédés de l'hypnotisme." (Cf. note by Ola Hanson in Schmidkunz's "Psychologie der Suggestion," 1892.) In reading a play of Shakespeare, writes Gustave Flaubert ("Correspondance," I. p. 163), "On n'est plus homme, on est œil." (Cf. Schopenhauer: "Welt als Wille," Section 38: In the contemplation of beauty one is no longer "an individual, but pure will-less subject of knowledge.") Of artistic creation, Flaubert writes ("Correspondance," II. p. 359): "C'est une délicieuse chose que d'écrire, que de ne plus être soi, mais de circuler dans toute la création dont on parle. Aujourd'hui, par exemple, homme et femme tout ensemble, amant et maîtresse à la fois, je me suis promené à cheval dans une forêt par une après-midi d'automne sous des feuilles jaunes, et j'étais les chevaux, les feuilles, le vent, les paroles qu'on se disait et le soleil rouge qui faisait s'entrefermer leur paupières noyées d'amour." The happiness of the state is by Schopenhauer attributed to its calm (Section 38, "It is the painless condition that Epicurus extolled as the highest good, and the condition of the gods: for we are for the moment freed from the vile urgency of will, we celebrate the Sabbath of the work-house labor of volition, the wheel of Ixion stands still." Cf. Narcosis). Yet there is certainly involved, besides, a hyperæsthesia to the pleasurable content round which the dream-state has been formed. Conceiving in this way of æsthetic contemplation as onirotic in character and conceiving of its object as the form (consensus of relations between parts) of what is contemplated (cf. Kant: "Kritik of Judgment," Bernard's tr. p. 90: Herbart's "Werke," III. 381). Beauty might be described as a pleasurable content in the internal relations of an object which makes, or should make, their perception the nucleus of a purely pleasurable dream-state; i. e., when the perception of the relations between the parts of any whole is unremittingly pleasant ("Rien ne me choque," Chopin), their contemplation ends in a dream-state over the object productive of no other than pleasant suggestion, this being the mood of enthusiasm over its beauty. The consciousness is narrowed down to the unselfconscious contemplation of the object in question and related agreeable presentation. The beautiful is so often the simple, we may claim, because so seldom can the relations between the parts of a complex object be kept even predominately agreeable. What is called an "artistic atmosphere" may be regarded as an environment where pleasure-giving experiences of this kind are frequent enough to be apt to sum themselves to a height

Hecker proposes a theory of the physiological basis of the perception of the ludicrous, according to which laughter is a consequence advantageous to the organism. Like the experience of being tickled, that of the appreciation of a joke has as a physiological accompaniment an intermittent pressure upon the brain through the intermittent contraction of the minute blood vessels therein, laughter equalizing this by causing their congestion. ("Physiologie und Psychologie des Lachens und des Komischen," 1873.) The conception of inferiority is sometimes thought to be the essential element in the ludicrous, but more commonly that of incongruity. The former analysis is that of Hobbes ("Human Nature," Chap. IX. Section 13): "I may therefore conclude that the passion of laughter is nothing else but *sudden glory* arising from some sudden *conception* of some eminency in ourselves, by *comparison* with the *inferiority* of others, or with our own formerly." Akin to this is the formula of Bain, who finds that "the occasion of the ludicrous is the degradation of some person or interest possessing dignity in circumstances that excite no other strong emotion." ("Emotions," Chap. XIV. Section 39.) The latter analysis (incongruity) is illustrated in the formula of Kant, who finds absurdity the basis of the comic ("Kritik of Judgment," Section 54, Bernard's tr.): "Laughter is an affection arising from the sudden transformation of a strained expectation into nothing:" moreover, the pleasure of the ludicrous is solely that of the accompanying laughter; "the lungs expel the air at rapidly succeeding intervals, and thus bring about a movement beneficial to health: which alone, and not what precedes it in the mind, is the proper cause of gratification in a thought that represents nothing." To Prof. James the emotion of the ludicrous is like other emotions, the feeling of its expression (*i. e.*, laughter): "If we imagine away every feeling of laughter and tendency to laugh from [our] consciousness of the ludicrousness of an object," would what remains be "anything more than the perception that the object belongs to the class 'funny?'" ("Psychology," Chap. XXV.) In Schopenhauer's theory ("Welt als Wille," I. Section 13), "Laughter never arises from anything else than the suddenly recognized incongruity between a conception and the real objects that in some respect or other have been thought through it, and it is itself simply the expression of this incongruity." This conquest of perception over thought delights us ("Welt als Wille," II. Chap. 8): for perception is the effortless medium of our pleasures, thought the laborious medium of our cares. ("Grau, theurer Freund, ist alle Theorie, und grün des Lebens fruchtbar Baum." Faust.) "It must therefore be delicious to us to see this hard, tireless, oppressive

taskmistress Reason for once proved insufficient." As the writer sees the matter, the perception of the ludicrous is not complete when the incongruity here described has been discovered, nor does the pleasure of the comic proceed from its recognition. The completed perception of the ludicrous involves a sequence of a satisfaction upon a disappointment, and the pleasure of it is the intrinsic pleasure of the one sharpened by the excitement of the other. When an object of perception is ludicrous to us, there ensues at a certain point in our contemplation of it some development of presentation which is incompatible with the existing content of our expectation, and something, moreover, which does not leave us simply bewildered—*i. e.*, does not simply make the object incomprehensible to us, empty our minds, or put us in a whirl of those reachings forth of the imagination which are involved in comprehension,—but which is of a nature to substitute a new mass of ideal activities for the old one proved insufficient, the new comprehension taking in not only old elements of the object of comprehension, but also the new and hitherto surprising one. The moment of this twist of the mental kaleidoscope is the moment of "seeing" the joke, and the glow of the ludicrous is our delight at the new order that springs out of the momentary confusion. (On the ludicrous, cf. Prof. C. C. Everett: "Poetry, Comedy and Duty," 1888, II., "The Philosophy of the Comic.") In the conception of the Tragic, the idea of human plans rendered impossible of realization is an important if not essential element. According to Prof. Everett (*ibid.* p. 134), tragedy involves the three elements, necessity, blindness and retribution. Perhaps the formula might be, if α , then necessarily β , but if β , then I (an ill): the sequences involving the activity of persons but partially alive to the situation in which they are involved. Prof. Everett remarks upon the similarity of psychological outline between the comic and the tragic (p. 165f.), and finds a fundamental differentia to be that the comic is given in the relation itself of incongruity, while the tragic involves the elements entering into an incongruous relation, the causes that produce it and the effects resulting from it (p. 188). The special fascination of the tragic may be ascribed to the onirotic condition produced by its horror, with the accompanying hyperæsthesias among other things to the pleasure of the realization of recognized necessities. (The *κάθαρσις* of pity and terror, which Aristotle's description of tragedy asserts as its result to the spectator, is interpreted by Mr. Bosanquet, "Hist. of Æsthetics," p. 64, as "an alleviating discharge," the rendering being based upon a passage in the "Politics," Book VIII. Chap. VII., where a similar working of music is described as "a

kind of purgation and relief accompanied by pleasure.")

§ 62. The FINE ARTS may be defined as those branches of human activity into whose aims the purpose to construct beautiful objects enters as an element. They may be classed as mixed (decorative) and pure (high), according as their products are or are not constructed with other ends in view than that of the embodiment of beauty. They may again be classed as dynamic arts (arts of movement) and static arts (arts of rest), according as their products do or do not involve change as an essential element. Among the latter the more conspicuous are picture, sculpture, architecture, ornament (of utensils of stone, earthenware, wood, metal, glass, etc.; of textile fabrics : arts of the gardener, of the jeweler, etc.): among the former, manners, dancing, drama, literature, music.

§ 63. *Picture* may be described as the beautiful representation of visible things by the application of color to a surface (including the use of black and white). The purpose of any imitative art may be said to be illusion, but not deception: the beholder is not to conceive himself in the presence of what is imitated, but to forget himself in the beauty presented to his contemplation. The value of verisimilitude in picture may in part be explained by assuming as a general principle that that alone to which we are accustomed gives us pleasure. Recondite habits derived from daily visual experience wait within us to be engraved deeper; the subjective result of this process being enjoyment. There is no such response within us to unreal representation, whether simply arbitrary (unfamiliar) or conventional (overfamiliar); hence the greater masters of picture have first been great seers. Yet verisimilitude, while necessary, is insufficient in picture. Compare with the utterance of de Goncourt (*Journal* III. p. 127), "Le supreme beau est la représentation de génie exacte de la nature," that of Heine (in "Gedanken und Einfälle"): "Daguerreotypy is a witness against the erroneous view that art is imitative of nature. Nature herself offers evidence how little she understands of art, how lamentable the outcome is when she tries her hand at art." That the confusion of beauty and truth is an attempt to obliterate a real distinction is again the burden of the passionate line (*Letzte Gedichte* : "Für die Mouche"): "Stets wird die Wahrheit hadern mit dem Schönen." Evidence in support of the doctrine basing beauty on form (*i. e.*, relations), is given by the instinctive choice in discussions of picture of words (as yet without clear and generally received definitions in this application) derived from the terminology of music, *e. g.*, tone, pitch (perhaps place in a color scale), harmony (relation of color), chord (combination of relations of color), key (relation between com-

binations of relations of color.) (Cf. Prof. Van Dyke: "Art for Art's Sake," lectures on the technical beauties of painting, 1893.) A distinction may be made between the art of picture and the art of painting; the products of the former being the pictorial result, those of the latter the pictorial process. (Cf. Van Dyke: *ibid.*, Lect. VII., who writes of brush work, "If rightly used it is an embellishment of art, and in some cases it is art itself.") To the eye of a craftsman a product of picture may be the evidence of another work of art, invisible to those not of the craft, viz., the method which has brought it forth. Thus among "painters' painters" there may be those whose manner of painting is their real achievement: yet his pictures must be the achievements of a painter for the world. In this sense of an art of the process, any fine art must be conceded to have but limited importance.

§ 64. *Sculpture* may be described as the beautiful reproduction of the form of tangible things; the object of representation generally chosen being the human figure. As far as this material goes the art may be claimed to be an exotic in modern European civilization. Since the human form is an object we seldom see, there are no visual habits of its observation within us to be flattered by the modeler's art. Fechner remarks that the sense of the beauty of the human foot is entirely lost in modern Europe, what is called a pretty foot being in reality a particular form of shoe we have grown to admire. (With the Chinese it is worship.)

§ 65. *Architecture*. According to Lotze ("Geschichte der Ästhetik in Deutschland," p. 507), a work of architecture exists whenever many separate heavy masses are combined into a whole that maintains itself in equilibrium upon a supporting plane through the interaction of its parts. Architecture is to be regarded as a mixed art, since its products almost always subserve other purposes than those of æsthetic contemplation. An important psychological principle of architectural beauty is that according to which a building should in none of its parts awaken conceptions of structural necessity which are either contradicted or fail of satisfaction by other parts. Nevertheless forms admitting readily of interpretation as ornament may without detracting from its beauty be of a character to make structural suggestions about a building which it does not realize. Such devices are of the nature of artistic play: they betray the outlines of the comic; i. e., a suggestion made by one element in a whole is contradicted by another element, the contradiction ceasing upon another patent interpretation of the whole. In the present case this latter is the interpretation of the given form as superfluous (ornament). The forms of Greek architecture

which can be interpreted as reminiscences of wood construction are perhaps to be regarded as essentially ornament: the style would not then be on their account defective. The same can be claimed for the delicate nonsense of classic form in earlier Renaissance architecture. But the ponderous Roman builders and those of the high Renaissance missed the point of their predecessors' humor, and made inharmonious earnest of the playful efflorescence of earlier and happier times. One good reason exists for conceiving of architecture as "petrified music" in the resemblance between the use of typical forms in architectural styles and thematic development in musical composition: *e. g.*, in Gothic the pinnacles of buttresses repeat the spire form, the pointed arch of the vaulting is repeated in the windows, etc., etc. Lotze finds the flying buttresses of Gothic by no means a happy thought; they give the idea of a scaffolding left standing: (Cf. de Stendhal: "Memories d'un Touriste.") The world yet waits to be impressed by the beauty of forms of iron construction in architecture. Is this simply because these admit of a greater freedom of line than the powers of design possessed by the present generation are able to cope with? or because they are in large measure determined by other (useful) considerations than those of beauty; or because architectural iron work is by nature a construction full of straight lines, of great complexity, must be given a color and is devoid of romance, *i. e.*, there is no history in it, and no suggestion of permanence?

§ 66. *Ornament.* The sense of beauty has always in greater or less degree contributed to determine the look of every kind of utensil—machinery of shelter, food, transportation, etc. According to William Morris ("Hopes and Fears for Art"), this fact is the result of the joy of the producer of these things in his product; and it is, moreover, because under the wage system of modern Europe, this joy has vanished that the goods and chattels of a contemporary civilized householder are in general lacking in any charm of appearance. (Hence we hark back with antique furniture, old iron work, etc., etc., away from the present joylessness of laboring lives.) The development of national and tribal costumes has doubtless been influenced by the sense of beauty in that happy hits of dress have been copied, cleared of their unbecoming accompaniments, the coöperation after this manner of many generations of a people resulting in a type of clothing of æsthetic value. The demand of modern life that many modifications of clothing shall be brought forth by one generation, is one which the æsthetic inventiveness of mankind is entirely unable to meet, and in its fulfillment the sense of beauty is to a good extent inoperative.

§ 67. *Manners.* The conception of elegant manners as a delicate form of beneficence, of a gentleman and a gentlewoman as exponents of goodness in the daily personal relations of life, is a moral and not an æsthetic theory of behavior. Yet it is possible to regard good breeding as a form of the incorporation of beauty, and a gentleman and gentlewoman not as saints, but as artists. While it may be that fine manners are at the same time right action, the beauty of behavior is nevertheless an entirely different thing from its morality. The French phrases, "grand seigneur" and "grande dame," have more æsthetic significance than the corresponding English words: a sign, perhaps, of a stronger sense for rightness of conduct in the Anglo-Saxon consciousness. Social life in the restricted meaning of the meetings of a community for the sake of meeting ("society"), has as its motive neither the gratification of vanity solely (for which Thackeray had so delicate a sense), nor impulses of good will solely (which would appear to be mainly operative where as often in the United States social intercourse is made another side of religious association: church teas, receptions, etc.), nor simply the wish to be amused: (Cf. McAllister's "Society as I have found It.") An æsthetic element enters essentially, the impulse to make a work of art out of the elements offered by simple companionship *en masse*. Amiel writes ("Journal Intime, II." p. 114): "Les réunions choisies travaillent sans le savoir à une sorte de concert des yeux et des oreilles, à une œuvre d'art improvisée. Cette collaboration . . . est une forme de la poésie . . ." The like æsthetic conception of conversation, not for information, nor edification, nor as an avenue of sympathy, nor an opportunity to shine (*se faire valoir*), but for the charm inherent in the form a few of talk may assume, is according to common report chiefly a growth of French soil. (Causerie.) Although Lord Bacon wrote ("Essays," XXXII), "The honorablest part of Talk is to give the Occasion," we still find special conversational powers attributed among English speaking people to those who can only lecture. Considered as a texture of speech woven in common by several interlocutors, a conversation can hardly lay claim to beauty without the observance of some or all of the following rules: (1.) Every participant is to listen during a much longer time than he talks (else he will be lecturer and the other audience.) (2.) The topic is to be changed every few minutes (else to some one it will no longer be productive of idea, or the interest in it will extinguish interest in the manner in which the shuttle of discourse passes from one to another.) (3.) Every subject is to be

lightly (else interest may again leave the manner for the matter of the talk). (4.) There is to be no argument there will be lecturing, tête-a-tête, vehemence, interruption, expression, or other mutilation of the form of the conversation. (5.) There is to be little or no anecdote (for the anecdote in a menu, anecdote for the time destroys one's interest, and if used when a period is not needed, easily spoils a capping of stories). (Cf. Disraeli in "Lothair," "The conversation fell into its anecdotage.") (Cf. Deane's "Hints toward an Essay on Conversation.")

Dancing may be viewed as an exploitation of the rhythm there is in human movement in general, or in expression (gesture). Dances exhibit all degrees of the prominence of the formal and the expressive (story and element. (Minuet—Pantomime.) An Oriental, inquiring why the Occidentals do not engage others to dance, dancing for them, takes the æsthetic view of this as a form of social play which to the western world is not a visual art, but an intoxication of muscular, auditory and emotional elements. (This general subject has been given a discussion by Souriau in his "L'æsthetique du mouvement," 1889.)

Drama. The object of the drama may be said to be the incorporation of beauty in representations of the fates of human beings. Psychological factors fundamentally involved are the pleasures accompanying both anticipation (e. g., suspense) and satisfaction (e. g., poetic justice). Human plans are essentially impossible form subject-matter for drama; comedy perhaps generally exhibiting a superficial purpose which turns out an essential fulfillment. In the tendency toward elaborate perfection of scenic effect, the stage presents the spectator with a mixed product, elements both from the art of personal fate (drama) and from the art akin to picture. In the operatic plays (Festivals of Wagner, the dance and music lend their aid to the scenic representation; and in his æsthetic writings he has declared the goal of artistic drama. (Schriften, III. p. 115ff., "Das Kunstwerk der Zukunft.") But cf. contra, the remark of Goethe, "One of the most unequivocal signs of the decadence of art is the separation of different branches of it." Einleitung in die Werke. Werke Ed. Cotta, XXIV. p. 219; cf. also the following this sentence). (On the drama, cf. Lessing's "Theatralische Dramaturgie" and Professor Sully's essay on "Sensation and Intuition.")

Literature. Written discourse consists of the symbolism of a sequence of sound (partly noises or

ants and partly tones or vowels), which can be led into elementary combinations used as signs of ideas (men (words)). To be literature this symbolism of sign and sound must be an incorporation of beauty, and this it achieves either through the sound or its significance, or both. The influence of sounds may have charm either material (agreement of elements; in literature, syllables) or formal (manner of character: in literature, [1] assonance either initial or final=rhyme, [2] rhythm, [3] metre). The opinion that it is verse alone that concerns itself essentially with the primary charm of discourse, prose having to do with the matter of its ideal content alone, can hardly be maintained. Heine writes ("Gesch. der Ästhetik," p. 639) of "a false position of metric speech to prose," the former only fulfilling further certain demands of the ear met also by the latter. Heine speaks of the offense given delicate ears by the combinations and separations of phrase in prose that belong rightly to poetry. ("Über L. Börne," I.) The remark of Flaubert (quoted in Bourget's "Essais de psychologie Contemporaine, I. p. 170), "Les phrases mal faites . . . oppressent la poitrine, gênent les battements du cœur; et se trouvent ainsi en dehors des conditions de la beauté," suggests pleasurable (or unpainful) delivery of discourse as a further element of its charm. The auditory element in the delight of verse is specially appealed to in the work of some contemporary writers: (e. g., Swinburne, later French poets). One ignorant of the Romance languages feels the element of poetic charm alone, and often most intensely in listening to a reading of a master of Italian verse (e. g., Petrarch). A definition of poetry that drops the element of sound for that of sense is contained in J. S. Mill's early essay (1833), "Thoughts on Poetry and its Varieties." It is a rational soliloquy. "Poetry is feeling confessing to itself moments of solitude" (and differs therefore from eloquence, which is address). That is, although words are the necessary form of incarnation of products of poetry, the poem itself is a certain train of thought interpenetrated by certain emotion, having the character, moreover, that it does not seek to communicate itself. A definition which would seem applicable to both elements, sensational and intellectual, and which, further, carries in it a reference to moral ideas, is that of Matthew Arnold, who speaks of poetry as "a criticism of life under the conditions fixed for such a criticism by the laws of poetic truth and poetic beauty" (preface to Ward's *English Poets* and elsewhere). The question as to which of these conceptions (or what other) presents to us the true ideal of the art, is an ambiguous one. It may mean: (1)

Which of them can give us the most perfect (or fullest) incarnations of beauty? (2) Which can give us the noblest (embodying moral ideas or producing moral effects) incarnations of beauty? (3) Which conception best expresses the essential nature of those units of discourse the world has hitherto agreed to call poems? (On poetry, cf. the recent minute investigation into the lyric by Professor Werner: "Lyrik und Lyriker," Vol. I. of "Beiträge zur Ästhetik," 1890.) The modern development of the novel has extended the discussion between realism and idealism in art to the domain of the story-teller also. The modern French masters of romance, de Stendhal, Flaubert, de Goncourt, Zola, Maupassant, Bourget, are, in general, conceived as representatives of the former alternative. Yet to Flaubert the material (coté vaudeville) of "Madame Bovary" was indifferent, his purpose being the composition of "quelque chose de gris" as his purpose in *Salammbô* was the presentation of "quelque chose de pourpre" (cf. the flaming idealism of "La Tentation de St. Antoine"). Zola's claim to the title of realist has been disputed, and an idealism of the disagreeable found the phrase more applicable to his art. Maupassant writes: "Le réaliste s'il est artiste cherchera non pas à nous montrer la photographie banale de la vie, mais à nous en donner la vision plus complète, plus saisissante; plus probante que la réalité même" (preface to "Pierre et Jean"): an opinion not far from Goethe's "People say: let artists study nature! but it is no little thing to develop the noble out of the common, beauty out of formlessness:" (Maximen und Reflexionen, III. Abth.) In this we may say art simply carries on a work already begun in common observation.

§ 71. *Music*. We may perhaps interpret Mr. Pater's remark that "all art constantly aspires toward the condition of music" ("Studies in the Renaissance: The School of Giorgione") as the expression of the idea that a work of music may be more purely beautiful than the product of any other art. A piece of music needs no excuse but its beauty for its being. Any production of tones for the enjoyment of their relations is music: ("la quale è tutta relativa," Dante, *Convito*, II. Chap. XIV.). Yet its extra-auditory effect may be admitted as an important if not an essential part of the art: ("ancora la musica trae a se gli spiriti umani," *ibid.*) Fundamental in the auditory structure of works of the developed art (*e. g.*, the music of modern Europe) is the fact of scale. A scale is a melody held in the performer's mind, by the production of whose notes without restriction as to how often or in what order in time his performance

proceeds. These generative melodies of music consist ordinarily of notes repeated in octaves, and the number of intervals per octave is in general either five or seven. The reason of this fact has not yet been clearly made out. The XIV. Abschnitt of the *Lehre von den Tonempfindungen* of Helmholtz contains a discussion entitled a Rational Derivation of the Scale, which yet the text indicates is taken not simply as a way in which the five and seven step octave may be, but in which it has been produced. Since this derivation proceeds upon the assumption of a note held in the mind of the performer through the performance which gives rise to the scale (tonality), and since not all primitive music appears to involve this procedure, the latter claim can hardly at present be allowed. For the definite settlement of the question, much fuller and exacter knowledge of the forms assumed by primitive (or simpler) music than we now possess is essential. (This branch of research has received many contributions of late from A. J. Ellis, Baker, Stumpf, Land, the writer, and others. On other facts of the structure of music in pitch: tonality, its history and ethnology, key, modulation, discord and resolution; consult Aristotle *Problemata*, XIX. 36; Bryennios, "Harmonik," quoted by Gevaert: "Histoire et Théorie de la Musique de l'Antiquité," I. 381. Zarlino: "Istitute Armoniche," 1558. Von Winterfeld: "J. Gabrieli und sein Zeitalter," 1834. Hauptmann: "Die Natur der Harmonik und Metrik," 1853. Helmholtz: "Tonempfindungen," 1862, XIV.-XVIII. Abschnitt. Sully: "Sensation and Intuition," 1874. Gurney: "The Power of Sound," 1880. Seinitzer: "Die psychologischen Wirkungen der musikalischen Formen," 1885. Musical forms are discussed in connection with those of other arts by Professor Raymond in "The Genesis of Art-form," 1893.) The discussion of structure of music in time falls under the general doctrine of rhythm and metre: (cf. the great work of Hauptmann above mentioned; Bain: "Emotions and Will," Chap. XIV. 12; Westphal: "Die musikalische Rhythmik seit J. S. Bach," 1880.) In a work of music, relations of tone are combined in a chain of events. (For the perception of the beauty of melodic form, Gurney posits a unique faculty. "Power of Sound," criticised by Professor Sully: *Mind*, XXII., and by Stumpf: "Musik-psychologie in England," *Vierteljahrsschrift für Musikwissenschaft*, 1885.) The changes involved in the sequence are either in pitch (which has but one dimension), in intensity, or in timbre (quality of sound). The textures of notes of which music consists, therefore exemplify, or present us with, general forms of change in concrete instances

of a very simple kind and unincumbered with details: (cf. Lotze: "Grundzüge der Ästhetik," Chap. III.) But any object of contemplation may be what is to the soul not only through the general notions of which it is a concrete case, but through the other concrete cases of the same notions which it may call up before the fancy, as well as through other psychic fact that may be complicated with it or with these suggestions. The distinction between classic and romantic impressiveness is based on the first two of these alternatives. According to Heine ("Religion und Philosophie in Deutschland," Book I.), "the handling is classic when the form of that which is presented to us is completely identical with the conception which it is purposed to present The handling is romantic when the form does not reveal the idea [to be presented] by identity, but lets it be guessed parabolically." (Cf. also James' "Psychology," Chap. XXV.) The extent and intensity of the extra-auditory effect of music have always excited the wonder and curiosity of civilized man. Three questions are fundamental in the matter: (1) What are the nature and extent of the power of experiences of tone over the rest of the psychic life? (2) What are the sources of this power? (3) Are these elements from psychic domains outside the auditory essential or unessential to the beauty of works of music? The results of an attempt by the writer to make an experimental contribution to the first of these questions are given in a "Report on an experimental test of musical expressiveness," (first published in the AMERICAN JOURNAL OF PSYCHOLOGY, August and October, 1892.) As to the sources of the extra-auditory effects of music, while we may recognize the association of idea (romantic effect) as the main channel, the influence of tone on animals and nervously unhealthy persons can hardly be accounted for on an intellectual formula: (cf. charm of light for insects.) The question as to which element in the impression made by a music, that internal or that external to the auditory sense, contributes mainly (or perhaps exclusively) to its beauty, has been made prominent of late years from the fact that the theoretic writings of R. Wagner can be interpreted in favor of the latter alternative (unquestionably also the popular one). "Tone is the organ of the heart" (Wagner's "Schriften," III. p. 99; cf. H. Spencer's essay on "The Origin and Function of Music," 1857, and "Origin of Music," *Mind*, October, 1890. Cf. also Darwin: "Descent of Man," Part III. Chap. XIX., on "Voice and Musical Powers." Darwin's view discussed by Gurney in the "Power of Sound," Chap. 21; and destructively criticised by Stumpf: "Musik-Psychologie in England," 1885). The

former alternative is represented by E. Hanslick ("Über das Musikalisch-schöne," 1854). Gurney accepts the classic alternative also, and denies that this internal impressiveness of tone textures is non-emotional: æsthetic emotion of the intensest kind may have a background of auditory presentation pure and simple ("Power of Sound," and *Mind*, 1884). (In this discussion, cf. G. Engel: "Æsthetik der Musik," 1884, III. Abschnitt; Saint-Saens: "Harmonie et Melodie," 1885; Hansegger: "Die Musik als Ausdruck," 1887.) On questions of delimitation among the fine arts, a classic authority is the *Laocoön* of Lessing (sub-title, "On the limits of poetry and painting"). (A useful "guide to the literature of æsthetics" has been prepared by Messrs. Gayley and Scott, and is published as No. 11 of the Library Bulletins of the University of California.)

III.—THEORY OF HABIT.

§ 72. In an abstract of lectures published during 1889-1890, the writer proposed the hypothesis that "the source of all pleasure is the reperformance by the nerves of activities which have once become familiar to them," and that "pain has its source in a violation of nervous habitude." The word nervous was here used for simplicity, instead of Fechner's more careful term of "psychophysis process." While any precise hypothesis of the physical conditions of pain and pleasure, advanced at present, may be expected to be incorrect in important respects, yet the attempt to form one is worth making for the sake of the advantage which clearness has, even if mistaken, over confusion of thought. The following more detailed formulation of this notion of the dependence of pain and pleasure upon the repetition of psychophysis change is now proposed.

§ 73. Considering any bodily process as a sequence of state of affairs β upon state of affairs α , it is assumed (1) If α is novel to the body which it involves, the occurrence of the process leaves a special trace therein (by which is meant only that the body is different after the process from what it was before, in a way that differs for different processes), which is greater or less according to the intensity of the process, and which continually diminishes and eventually vanishes if it be not repeated. (2) If α have occurred before in the given body, but only with β as its consequent, the trace of the process if it have disappeared will be renewed, and if it still exist will be increased, thereafter to diminish as before as long as the process does not recur, although, according as it is greater, a greater intensity of the process will be required to give the same increase. (3) If

α has occurred before with other than β as its sequent, the trace resulting will have the character of the greatest of the traces which would have been produced by the occurrence of each of the various processes of which α has been the inception without the others, and a quantity equal to the difference between that of the greatest and the next smaller. There will then be no trace in the body of any process involving α as a first term, unless one sequence from it shows a superiority to all others when we take both number, recentness and intensity of occurrence into consideration. It is further assumed that any process which has a trace in a given body is a habit of that body, and that a habit is formed by every recurrence of a process that deepens its trace. Using these postulates, the hypothesis here presented may be expressed as follows: Any presentation correlated with a bodily process that tends to fix a habit (increases a trace), is pleasurable; while any presentation correlated with a bodily process that tends to loosen a habit (decreases a trace), is painful. The latter case is exemplified in the occurrence of any process α -followed-by-other-than- β in a body where the trace α -followed-by- β exists. It is this phenomenon that it is intended in this discussion to denote by the phrase "the thwarting of a habit."

Expressed without the aid of the conception of a trace involved in habit, the principles constituting the hypothesis are as follows: (a) Presentation correlated with psycho-physic event which is novel to the body it concerns, is neither pleasurable nor painful. (b) Presentation correlated with psycho-physic event which is a recurrence in the body it concerns, will, when its outcome has a superiority over any other that has before attended its inception, taking both frequency, recentness and intensity into account, at first be pleasurable (habit forming) and later unattended by pleasure, unless it occur in unusual strength (habit intensified) or after a considerable interval (habit renewed); and it will always be painful when its outcome has an inferiority in the respects named (habit thwarted).

§ 74. If we consider the conception of function to involve that of a repetition of change, and conceive of the nature of a bodily organism as a consensus of function, much of the evidence which has given the doctrine of pleasure as furtherment and pain as hinderment of life, its preëminent hold upon human belief may be claimed in favor of the hypothesis here proposed. (In Plato's "Philebus," Socrates asserts that pain is produced by the disturbance of, and pleasure by the return to, the natural connection between elements of a bodily organism. Cf. Aristippus *γένεσις εἰς φύσιν* and its contrary: Zeller:

"Geschichte," II. 1, p. 353.) For this evidence will, we may suppose, in large measure consist of cases in which the balking or the intensified performance of a certain course of change which is a familiar recurrence in the organism concerned, is found to be accompanied by pain or pleasure respectively. By the substitution here made of the conception of the habits of an organism for that of its nature, the cases are covered in which pleasure and pain arise, not through the vivification or thwarting of inveterate repetitions (functions, normal activities), but in the earliest recurrences and earliest deviations of psychophysis event.

§ 75. It is the conception of the nature, instead of that of the habits, of the bodily organism which is used in Spinoza's theory of pleasure and pain; pleasure being based on the strengthening of the powers constituting this nature, and pain not upon their thwarted exercise, but their weakening. ("Ethics," III. Prop. VII.: "The endeavor wherewith everything endeavors to persist in its own being is nothing else but the actual essence of the thing in question." Prop. XI.: "Whatsoever increases or diminishes, helps or hinders the power of activity in our body, the idea thereof increases or diminishes, helps or hinders the power of thought of mind." Note: "Thus we can see that the mind can undergo many changes and can pass sometimes to a state of greater perfection, sometimes to a state of lesser perfection. These passive states of transition explain to us the emotions of pleasure and pain. By pleasure, therefore, in the following propositions, I shall signify a passive state wherein the mind passes to a greater perfection.") Aristotle's remarks, that pleasure is the sign of the perfection of an act, as a blooming cheek is of health, appears to involve the conception of the realization of ideals of activity (cf. Zeller: "Geschichte," II. II. Abth. p. 618). In our theory of habit such ideals are posited, but they are defined as those laid down by the past experience of the organism concerned; moreover, it is not every realization of these that is pleasurable, but only such as impress them deeper as habits of the organism. Ideals of life not fleeting like those of habit, but permanent and as numerous as are the types of sentient beings, seem, as we have found, to be postulated in Lotze's theory of pleasure and pain. Another expression of this is as follows: ("Microcosmus," II. Chap. V.) The soul in the course of its varied experiences has the capacity "to realize in pleasure and pain the worth that they have for it, in that they now excite it in the direction of its own nature, and now impress upon it forms and combinations of condition that go counter to the natural course of its activities." On our theory it is not quite cor-

rect to say with Bouillier ("Du Plaisir et de la Douleur," Chap. III.): "Cette tendance fondamentale à persévérer dans l'être, ou cette amour essentielle de soi-même, voilà en effet d'où nous vient tout plaisir, comme aussi toute douleur." What we love, is rather what we are becoming than what we are, although what we hate, to be sure, is dissolution. Like Fechner's theory of stability ("Vorschule," XLIII.), our hypothesis of habit bases pleasure upon repetition: yet a difference of importance in the conceptions used is that while a habitual process is one that is repeated, a stable process is one that repeats itself. The writer realizes, nevertheless, that he has been more influenced in his study of this subject by this suggestion of Fechner's than by any other idea, and hopes that "ein Kern des Richtigen" may be found in both hypotheses. If in the principle which Mr. Spencer makes the foundation of his evolutionary theory of pleasure and pain, viz. that animals tend to perform whatever activities are pleasurable to them ("Psych." I. Section 125), we take the word activity in a wide sense, as bodily change in general, the assertion becomes, according to the present hypothesis, in a measure, the converse of the truth of the matter. This is that whatever activities animals tend to perform are in the direction of pleasure to them. A like principle of the dependence of habit on pleasure (and not pleasure on habit as in our hypothesis) is thus stated by Mr. Hodgson ("Theory of Practice," Book I. Chap. I. Section 2): ". . . pleasures and pains stand to actions and consequent habits, in the relation of cause to effect, so that, in studying pleasures and pains we are studying actions and habits at their source, and in studying actions and habits, we are studying pleasures and pains in their stream." If we take activity, again, to mean bodily change in general (which undoubtedly it does not in this passage), this assertion is another expression of the converse of the hypothesis here proposed, and becomes a formulation of it, by giving each member of the pairs of words, cause and effect, stream and source, the other's place. Understanding by the term habit motor phenomena only, these statements are not the converse of the theory here proposed, but express a principle of narrower scope.

§ 76. This theory basing pleasure on habituation, and pain upon dehabituation, has been in the writer's mind in forming all the special hypotheses advanced in the course of the foregoing survey of the field of pain and pleasure. Bodily functions being fixed habitudes, would not in general contribute pleasure to consciousness unless when exceptionally intense, (e. g., our occasional feelings of exuberant health, particularly in youth: Mantegazza: "Fis. del Piacere," Chap. II.); or

when the system were re-creating itself (joy of function after privation, physical bliss of convalescence). In so far as physical pain is the result of consciousness of stimuli extraordinarily great (intense or extensive), their explanation on this theory involves the conception of a resulting large interference with nervous functioning. Of the physical basis of forms of pain apparently involving no great bodily disturbance (qualms, twinge of neuralgia), we have no conception to confront with any psychological theory of the disagreeable. (Yet it is natural to think of a neuralgia as physically a disordered process: "molecularsturm"—Du Bois-Reymond.) Present views as to the nervous basis of other forms of sensation are likewise too vague to afford decided evidence. There is little foothold for such a conception as that of regularity and irregularity of vibration beyond the sense of hearing: and the propriety of its application even here is not as yet made clear. We have already noted (Section 40f.) the wide applicability of the ideas of hindered and successful process to the explanation of pain and pleasure in the ideal sphere. Among morbid conditions, it is apparently the quickened and intensified performance of mental function (*e. g.*, association) with what it signifies in the body as a whole that is the cause of the bliss of mania, and its morbidly halting and interrupted performance with the general bodily hinderment accompanying, that produces the misery of melancholy. A possible explanation of the tendency toward pleasure of the states above called onirotic is, as noted, that the brain activities therein are isolated (and therefore uninterrupted), and intensified (and therefore pleasurable), following-out of psycho-physic habitudes, often such as are brought into the focus of consciousness only in those exceptional states. Narcosis, we can say, lets us know what heights the pleasures of creation may reach when the pains of dissolution no longer, as in common life, prevent their summation.

§ 77. The principle assumed by Mach ("Die Symmetrie," 1872), that the repetition of a visual experience is in the direction of pleasure, gives an illustration of the present theory in the field of æsthetics. (An agreeable pattern emerges from the repetition of any accidental blotch on paper. Cf. Bain: "Emotions," Chap. XIV. Section 18). Thematic treatment in music, the use of typical forms in architecture are further applications of the principle. Speaking of the aqueducts of the Roman Campagna, the President De Brosses writes: "C'est fort peu de chose que chacun de ces arcades de briques prise en soi, mais vous ne sauriez croire combien en fait d'architecture la quantité de choses médiocres, soit piliers, pilastres ou colonnes rassemblées en grand nombre produit un bel

"Ciò che non muore e ciò che può morire
Non è se non splendor di quella Idea
Che partorisce, amando, il nostro Sire."
(Dante: "Paradiso," XIII. 52.)

With these attributes of foundation of reality and source of joy, the conviction of Christendom has combined a third, that of arbiter of right. In the doctrine of the Trinity may be seen an expression of faith in the personality of Deity. For a person is resolve and feeling and idea ; and while these three are one—his resolve, his feeling and his idea—yet resolve is not feeling, nor feeling idea, nor idea resolve.

THE NEW LIFE: A STUDY OF REGENERATION.

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INTRODUCTION.

In its best estate, theology was the best expression of the highest interests and needs of man. In it all other sciences culminated, bringing to it their maturest thought, their deepest insight and their largest generalizations. It has been a passion, has given the will its strongest impulses, and has illuminated lowly and untaught minds as nothing else has ever done. It sought to express the entire religious consciousness, and used all Scripture as man's great text-book in anthropology. Under its influence society has been transformed and our modern institutions of state, church, family and school have arisen.

As all admit, theology has lapsed from its high functions. It no longer expresses its highest aspirations nor reflects the profoundest insights into Scripture, nature and the human heart. It has become so conventionalized and rigid that low views of the Bible prevail, that science and faith are no longer unified, that religion itself has often fallen into disrepute and is losing its hold both upon the masses and the cultured classes.

This condition of affairs is apparent to one who can read and understand the "signs of the times." It finds expression and emphasis in the modern movements "to reach the masses;" in the spirit which underlies the "higher criticism;" in the reconstruction of long established creeds, and in the growing demand for a "Christianized" theology. The present, however, is not so much an occasion for alarm as for inspiration and hope. To many minds there is imminent peril in the increasing decay of faith. But the fundamental truths and doctrines of religion are called in question not by reason of any weakness or irrationality in the truths themselves, but because as conceived and stated in the traditional theological systems, they do not answer the changed conceptions of God and man.

The conception of growth and development which has revolutionized and given new life to other sciences, has hardly gained a foothold in theology. Hence the constant, stubborn but vain endeavor to keep alive and make men satisfied with doctrinal statements which are formulated according to an anthropology and psychology no longer accepted, and which breathe a moral and intellectual atmosphere that lacks the life-giving properties needed by man in his present stage of development. It is asking intelligent, rational beings to accept a conclusion, while at the same time denying the premises. But others, in this "night of fear," "hear a deeper voice across the storm," and think they see a new light breaking and a new era dawning for faith and theology. I shall immediately proceed to point out one ground for this hopeful view of the future of theology and religion.

Every reform and advance in the religious consciousness has begun with new and truer views of man. Happily the department of anthropology, which is the pedagogical root and very life-spring of theology, and which is always in danger of becoming obsolete, is receiving a new, richer and deeper life. Anthropology no longer contents itself with the measurement of skulls and bones, but is devoting its best talent and energy to the study of man's mental life as it is expressed in the customs, beliefs, literatures of all races. Theological opinion on the other hand, as has been said, "has habitually moved within the limitations of particular customs, cults and religious traditions" (1).*

But this new anthropology, which gathers strength from biology, physiology and so many other sources, which culminates in psychology, is ready to offer a few ripe insights for the rehabilitation of theology, point by point. It will not only elevate theology to its original estate, but re-reveal the Bible as man's great text-book in anthropology; show that it grew slowly up out of the heart of human life, and rescue it from the winding sheet of petty interpretation, mean and unworthy glosses; save it from its friends and regenerate it as very word of very God to very man. The world hungers for Bible-truth and is given a stone. Philosophy, science, literature, school, have been robbed of the very truth and life they need. As an illustration of what is thus promised to theology and religion, I have taken a single subject and tried to show it forth in the larger light of the new psychology and anthropology. It is one, perhaps not the best that could be chosen, of many themes which need analogous treatment.

*¹ See bibliography page 104.

I try to show that so far from being an arbitrary, traditional formula, regeneration is one of the deepest needs of the human body and soul, felt among savage and civilized men of all races and times. It is an attempt to re-base the doctrine on sound anthropological and psychological principles, in the hope and confidence that it will not only strengthen the pulpit and give it greater dignity with the educated, more interest and power with the masses, but also widen and deepen the sympathy between theology and other sciences.

I have begun this dissertation with a study of the initiation rites and customs of various peoples, expressive of a new life. I then note the leading characteristics of this new physiologic-al and psychological life at puberty and adolescence, with the purpose of showing both the natural predispositions to, and the need of the spiritual change which is formulated in the doctrine of regeneration.

The following is a rough classification of the ceremonies and customs which are included in the initiation rites at puberty. The list is by no means complete and exhaustive. This has not been attempted. But it is thought that the characteristic rites are mentioned which will suffice the main thesis.

This work was undertaken at President G. Stanley Hall's suggestion. Throughout its preparation, his supervision and counsel have not only lent material aid, but also have been a constant stimulus and source of sincere enthusiasm.

Dr. A. F. Chamberlain, lecturer on anthropology in Clark University, has rendered valuable assistance in introducing me to the various authorities, in the arrangement of the material and many important details.

INITIATION RITES AND CEREMONIES AT PUBERTY.

I. Circumcision.

It is by no means a distinctively Jewish rite. It is found to be such a primitive and widespread practice that the questions, "Where was its original home?" and, "Did Abraham first become acquainted with it in Egypt?" are no longer seriously asked by those who are much acquainted with the religious and political ceremonies of primitive peoples. Andree says, "Die Beschneidung des männlichen Gliedes," belongs to that custom which extends over the whole world, and is by no means the special characteristic of a single people. (2) s. 53. "This, generally regarded as a distinctive mark of the Israelites, is by no means peculiar to them, did not originate with them, and is found in so many parts of the world with such evidences of great antiquity as to con-

travene its attribution to them. Its origin is a subject of great dispute. As practiced indiscriminately in infancy it may, perhaps, be a surgical blunder. It is certain that it is not at first among the Israelites a religious rite. . . . It afterwards was regarded as an initiatory ceremony, and as such its parallels may be found all over the world, but as a special national distinction the declared object was not accomplished. Besides the Egyptians, Arabs and Persians, the coincidence with whom might be expected, many tribes of Africa, Central and South America, Madagascar and scores of islands of the sea, show the same mark, and it has even been found in several of the North American tribes." (³) p. 29. Dr. Brinton doubts whether true circumcision as practiced by the Jews was found in America, though various mutilations of the prepuce certainly were.¹

In the Old Testament it bears marks of a later development. It had lost its significance as an initiation ceremony, and instead of taking place at the marriageable age, it assumed the dignity of a consecration ceremony of the young child to God. This illustrates a general truth that "as manners become less fierce and society ceases to be organized mainly for war, the ferocity of the primitive ritual is naturally softened, and the initiation ceremony gradually loses importance and ultimately becomes a mere domestic celebration, which in all its social aspect may be compared to the private festivities of a modern family when a son comes of age, and in its religious aspect to the first communion of a young Catholic." (⁴) p. 310.

With the Malagasy tribes in Madagascar, of Malay origin, there appears to be very little religious significance attached to circumcision. It is a rite by which children are "made men." None but those who have been subjected to this treatment can become soldiers or in any way fit for government service. After the child has been measured and sprinkled with water the following is repeated: "The lad is not a child. He is a man breasting the stream; not caught in crossing, not taken in a net. The lad is a banana tree north of the town (*i. e.*, the leese side sheltered from the prevailing southeast winds). The lad is not a child. He is a bird upon a rock, thrown at, not hit. His money fills a large tomb (repository). His slaves crowd his country house." (⁵) p. 217 *seq.* Livingstone says that among the Bechuana and Kaffir tribes south of the Zambezi, circumcision "is a civil rather than a religious rite. All the boys of an age between ten and fourteen or fifteen are selected to be

¹ Personal letter, April 21, 1893.

companions for life of one of the sons of the chief. They are taken to some retired spot in the forest, and huts are erected for their accommodation. The old men go out and teach them to dance, initiating them at the same time into all the mysteries of African politics and government. Each one is expected to compose an oration in praise of himself, called a 'lima,' or name, and to be able to repeat with sufficient fluency. A good deal of beating is required to bring them up to the required excellency, so that when they return from the seclusion they have generally a number of scars to show on their backs." ⁽⁶⁾ p. 165. The circumcision ceremony among the Bechuanas was so important, coming every five or six years, that they reckoned their history by these events, as the Greeks did by their Olympic games. ⁽⁷⁾ p. 423.

The Kechuas, one of the semi-civilized tribes of Peru, wrap the boys and maidens in leather garments after the operation has been performed, the head only being left bare. Then their relatives pour a lot of fresh milk over their heads and bodies and they are received among the adults of the tribe. Each boy is given two or three oxen by his parents, and forming a company, the boys go off by themselves into some hiding place in the forest and there feast "until they are tired of eating and idleness and are grown fat." ⁽⁸⁾ II. s. 438. For further evidence of its use in Africa, see ⁽⁹⁾, ⁽¹⁰⁾, ⁽¹¹⁾, ⁽¹²⁾. The latest testimony on this subject is that of James MacDonald. ⁽¹³⁾ pp. 99-122. He states that "the life of an African properly begins at puberty. The rite of circumcision is general." Circumcision is very generally practiced in Australia. Ploss says "it is regarded by many Australian tribes and Polynesian peoples as a sacred rite and symbol of manhood." ⁽⁸⁾ II. s. 421. "The rite of circumcision is practiced throughout a great portion of Australia, and is one of the sacred ceremonies by which the young males are in many tribes admitted to the privileges of manhood, the chief of which is the right to marry." ⁽¹⁴⁾ I. p. 159. See, also, A. Bastian ⁽¹⁵⁾. With the Turks, circumcision takes place between the eighth and thirteenth years. ⁽²⁾ s. 56. The Mohammedans of the Malay Archipelago usually perform it from the eighth to the twelfth year. ⁽²⁾ s. 57. The Mohammedan Malays of Sumatra at the same age, and the Malays of Celebes in the fifteenth year. ⁽²⁾ s. 57.

Closely allied to circumcision are other operations in the process of "making young men and women," too obscene to be scarcely mentioned. The Hottentots and Kaffirs cut out the left testicle. Strabo mentions this custom among the Egyptians. The Bedschas of northeast Africa remove the right testicle. The same custom prevails in the Caroline

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ds. (¹⁶) s. 189. The Australians of Peake River celebrate the advent of puberty in girls by piercing the hymen with the finger. (⁸) II. s. 376. The Totonacs of central America performed the same operation in the sixteenth year. s. 189. Schomburgk describes the terrible practices of natives of Peake River, Charlotte Waters and Alice Springs. These ceremonies are performed once a year upon the boys and girls who have shown signs of puberty. (¹⁷). For further evidence of these practices see (¹⁸) I. s. 145-163. (²⁰), (²¹).

Knocking out Teeth.

This custom is quite general in Australia. The Unallaks knock them out of some of the males when they are eighteen years old. (¹⁴) I. p. 272. In eastern Australia (in the quarrie districts), at some period during the summer months, the "mysteries" are celebrated and peace is declared among all the tribes. The boys are brought into the presence of the assembled people and their teeth knocked out by pushing the head against a stick fastened at one end into the trunk of a tree." If a boy shows any sign of pain he is killed on the spot. Then long cuts are made with sharp spears upon his back and shoulders. If all this is not endured calmly, they declare that he is not fit to mingle with the men of the tribe. The women derisively call him one of the "number." If he endures the tortures without moving he passes into the degree of warriors and hunters. They give him a piece of crystalline substance, which is kept secret from the women, and present him with a shield and warrior's arms. II. s. 415-416. In the eastern part of South Wales a similar ceremony takes place. During the feast, with dancing and singing, each of the boys is taken by a man and carried on his shoulders. Then a tooth is broken out and the blood from the gum runs down the breast of the boy and is put in the head of the man who carries him. Then the boy is taken to his relatives, who give him a girdle with a sword and crown him with a wreath of leaves. I. s. 413 seq. Frazer thinks that the practice of knocking out the upper front teeth at puberty is or was once, probably, a rite of initiation into the totem. "The Batoka in Africa say that they do so in order to be like oxen, while those who retain their teeth are like zebras." (²²) p. 28. Connected with this extended primitive rite are the customs of filing and boring the teeth.

Hair Offering.

Shaving the head or cutting off part of the hair is a com-

mon practice. The Indian tribes in British Guiana shave the heads of the girls at puberty, and the Caribs burn the hair off of the girls and then make deep cuts from shoulder to shoulder and rub in pepper, and the girl must not utter the slightest cry of pain. ⁽⁸⁾ II. s. 425-426. Curr gives a description of the rough treatment which the youths of Narringairs, in Australia, receive. The hair grows uncut for two or three years before puberty, which usually begins about the age of fourteen. Then they are taken by the men and their mustaches and hair of their bodies pulled out, and the hair of their heads torn off in handfuls. They must fast three days, drinking only water, and must not sleep during that time. They must submit to the same treatment three times at intervals of about two years, and then they can marry. ⁽¹²⁾ I. pp. 254-255.

In Japan the godfather cuts off the forelock near the age of fifteen and gives the boy a new name. The Spartans let the hair grow as soon as the boy reached the age of the ephebi while up to that time it was cut short. After the Persian war they cut off the long hair of the boys when they arrived at the age of the ephebi and devoted it as an offering to a god.¹ W. Robertson Smith says: "Among the Arabs in the time of Mohammed, it was common to sacrifice a sheep on the birth of a child and to shave the head of the infant and daub the scalp with the blood of the victim. This ceremony, called 'acica,' or the 'cutting of the hair,' was designed to 'avert evil from the child,' and was an act of dedication by which the infant was brought under the protection of the god of the community. Among Lucian's Syrians, on the other hand, the hair of the boys and girls was allowed to grow unshorn as a consecrated thing from birth to adolescence, and was cut off and dedicated at the sanctuary as a necessary preliminary to marriage. . . . The same thing appears to have occurred, at least in the case of maidens at Phœnician sanctuaries; for the female worshipers at the Adonis feast of Bylus, who, according to the author just cited,² were required to sacrifice either their hair or their chastity, appear from other accounts to have been generally maidens, of whom this act of devotion was exacted as a preliminary to marriage.³ I apprehend that among the Arabs in like manner the 'acica' was originally a ceremony of initiation into manhood, and that the transference of the ceremony to infancy was a later innovation, for among the Arabs, as among the Syrians,

¹ *Life of the Greeks and Romans.* Guhl and Koner, p. 172.

² Lucian, "Dea Syria," VI.

³ *Sozomen*, V. 10.7. Herodotus, 1.199.

young lads let their hair grow long, and the sign of immaturity was the retention of the side locks, which adult warriors did not wear." (4) pp. 310-312.

IV. *Tattooing.*

In some of the Philippine islands, at the age of twelve, the boys and girls are tattooed in various figures upon the arms, breast and legs. The skin is first tightly drawn, then cut with a semi-circular knife and some soot rubbed in. (22) The women of Murray submit to the operation of having their backs cut with stones or shells crosswise from the right to the left side. This operation, though very painful, is submitted to willingly, for a tattooed back is much admired. In New Zealand the young man is declared marriageable by tattooing him at sixteen. (8) II. s. 417 *seq.* Among the Belladong or Bellerdocking tribe, the chests, foreheads and thighs of the youths, when their beards are grown, are burnt with heated stones. Very often it is a figure representing some animal or the totem of the tribe which is made upon the body. In this way it is believed that one is placed more securely under the protection of the totem. Many of the North American Indians seem to believe that they have an animal in their bodies. (23) p. 26.

V. *Piercing the Septum* of the nose is a very common mutilation. It is the custom of almost every tribe in Australia. Very often a reed, bone, feather or bit of wood is worn in the opening. (14) I. pp. 71, 164.

VI. *Fasting.*

In Australia many tribes have regulations against the use of certain kinds of food by the boys after they are eight or ten years old. (14) I. pp. 71, 72. Dr. Boas, speaking of the Indians in British Columbia, says: "Girls, even before reaching puberty, must not eat parts of fish near the head, but only tails and adjoining parts, in order to secure good luck in their married life. On reaching maturity they have to observe numerous regulations. They must eat only dried fish and may eat clams. Gooseberries and crab apples are forbidden, as it is believed they would injure the teeth. At Victoria, the girl when reaching puberty must take some salmon to a number of large stones. This is to make her liberal." (24) p. 22 *seq.* Those who were initiated into the second degree of the Eleusinian Mysteries, which was administered at the age of manhood, had to abstain from several articles of food. (25) p. 287 (note). Before and during the Mysteries they could eat no flesh of chickens and

fish, neither beans, apples nor pomegranates. With the Andamanese there is a fast required of the boys and girls before puberty. "The fasting period (during which turtle, honey, pork, fish and a few other favorite articles of food are forbidden) commences between the eleventh and thirteenth year and varies in length from one to five years; it is observed by both sexes, but lasts longer in the case of girls, with whom, indeed, it is not terminable till some time after matrimony. . . . It does not rest with the youth or maiden to determine when he or she will resume eating the various articles above mentioned, but with the chief, who decides when each individual's powers of endurance and self-denial have been sufficiently tested. . . . As at present understood, the fasting period is regarded as a test of the endurance, or, more properly speaking, of the self-denial of young persons, and as affording evidence of their fitness and ability to support a family." When the fast is declared off, and the neophytes allowed to resume eating the dishes they have been deprived of, among other ceremonies, dancing and singing, their bodies are smeared by the chief with honey and melted turtle and pork fat. (²⁶) pp. 61-67, 133-135.

VII. *Seclusion.*

In connection with the practice of fasting, it may be observed that isolation or separation of the youths and maidens from the other members of the community is a very common mode of treatment. With the Australians the boy at eight or ten years of age must leave the hut of his father and live in common with the other young men of the tribe. He is called by another name than that which he has borne from birth, and his diet is regulated to some extent. (¹⁴) I. pp. 71-72.

The period of isolation varies very greatly. The boys of the Goulburn tribes in North Melbourne, who are to be consecrated to manhood, are carried into a forest, where they stay two days and one night. Meanwhile they must knock out two of their upper incisors and give them to their mothers on their return home. (²⁷) p. 201. In northern Guinea the "novice" is shut up for eight days, and receives food once a day from a slave. At the end of this time masked men take him and make numerous tests of his courage. (²⁸) p. 420. Among the Quojas in Africa, "the boys after circumcision are carried by force into the woods. There they remain a year and are instructed by the older members of the community—the 'Seggone'—in civil government and military science. The tests to which these boys are subjected they call 'Belli-Paato' or 'Belli-Paaro.' Its meaning is explained by the

Quojas in the following way: 'It is a death and a new birth (*eine Wiedergeburt*), since they are wholly changed in the consecrated thicket, dying to the old life and existence and receiving a new understanding.' When the youths return from the thicket they act as if they had come into the world for the first time, and had never known where their parents lived or their names; what sort of people they were; how to wash themselves." (²⁷) s. 199-200.

In New Ireland, one of the New Britain group, the girl is placed in a small conically shaped structure, made from the leaves of the pandanus tree. In this small, dark enclosure she is obliged to lie down or sit in a crouched position on a platform of bamboo sticks, four feet from the ground. She can come out only once a day to bathe. Girls are often confined in these cages while quite young and must remain there until they are of a marriageable age, so that their imprisonment often lasts four or five years. All this time they must not touch the ground with their feet, for it is "tabooed." (²⁹) pp. 281-294. The negro girls of Loango at puberty are confined in separate huts and they must not touch the ground with any part of their bare body. (³⁰) s. 23, (³¹) II. p. 226. "The heir to the kingdom of Sogamoso in Colombia, before succeeding to the crown, had to fast for seven years in the temple, being shut up in the dark and not allowed to see the sun or light." (³¹) II. p. 226. In Bogota (Colombia) the prince "had to undergo a severe training from the age of sixteen; he lived in complete retirement in a temple, where he might not see the sun nor eat salt, nor talk with a woman." A magician takes the boys of the inhabitants of Rio Nunez into a forest after the ceremony of circumcision. They live in huts covered with tree-limbs. They must be ransomed before they can return to their native place. (²⁷) s. 199. The girl of the South American Indian tribe Macusi, who has reached puberty, must live in the attic of the hut apart from the other members of the household. When she has fasted seven days she can make some broth for herself. Her playthings are broken up. Then she is bathed. Her mother beats her with sticks during the night and she must not cry loud enough to wake up those sleeping under the same roof. At the second flow of the menses another scourging is inflicted. Then she is regarded pure and can be taken by the bridegroom. (²⁷) s. 197. With the Huron, Iroquois and Algonkin Indians, the boys at puberty are placed in charge of an old man and the girls of an old woman. They must fast rigorously and a careful observance is made of all their dreams. Then they are placed under a tutelary divinity, who has care of them through life. The Ojibway boys about to enter man-

hood must build a hut in the spring near some high tree and "there remain lying upon moss, fasting many nights until the pangs of hunger and thirst are no longer felt, and the soul becomes free. The soul stays in heaven during the sleep and there knowledge of life is revealed." (²⁷) s. 195. A practice strikingly similar to the one just observed among the Quojas is that of the Virginian Indians. After a very severe beating the boys are thrown into a secluded spot. There they must stay nine months and can associate with no human being. They are fed during this time with a kind of intoxicating preparation of roots to make them forget all about their past life. After their return home, everything must seem strange to them. In this way it is thought that they "begin to live anew." They are thought of as having been dead for a short time and are "numbered among the older citizens after forgetting that they once were boys." (²⁷) s. 195. The Californian Indians burn into their flesh the figure of the beast seen by the boys during an intoxicated state which is produced by a similar drink. (²⁷) s. 196. The lads of the western tribe of the Torres Straits Islanders undergo a month's "isolation in the bush," separated from any woman and their own fathers. A relative attends them and teaches them the customs and morals of the people. "This was followed by a great feast, when the lad was presented to his relatives gayly ornamented and thenceforth he took standing as a man." (³²). In New South Wales "the novice is not permitted even so much as to look at a woman or to speak to one during the initiation period; and even for some time after he must cover his mouth when one is present." (²²) p. 43.

VIII. *Change of Name.*

As the youth at puberty leaves the family or domestic circle, preparatory to becoming a member of the tribe, he is often obliged to give up his family name for one which has significance with reference to his new standing as a full-fledged citizen. This, as has been observed above (p. 69), is a widely prevalent custom with the Australian tribes. The boys belonging to the Noeforeze in New Guinea are given a new name about the twelfth year. (⁸) II. s. 423. Among the other things and surroundings of childhood which the young man must forget is his name. MacDonald says, "It is a terrible way to tease a Wayao, to point to a boy and ask him if he remembers what his name was when he was about the size of that boy. Some would not mention their name for any consideration." (³³) I. p. 128.

IX. Beating or Torture.

It would seem that torture was a predominating element in almost all the practices mentioned. And yet it appears that beating in one form or another is often but one of a series of tests which the youth must undergo in the initiation process.

Livingstone, in describing as an eye witness the second part of the ceremony among the Bechuana and Kaffir tribes, says: "Just at dawn of day a row of boys of nearly fourteen years of age stood naked in the 'kotla,' each having a pair of sandals as a shield on his hands. Facing them stood the men of the town in a similar state of nudity, all armed with long thin wands of a tough, strong, supple bush, and engaged in a dance named 'koha,' in which questions are put to the boys as, 'Will you guard the chief well?' and while the latter give an affirmative answer the men rush forward to them and each aims a full weight blow at the back of one of the boys. Every stroke makes the blood squirt out of the wound a foot or eighteen inches long. At the end of the dance the boys' backs are seamed with wounds, the scars of which remain through life. This is intended to harden the young soldiers and prepare them for the rank of men. After this ceremony and after killing a rhinoceros, they may marry a wife." (³³) I. pp. 131-132. (⁶) pp. 164-165. "Likewise the young women are drilled under the surveillance of an old lady. They are clad all the time in a dress of alternate pumpkin seeds and bits of reed strung together and wound round the body in a figure '8' fashion. They are inured in this way to bear fatigue and carry large pots of water under the guidance of the stern old hag. They have scars from bits of burning charcoal having been applied to the forearm, which must have been done to test their power of bearing pain." (⁶) p. 167. The severity of these scourgings is well illustrated by the methods of the Indians of North Mexico. At the age of puberty the chief seizes the boy by the hair, throws him down and strikes him with his fist. If he smiles in return for this and appears fresh and active, he is ready for the second course of treatment, which consists in scourging his whole body with sticks and thorns until the blood flows. If he shows no sign of pain he is submitted to the third test. With the claws of birds of prey his bare body is hacked and torn. Amidst all this torture he must present a fresh and self-controlled appearance. The slightest expression of pain would pronounce him unfit for war. At the close of these three tests the youth is presented with a bow and arrow, and he is told that he "must never be timid that he and his people only are men, and must consid

overestimated and made to do too much in the way of interpretation of the practices of various people. Jacobs' own view will be mentioned later.

II. The shedding of blood has a deeper meaning than the one sometimes mentioned, viz., to make the youth accustomed to the sight of blood as a preparation for the warrior's life. In Australia during the ceremonies, a godfather, who stands for the life into which the youth is to be initiated, is selected for each candidate. He opens a vein in his own arm and the youth drinks his blood. "After this the lad drops forward on his hands and knees and the sponsor's blood is permitted to form a pool on his back and to coagulate there. Then the sponsor cuts with his stone knife broad gashes in the lad's back and pulls open the gaping wounds with his fingers. The scars of these gashes remain as a permanent sign of the covenant ceremony." ⁽³⁵⁾ Appendix, p. 336. This is similar to the practice already mentioned in New South Wales, of letting the blood from the wounded gum of the youth fall upon the man who represents the tribe. There is a ceremony among the Caribs of admitting the youth into the life of the clan, where the father of the youth takes a live bird of prey of a particular species, the totem of the tribe, and beats his son with it till the bird is dead and its head crushed, thus transferring the life and spirit of the bird to the future warrior. Further, he scarifies his son all over, rubs the juices of the bird into the wounds and gives him the bird's heart to eat. ⁽²²⁾ p. 45.

It seems to me that in these ceremonies there is a sort of blood-covenant idea. Trumbull has shown how widespread this practice is. The union by means of blood, whether between men and gods or between man and man, has been universally regarded as the strongest and most sacred tie, closer than that of birth: "There is a friend which sticketh closer than a brother."¹ It would seem that the covenant which Abraham is represented as having made with God, (seventeenth chapter of Genesis), was of the nature of a blood covenant, the mark of which covenant he bore in his flesh. And in these initiation ceremonies, the bond which bound the youth to the life of the tribe, which made him one with the community, was made and sealed by this closest, most lasting and most sacred means.

III. Very closely connected with this blood-covenant conception is the sacrificial element in these ceremonies; for, as W. Robertson Smith maintains, "the fundamental idea of ancient sacrifice is sacramental communion." ⁽⁴⁾ p. 418.

¹ Prov. 28:24.

The early Semitic peoples had their animal sacrifices, in which the god and his worshiper united by partaking together of the flesh and blood of the sacred victim. ⁽⁴⁾ p. 209. By some ancient peoples human sacrifice was regarded the only worthy sacrifice. It was practiced not only by the Aztecs and African negroes, but by those people of proverbial superior civilization, the Greeks and Semites. Witness the story of Jephthah¹ and the offering of Isaac by Abraham. The significance of the narrative in the twenty-second chapter of Genesis is not to show the faith of Abraham, but the revelation to a man living in a crude, primitive civilization that there is a substitute for human sacrifice acceptable to the deity. Human sacrifice appears to have been a universal practice, but was superseded by various rites, such as flagellation, mutilation of some essential part of the body or emission of a certain quantity of blood. ⁽³⁶⁾ p. 84. Tylor says: "Offering a part of the worshiper's body is a most usual rite. . . . Various rites of finger-cutting, hair-cutting and blood-letting are no doubt connected with sacrifice. They belong to an extensive series of practices due to various and often obscure notions which come under the general head of ceremonial mutilations." ⁽³⁷⁾ II. p. 363. In these barbarous initiation ceremonies death often resulted. But in every case there is a loss, a giving up of something — the blood, hair and other parts of the body regarded most precious. J. P. Trusen says in "Die Sitten, Gebrauche und Krankheiten der alten Hebräer," s. 124, "It is not improbable that Abraham considered that the giving up of the whole body was substituted by the offering of the noblest part." Quoted in ⁽¹⁶⁾ s. 245. The conclusion which Jacobs reaches is that "the original signification of circumcision was the bringing of an offering to the deity, from which the people imagined that the life-giving or animating power proceeded, and such is its meaning among most savage peoples." ⁽¹⁶⁾ s. 255. "In their origin the hair-offering and the offering of one's own blood are precisely similar in meaning. But the blood-offering, while it presents the idea of life-union with God in the strongest possible form, is too barbarous to be retained as an ordinary act of religion. It continued to be practiced among the civilized Semites by certain priesthoods and societies of devotees; but in the habitual worship of laymen, it either fell out of use or was retained only in a very attenuated form, in the custom of tattooing the flesh with punctures in honor of the deity. The hair-offering on the other hand, which involved nothing offensive to civilized feelings, continued to play an important

¹ Judges, 11th chap.

part in religion to the close of paganism, and even entered into the Christian ritual in the tonsure of priests and nuns." (6) p. 118.

IV. Another characteristic of these ceremonies is that they tested the courage and endurance of the subject. Infanticide, so common with savage peoples, may be largely accounted for by the fact that the infant soon after birth is subjected to a kind of treatment which often results in death. It is believed by many savage peoples that only the fittest should survive. Infants who are unhealthy, crippled or in any way deformed would only be a burden to the community. None but those might live and be cared for who could survive certain tests. Hence the bathing of the young child in snow or water, or exposing him in some way. This practice of exposure we may believe was prevalent in ancient Greece. The story of *Oedipus* exposed on the lonely mountain may have been suggested by this practice, and probably the story of *Moses* hid in the bulrushes is founded on a similar custom. At puberty there is a kind of repetition of these practices of infancy. Some writers explain the custom of knocking out teeth as an imitation of nature's process. As in passing from childhood to youth the milk teeth are shed, so in the transition from boyhood to manhood the people take the place of nature and knock out the teeth. (27) Those who had arrived at the age of manhood and were to be qualified as citizens had to prove their ability to fulfill the obligations of the new life. Only those who could endure certain tortures and ordeals and display courage and strength are fitted for the duties of manhood. Therefore we are not so much surprised at the willingness, in most cases, of the youth to submit to various ceremonial operations. Halévy says that in South Arabia the boy who is not willing to be circumcised, the first act of manhood and a consecration to the warrior's life, is looked upon as a coward. (8) II. s. 436. The young men of the *Dajaks* regard it an honor to be selected for the operation. These testing, hardening methods seem to have their survivals in many forms. The Germans used to test the child's courage by putting him on a sloping roof; "if he held fast, he was styled a stout, brave boy." (8) II. s. 448-9. The "*Abhärtung*" process was especially emphasized in the Spartan system of training. The youths had to go with scant clothing and food, sleep on hard beds, submit to severe punishments for transgression, and undergo the yearly torture tests and scourgings at the altar of *Artemis*. No matter how severe the whippings, death often resulting, it was regarded ignominious to show pain or beg for mercy, and the boys who could hold out the longest were praised as

victors. (²⁷) s. 201.¹ The educational methods in the middle ages were very severe, as we may learn from Augustine's statement: "I was put to school to learn lessons in which I (poor wretch) knew not what use there was; and yet, if idle in learning, I was flogged. For this method was commended by our forefathers; and many passing the same course before us framed for us weary paths, through which we were compelled to pass, multiplying toil and grief upon the sons of Adam."² At a school in Paris, where Erasmus spent his youth, we have the following observation, made about 1496: "The bed was hard, the meat so bad and scanty and the work so difficult, that many of the most gifted youths died during the first year of their stay there or became blind, insane or leprous. The discipline ended by flogging." (²⁷) s. 202.

V. But these initiation ceremonies are something more than mere tests of courage and endurance. The recognition in so many different ways and by almost every race, of the transition from youth to manhood, from the narrow domestic circle to membership in the community, has a deep psychological as well as a physical significance. The boy as a member of the family, supported by others and feeling almost no responsibility, when becoming a man enters upon a new kind of life. He must now not only assume the care of himself, but must work for the good of the whole community. And the way which these simple, crude people adopted to impress him with the significance and sacredness of this new life, was to put him through a series of ceremonies. In the minds of these peoples there was a fixed gulf between the life of manhood and that of childhood, and he who would become a man must put away "childish things." He must cut himself aloof from the things which interested him in his early days, even his own relatives. It was indeed a *dying* to the former life. Everything that might serve as a reminder of the old life must be scrupulously avoided. Kulischer thinks that we find a relic of this primitive practice in the school system of the middle ages. All the sciences were taught in the cloisters and in the Latin language. The boys and girls had to live in the cloisters in order to get their education. There they learned Latin and Greek and became wholly estranged from the life of childhood. There was a breach between school and life. The object of the cloister training seems to have been, not to prepare the pupil for life, but to make him a "new creature." (²⁷) s. 203. In truth,

¹ Schoemann. Griechische Alterthümer, I. s. 266-267.

² Confessions, Bk. I. Ch. IX.

we may say that the psychology of initiation into the various societies and organizations at the present time was in principle at least the psychology of initiation into manhood. "There seems in the mysteries of savage races to be two chief purposes: there is the intention of giving to the initiated a certain sacred character, and there is the introduction of the young to complete manhood and womanhood." (³⁸) I. p. 281. These ceremonies, barbarous and revolting to us, are so real and sacred to these people that they can not but deepen the sense of the change of life and make a lasting impression on the character of the subject. Those initiated into the Greek Mysteries "received impressions that they might be put into a certain state of mind."¹ Furthermore, the initiated carried on their persons various badges and signs, and in many cases they literally bore in their flesh the marks of their manhood. This badge might be the mark of circumcision, or the scar of the totem, or any other mark tattooed on the body. Some tribes decorate themselves with leaves, feathers, leopard's teeth, pieces of coral or some charm, as token of their new relation. The girls of the Szuaheli in Zanzibar at puberty, after their bath, have their hair gorgeously dressed and ornamented and are led about the town, "seemingly to show that they are marriageable." (⁸) II. s. 437. "The Nicobarese not only flatten the occiput of children in infancy, but from the period of puberty blacken their teeth and perforate the lobes of their ears to such an extent as to enable them, by the time they are full grown, to insert a wooden cylindrical instrument three-quarters of an inch thick." (²⁶) p. 115. A cap for the boys and a kind of head-dress for the girls are insignia of manhood among the Chinese; while the "toga virilis" was assumed by the Roman youths at the beginning of manhood. The Greek ephebi at about the age of sixteen were initiated into citizenship by a solemn service and sacrifice, and were given a short, dark gray cloak and a broad-brimmed hat.

Interesting as are the various interpretations of these rites and ceremonies, nevertheless the point of most vital significance is the fact itself of the widespread celebration of a particular period of life. In primitive religions, practice is everything, and the explanation of it goes for very little. As long as one strictly observes certain rites and conforms to certain regulations, he is not bound to give a reason for the faith that is in him, and is not persecuted if his interpretation of the common act is the very opposite of that of another

¹ Lenormant. "The Eleusinian Mysteries." *Contemp. Rev.* Vol. XXVIII. 1880.

the menses begin to flow. A prophet of Israel has thus described the outward characteristics: "Thy breasts were fashioned and thine hair was grown."¹ This change at puberty has been called a second birth. "It is, as it were, a second birth; one less rapid and less violent than the first, but which instead of surprising an organism in a state still apathetic, hardly conscious like that of the fœtus, surprises a being intelligent, sensitive, impressionable, and knowing to a certain extent how to observe himself and analyze what he feels. . . . It is a new life commencing, a new life of which nothing hitherto gave an idea and into which one enters with every apprehension of the unknown." (⁴⁰) p. 256. The same writer thus describes the peculiar physiological changes in the girl at this period of life: "Les yeux, un peu fatigués et un peu cernés de brun, sont tantôt rêveurs et voilés tantôt brillants d'un éclat presque fébrile; le regard clair, assuré, ingénu, presque animal de l'enfance a fait place à un regard expressif qui réfléchit et qui peut rendre toutes les nuances du sentiments; des rougeurs subites, des bouffées de chaleur lui montent au visage pour la moindre émotion et tout chez elle est prétexte à émotion; la voix, une fois la mue terminée, devient chaude, musicale, mieux timbrée et peut s'accommoder à toutes les inflexions de la passion; les mouvements brusques, bruyants, désordonnés, deviennent plus doux, gracieux, ralentis; l'attitude a plus d'abandon, la démarche plus de langueur et de mollesse. Le sommeil est moins calme est moins pur; il est troublé souvent par des rêves qui l'agitent et qui l'inquiètent. Les yeux qui lui plaisaient la laissent indifférente." (⁴⁰) p. 45.

In the primitive mind some of the phenomena of puberty, especially in the female sex, are associated with the miraculous and supernatural. A woman in childbed or during her courses is therefore usually tabooed by savage peoples, believing that everything connected with the propagation of the species as well as disease and death are manifestations of supernatural powers. The menstrual function was regarded by the old Persians as caused by the stars, and women were regarded unclean during their menses.² Such was the belief of the Israelites.³ That which is mysterious and out of the usual course of nature has been universally regarded as supernatural and dangerous. Hence woman at her monthly

¹ Ezekiel, 16:7.

² Zend-Avesta. Part I. "The Veudidad." Introduct. Chap. V. § 12. Ed. by Müller.

³ See, e. g., Lev. 15:19 seq.

See also, Koran, Chap. II. "Separate yourselves from women in their courses, and go not near them until they are cleansed."

periods has been kept apart from the people from fear that those who came in contact with her in any way would be defiled. "Three paces from her shall he stay, who brings food to a woman who has an issue of blood either out of the ordinary course or at the usual period."¹ The dishes in which the food was brought had to be of metal so that they could be cleansed.² With some of the Tinneh Indians, the girl, during her seclusion at puberty, must have her food served on dishes especially for her use and must not be touched by any other person. (³¹) I. p. 170. The Levitical law contained very definite regulations as to the purification of the woman herself and everything touched by her.³

"Among the Carriers, as soon as a girl had experienced the first flow of the menses, her father believed himself under the obligation of atoning for her supposedly sinful condition, by a small impromptu distribution of clothes among the natives. This periodical state of women was considered as one of legal impurity, fateful both to the man who happened to have any intercourse, however indirect, with her, and to the woman herself who failed in scrupulously observing all the rites prescribed by ancient usage for persons in her condition." (⁴¹) Frazer says that the "custom of stinging the girl with ants or beating her with rods is intended not as a punishment or test of endurance, but as a purification; the object being to drive away the malignant influences with which the girl at such times is believed to be beset." These developed later into tests of endurance. He supports this view by the fact that inanimate objects are beaten for the purpose of driving away the evil spirits from them. (³¹) II. pp. 233-234. Frazer also advances the idea, that one reason for the seclusion of girls at puberty was the deeply ingrained dread, which primitive man entertains, of menstrual blood. "The girl is viewed as charged with a powerful force, which, if not kept within bounds, may prove the destruction both of the girl herself and of all with whom she comes in contact." Amongst the Australian blacks, the boys are told from their infancy that if they see the blood they will early become gray-headed, and their strength will fail prematurely. (³¹) II. p. 238. This universal dread of menstrual blood is closely connected with the widespread fear of contact with the mysterious and sacred. If such contact is necessary the effects must be removed in some ceremonial way. The hands must be washed

¹Veedidid. Fargard XVI. I. §. 6.

²Fargard, VII. 9.

³Lev. 15.

after handling sacred things. A tribe of the Bechuanas, who regard the crocodile a sacred animal, believe that seeing the animal will cause inflammation of the eyes. ⁽⁶⁾ p. 225. The Indians have a special name, *Um-pannu-gas*, for the elk buck in the spring time, at the mating season, for they are thought to possess souls, and on no account must one be killed.¹ Other peoples think that if they eat the animal which is their totem, their bodies would be covered with boils.² But like many other things which impressed the primitive mind as mysterious, menstrual blood has often been regarded as a charm, and a panacea for various diseases. "In der Kosmographie des Arabers Zakarija ben Muhammed-al-Qazwini: 'Das Blut der Menstruation, wenn mit ihm der Biss des toten Hundes bestrichen wird, heilt ihn und ebenso knotigen Aussatz und schwarze Räude.'" Quoted ⁽⁴²⁾ s. 16. "Das Blut der Menstruation einer Jungfrau hilft gegen den weissen Flecken auf der Pupille, wenn man es als Augensalbe."

The reproductive or sexual instinct which develops at puberty is one of the most fundamental and powerful in human nature.

Krafft-Ebing says: "Sexuality is the most powerful factor in individual and social existence: the strongest incentive to the exertion of strength and acquiring of property, to the founding of a home and the awakening of altruistic feelings, first for a person of the opposite sex, then for offspring, and in a wider sense for all humanity." ⁽⁴³⁾ s. 1. Reproductive power might be called the "apperception centre," about which are clustered the religious thoughts and indeed thoughts about the most sacred and mysterious things, of many people. The soul was by some early Greek philosophers thought to be water, and Aristotle says that this conception was suggested by generative seed, which in all animals is moist. Hippo said the soul could not be blood; "for the seed is not blood and this seed may be regarded as the primary form of soul."³ In the stoic philosophy the image of reason is procreation. The universal reason as the creative force in nature was called Generative Reason or the "Spermatic Logos."⁴ Death and the reappearance of life have been the objects of world-wide ceremonies and festivities. The young girls, in the Kânagrâ district in India, in the spring marry the two deities Siva and Parvatî, who represent the spirits of vegetation. The Egyptians, Syrians, Greeks celebrated the

¹"Ploughed Under," p. 21 (note). N. Y., 1881.

²3rd Annual Rep. Bureau of Ethnology, Wash. p. 225.

³Aristotle, Psych., Wallace's Ed. p. 21.

⁴Zellar, "Stoics, Epicureans and Skeptics," p. 172 seq.

death and revival of vegetation in the feasts of Osiris, Adonis, Dionysus. And we have the modern remnant of this recognition of the fertilizing power in vegetation, in the "May King and Queen" and "May-pole" festivities. (³¹) I. p. 278. There is good evidence for believing that phallic worship also had its origin in this veneration of the life-giving or reproductive power. Some writers on this subject go so far as to maintain that every religion has sprung from the sexual distinctions. The sky has been worshiped as father and the earth as mother. The Tahitans believed that all existence came from the union of two beings. The stars were begotten by the sun and moon. In New Zealand there is a myth that there were two original ancestors of everything in the universe—the earth, which is the mother, and heaven the father. (³²) p. 26. But most writers on the subject of phallic worship seem to lose all critical sense, and everything is interpreted as symbolizing the sexual parts of man. Thus the sacred pillars and stones are phallic symbols; architecture and sculpture, ancient and modern, are governed by this distinction of the sexes. But that most of this literature is uncritical and fanciful is very apparent. (⁴) pp. 194-195, 437-438. It is but another instance of how one conception can be made to explain everything. Undoubtedly the underlying truth of this theory is the fact, that the parts of the body connected with the propagation of the species, have often been regarded sacred. The narrative in the twenty-fourth chapter of Genesis, which has its parallel in the Arabic manner of taking an oath, is based on this truth. Much more judicious than the statements that all religions are founded on the sexual distinction, we are concerned with the sexual passion, is the statement by Dr. Brinton, that "of all properties of organized matter, that transmitting form and life is most wonderful; and if we examine critically the physical basis of the labors and hopes of mankind, if we ask what prompts its noblest and holiest sagings, we shall find them in the vast majority of instances directly traceable to this generative power. No wonder that religious which spring from man's wants and wishes, very often bear the distinct trace of their origin in the reproductive function." (³³) p. 64. Enough has perhaps been said to indicate the associations of naïve thinkers of the most striking phenomena which begin to manifest themselves at puberty. But the changes which take place at the advent of puberty are psychical as well as physiological. Coincident with the functioning of new organs, and the development of cerebral centres, which have hitherto lain dormant, are profound intellectual and emotional changes. "Le système glandulaire exerce un grand influence sur le système cérébral

et cela doit être vrai surtout pour les glandes qui. . . . se distinguent par leur éminente sensibilité." (46) p. 25.

The activity of the organs, which connect the individual with the race, is accompanied by powers and instincts which affect his mental life in its various aspects and mark the beginning of a new life, intellectually, morally and emotionally. At puberty the differences between individuals as well as between the two sexes become more marked and characteristic. The plays and pastimes of childhood lose their attractiveness. "With the child, life is all play and fairy tales, and learning the external properties of 'things;' with *youth* it is bodily exercises of a more systematic sort, novels of the real world, boon fellowship and song, friendship and love, nature, travel and adventure, science and philosophy."¹

Dr. Burnham, in his "Study of Adolescence," draws the following inference from observation of individual cases: "There is at puberty a great increase in vitality and energy. This is manifested by the rapid growth at this period, by increased power of resisting disease, by the greater mental activity, and the like. The great evolution of energy and the corresponding influx of emotional vitality may objectify itself in many different ways. With some it may result merely in greater physical activity. With others it gives an impulse to intellectual work; with still others it leads to social and altruistic activity. A love affair, poetry, religious or political fanaticism, bizarre actions, general perversity and insanity are all possible outlets. The whole subject is most complicated. It involves the most profound questions of life and heredity. What the phenomena of adolescence may be in any given case depends largely upon one's general health, education, hereditary tendencies, temperament and the like." (47) pp. 181-182.

Aristotle's description of the period of youth is very apt. He says in the *Rhetoric*,² that of bodily desires it is the sexual to which the young are most disposed to give way. They are passionate, irascible and apt to be carried away by their impulses. They are charitable, trustful, sanguine, and have high aspirations. "Youth is the age when people are most devoted to their friends, relatives and companions." If they commit a fault it is always on the side of excess. But we must consider a little more in detail the psychological characteristics of this "new life," which dates from puberty and reaches maturity at the close of the adolescent period. Notwithstanding the fact that the psychology of adolescence, based upon a thorough study of a large number of individual

¹ James' *Psych.* Vol. II. p. 401.

² Bk. II. Chap. XII.

cases, has yet to be written and that the results already adduced lack sufficient data for accurate scientific purposes, yet a few and perhaps the leading characteristics may be mentioned. I am largely indebted to the article already referred to by Dr. Burnham, which not only makes a good beginning in the study of the adolescent period, but also contains many valuable suggestions for further study.

L. There is a decided awakening of the intellectual life at puberty. "Were it not that the importance of this period is often forgotten, it would be too commonplace to speak of the ardent activity of adolescents; for we take it for granted. An astonishingly large amount of the world's work has been done by them. To recount what has been done by young men before the age of thirty, would be to re-write a large part of the world's history. . . . Even when the work has not actually been done at this period, the inspiration and the stimulus came then." (⁴⁷) p. 191. The same writer says further that the incentive to philosophic thought generally comes at adolescence, and mentions the fact that Leibnitz had written several works at twenty-two; Berkeley published his "Essay on the Theory of Vision" at twenty-five, and "The Principles of Human Knowledge" at twenty-six. Hume wrote most of his "Treatise on Human Nature," as he himself says, when he was in college. Schelling published his "Ego as Principle of Philosophy" at twenty, and began to lecture at the University of Jena when he was twenty-three. "The Four-fold Root of the Principle of Sufficient Reason" was published when Schopenhauer was twenty-five. At twenty Herbart had written several philosophical essays, and at twenty-two Benike had published his first three works ("Outlines of the Science of Cognition," "Empirical Psychology as the Basis of all Knowledge" and his "Doctor's Dissertation"). Lotze's "Metaphysics" appeared when he was twenty-four, and "Jonathan Edwards, probably the greatest of American philosophers, wrote some remarkable philosophical speculations in his 'Notes on the Mind' when he was a boy of sixteen." (⁴⁷) p. 191-192. In the adolescent period, literature begins to be really appreciated and understood. (³⁰) p. 546. The reasoning faculty rapidly develops, and this period has appropriately been called "the storm and stress period" of life. In truth a new consciousness awakens; an imperative impulse is felt to readjust habits of thinking and living to new conditions. Doubts of a philosophic and religious nature are for the first time pressing, and in some cases the whole foundation upon which life has been constructed begins to totter and often falls to pieces. Creeds are overhauled, and there is an inevitable tendency to challenge authority

of whatever nature and question early convictions with almost heartless zeal.

II. There is a decided change in the moral life at puberty. Altruistic feelings begin to develop, and there is a decided inclination towards persons of the opposite sex. Youth is above all the period of most intimate friendships. Self-centered interest yields to the instinct of devotion to some object or person. Plato puts in the mouth of Agathon in the *Symposium*: "Youth and love live and move together." A recent writer has said: "It is in order for youth to take hold of a thing impetuously, to create for itself an ideal, to be full of enthusiasm for noble deeds and noble characters, to act with that ardor which has made us love even its exaggerations and its imprudences." (⁴⁸) p. 66. "There is a decided emotional ground tone of purely subjective origin showing itself in vague longings and pleasing moods of melancholy, and craving for something objective to attach itself to."¹ "A new birth of all the higher social qualities, the affective faculties, the social instincts, the altruistic organic cravings," takes place at adolescence. The individual lives not so much for himself and the present; for his horizon broadens, and as he thinks more of the future, a keener and deeper sense of the seriousness and responsibility of life is awakened. The distinction between right and wrong, of purity and impurity, becomes clearer and more defined. Rudyard Kipling strikes this psychological truth when he remarks: "Youngsters in their repentant moments consider their sins much more serious and ineffaceable than they really are."² The ethical perceptions are no doubt intensified by the influence of the reproductive organs. As Clouston says: "The powers and instincts that make for the continuance of the race strengthen every other power and faculty at that period (adolescence) of life. The sense of seriousness and responsibility of life is first roused through them. The sense of right and wrong, good and evil, is by them kindled into strength enough to guide the conduct. Shame, modesty, chivalry, self-denial, tenderness and a host of other virtues and essential social graces are founded on them. The highest moral qualities, the keenest yearnings after the good, the intensest hatred and scorn of evil are not to be found in the asexual men and women." (⁴⁹).

III. Another characteristic closely related to the foregoing is increased emotional activity. The adolescent mind overflows with enthusiasm. The most lofty ambitions are nour-

¹ Maudsley, "Pathology of the Human Mind," p. 449.

² "Thrown Away."

abel. Impulse, which never reasons out the consequences, is the chief main-spring of action. We find abundant illustrations of this phase of mental life in literature. George Eliot's characters furnish some very valuable psychological material along this line. "Maggie Tulliver, with her enthusiastic self-renunciation alternating with 'volcanic upheavings of imprisoned passions,' with her 'wide, hopeless yearning for that something, whatever it was, that was greatest and best on earth ;' and Tom, with his energy and self-reliance, kept from waywardness by the wholesome prophylactic of work; Gwendolen Harleth, with her intense desire for admiration, her impulsive activity, selfishness, and inchoate religious and ethical sentiment ;—these, perhaps, are the most striking examples." (⁴⁷) p. 177. Of Gwendolen Harleth, Clouston says: "From the time when at the gaming table, Gwendolen caught Deronda's eye, and was totally swayed in feeling and action by the presence of a person of the other sex whom she had never seen before: playing not because she liked it or wished to win, but because he was looking on,—all through the story till her marriage, there is a perfect picture of female adolescence. The subjective egoism tending toward objective dualism, the resolute action from instinct, and the setting at defiance of calculation and reason, the want of any definite desire to marry, while all her conduct tended to promote proposals, the selfishness as regards her relatives, even her mother, and the intense craving to be admired, are all true to nature." (⁴⁸) p. 546. To use the author's own words: "Gwendolen was in that mood of defiance in which the mind loses sight of any end beyond the satisfaction of engaged resistance; and with the puerile stupidity of a dominant impulse includes luck among its object of defiance. Since she was not winning strikingly, the next best thing was to lose strikingly." "I will do something. I will be something. Things will come out right." This is a characteristic outburst of adolescent passion and confidence.

But there is a darker side to the picture. Consequent upon this influx of new life so full of promise and potency, are manifold dangers to the health, both of body and mind. Although Prof. Key, from his observations upon children in Swedish schools, found that the rapid growth in boys from their fourteenth to sixteenth years, and in girls a little earlier, was accompanied by increase of power to resist disease, yet far too little is known of the exact relation of the "marked physical changes that occur at adolescence to health, and physical and mental activity."¹ (⁴⁷) pp. 176, 180. Science is

¹ Key, *Schulhygienische Untersuchungen*. Hamburg, 1889.

coming to recognize more and more the very intimate and constant interrelation between mind and body. The conditions of one determine to a large degree those of the other. Digestion, alimentation, and the various functions of the secretory, and by no means least in importance, the reproductive organs, exert a decided influence on the psychical life. "Il n'existe nulle part dans l'économie, une sympathie plus intime que celle qui relie aux centres nerveux les organes de la reproduction, et tel est leur empire sur les manifestations de la vie intellectuelle, qu'on pourrait sous ce rapport partager l'existence humaine en trois grandes périodes ; avant, pendant, après la période des fonctions génitales."¹ In the middle ages it was a subject of debate, how far a woman in her periods was responsible for her acts. But the reproductive functions if normal are healthy. "Comme le levain est bon pour la pâte, ainsi les menstrues sont bonnes pour la femme." In both sexes there is great danger that these functions may be developed prematurely, resulting disastrously both to body and mind. "L'influence morale se fait encore sentir sur l'époque de la première apparition des règles, qui retardée ou avancée suivant l'éducation que reçoit la jeune fille le milieu dans lequel elle vit. Toute excitation génésique (roman, bal, théâtre) hâte le moment de la puberté pour la jeune fille, et, pour la femme déjà réglée, augmente la quantité de sang perdue à chaque époque. Tout le monde sait qu'à la campagne la menstruation est plus tardive qu'à la ville." (⁴⁶) p. 37.

President Hall has said that one of the greatest dangers of this period "is that the sexual elements of soul and body will be developed prematurely and disproportionately. Indeed early maturity in this respect is in itself bad. If it occurs before other compensating and controlling powers are unfolded, this element is hypertrophied and absorbs and dwarfs their energy, and it is then more likely to be uninstructed and to suck up all that is vile in the environment. Probably the greatest and most experienced living teacher of physiology has expressed the opinion that at least nine-tenths of the thoughts, feelings, imaginations of the average male adolescent centre for a few early years of this period about this factor of his nature." (⁵⁰) p. 207. The advent of puberty when normally reached has generally something of the mysterious and unknown for the individual; and the newly awakened sensations put to test the balance and self-control of the healthiest and best instructed natures. But when

¹ Ball, "Leçons sur les maladies mentales; folies génitales." Paris, 1883, p. 571. Quoted by Icard, p. 4.

sexual maturity is hastened by reading bad literature, a perverted imagination, bad companionship, or by whatever causes, the disastrous results can hardly be estimated. It is at puberty that the distinctive traits and characteristics of heredity appear. With the youth of bad ancestry it is a particularly critical period. The diseases of this period of life are mostly the result of heredity. Some one has said that "every hereditary disease of adolescence means that the law of arrest and destruction of a bad stock by organic or reproductive death is coming into operation." Nature then may be said to make a test of the vitality and power of the individual to survive, a principle which the primitive races seem to imitate in their various initiation ceremonies.

It has been said that puberty is a period of "unstable mental equilibrium." "Puberty is the first really dangerous period in the life of both sexes as regards the occurrence of insanity; but it is not nearly so dangerous as the period of adolescence, a few years afterwards, when the body as well as the reproductive functions have been more fully developed." While affections needing asylum treatment are very rare during adolescence, affections of the nervous centres are very apt to appear at this period of life, notably the two great derangements of the motor centres, epilepsy and chorea. Insanity of puberty in both sexes is characterized especially by motor restlessness. Clouston (³⁹), pp. 538-539. In Tuke's Dictionary of Psychological Medicine, it is estimated that in seventy-eight per cent. of the cases of insanity of adolescence the symptoms are those of mania, which is characterized by marked mental exaltation, restlessness, self-conceit, actions of a hysterical nature. In the first stage there is a period of "low spirits" and "diminished energy." At such periods life is apt to be dreamy and uninteresting. Then there is an "elevated emotional condition," "assumption of manly airs," "acting out of bravado." This is usually followed by acute mania, "when the speech becomes incoherent, the conduct outrageous and violent, and the habits filthy and degraded." The same authority states that twenty-two per cent. of the cases are melancholia, delusion and stupor, all marked with a periodic tendency, and there is in some cases a suicidal tendency. (⁴⁰) Morel says that sixty-six per cent. of periodic psychosis start at puberty. This periodic tendency is particularly marked in the female sex. Krafft-Ebing cites nineteen personal observations of psychosis returning at each period. And according to the *Gazette Medicale de Paris*, out of two hundred and thirty-five cases of insanity, twenty-seven were due to menstruation. "The menstrual function can, by sympathy, especially in the case of those predisposed, create

a mental state varying from simple psychalgia—that is, simple malaise moral, simple disturbance of mind—to alienation, to complete loss of reason, and modifying moral responsibility from simple extenuation to absolute irresponsibility.”⁽⁴⁶⁾ Puberty is liable to “hallucinations and frenzied religious conceptions.” The writer, at the close of an account of a male patient twenty years of age admitted to the Mavisbank Asylum (Edinburgh, May 31, 1886), adds: “Mental disorder occurring at the period of rapid growth preceding full development, generally takes the form of mania. There is exaltation with a great deal of conceit, and the ideas and delusions, if they exist, are of a sexual and religious nature. Such cases exhibit in a greatly exaggerated and distorted form the mental state of most people at that period of existence.” This patient thought he had committed an unpardonable sin and had been condemned to everlasting punishment.⁽⁵¹⁾ pp. 69-72. A young girl of good family and equally good character, experienced a sudden suppression of the periods in consequence of a “conscientious scruple.” It brought on delirium and convulsions, and sanity and reason returned only with decline of menstruation.⁽⁴⁶⁾ p. 197.

This leads me to remark that the moral sense, which is usually much keener at puberty, may become morbid. Undoubtedly the phenomena of excess so characteristic in this period of life have a very close relation with this newly awakened capacity for the distinction between right and wrong, purity and impurity. Excess almost always produces reaction. In the case of the drunkard a sense of remorse usually follows a debauch. Icard says: “Un scrupule, un enseignement imprudent, l'éloquence peu sage d'un prédicateur ont suffi, dans quelques circonstances pour troubler, tout à la fois, les fonctions cérébrales et la fonctions menstruelle.”⁽⁴⁶⁾ p. 197. He also cites a case from the *Gazette Médicale de Paris* of a child twelve years of age, who was so frightened by the terrors of hell aroused in her mind by a preacher that she lost consciousness and had several convulsions. This is somewhat similar to the case which Clouston gives of a girl fifteen years old, “clever and studious, who, after hearing a sermon one Sunday, became very depressed and insisted on praying with the other girls at school.”⁽³⁰⁾ p. 43. A recent writer has said: “No part of us is more susceptible of morbidness than the moral sense; none demoralizes more thoroughly when morbid. The trouble, too, affects chiefly those of the finer fibre—the person who criticises everything he does, who has lost his sense of proportion, who teases himself endlessly and teases his friends about the right and wrong of each petty act. It is a disease, a moral

disease, and takes the place in the spiritual of 'nervous prostration' in the physical." (⁵²) p. 680. Again, what may be called morbid doubting is another danger to which the adolescent is liable. Dr. Burnham's study of adolescence contains some very valuable material respecting the "function of doubt" during adolescent development. The personal experiences which he collected were for the most part those of men who were especially interested in philosophical studies. He states that about three-fourths of his correspondents had passed through a period of doubt. Some regarded it a normal experience, others thought it to be abnormal. Naturally in these cases, doubts were of a philosophical and religious nature. In the newly aroused mental activity at puberty, there seems to be, as already observed, a tendency to analyze and reconstruct beliefs of every kind. A reason is demanded for everything. A typical, though perhaps little extreme, experience is that of one correspondent, who began by having some doubts as regards the efficacy of prayer. "The doubting process thus begun was carried on, being always stimulated by fresh discoveries of the error of early beliefs. It will be noticed that this skepticism was directed solely against authority. I was not conscious that God had deceived me, or the world, or nature, or the senses, but men, authorities, teachers, preachers, writers. My distrust, therefore, in authority increased, until I arrived at that sort of universal skepticism which consists in a disposition to accept no statement of authority till examined and verified by my own reason, in other words a kind of universal skepticism. Happily in this case, as the person himself states, doubt was rather a constant and steady stimulus to inquiry," and he was safely carried through the iconoclastic to the reconstruction period. (⁴⁷) pp. 184-5. Doubt itself is, in some persons, a most powerful stimulus to study, and though "there is more faith in honest doubt than in half the creeds," there are instances where this passion for analysis and reasoning becomes almost a disease, absorbing all interest and usurping all activity. It would seem that pessimism would be one result of this mental condition, as it plainly is when emotion becomes the aim of life. "Shelley, Byron, Novalis, and perhaps both Goethe and Schiller in their younger days, are examples. With these adolescents the hunt for intense and lasting emotion goes vigorously on, but since emotion seems to be subject to the psycho-physic law which applies to sensation in general, and an emotion can be sustained only by constantly increasing stimuli, and since emotion itself, as Mr. Mill found, is apt to vanish if made an end and analyzed, they are doomed to failure, and pessimism is the inevitable

result. Much of modern poetry has never outgrown this pessimism." (⁴⁷) p. 192. "Active emotion, intense in quality, unlimited in quantity, is what the poets of the revolution desire. One need only mention 'Werther,' 'The Robbers,' 'The Revolt of Islam,' 'Faust,' to suggest what is meant by the spirit of these revolutionary poetry."¹

Furthermore, this period of such intense intellectual and emotional activity is attended by another danger. Youth is a period when habits are being permanently formed, when life is plastic, and one is liable to drift in any direction. If these yearnings and activities of the adolescent are wrongly directed, they often lead to crime. W. D. Morrison states that there is a very rapid increase of crime from the age of sixteen. He further says "that if youths between the ages of sixteen and twenty-one could by any possibility be prevented from embarking on a criminal career, the drop in the criminal population would be far-reaching in its effects. It is from the ranks of young people just entering early manhood that a large portion of the habitual criminal population is recruited; and if this critical period of life can be tided over without repeated acts of crime, there is much less likelihood of a young man degenerating afterwards into a criminal of the professional class." (⁵³) p. 168.

Corre gives some figures showing the rapid increase at the advent of puberty: Of 7,473 prisoners in France in 1883 under twenty-one years of age, there were as follows: (⁵⁴) p. 309.

Below 8 years of age,	14 boys,	6 girls.
From 8 to 10 years of age,	159 "	37 "
" 10 " 12 " "	425 "	117 "
" 12 " 14 " "	1,214 "	269 "
" 14 " 16 " "	1,739 "	409 "
" 16 " 18 " "	1,765 "	385 "
" 18 " 20 " "	714 "	209 "
Over 20 years of age,	3 "	8 "

The writer also adds in a note to page 316, the following: "Sur 26,000 malfaiteurs arrêtés dans une année à Paris, 16,000 n'ont pas 20 ans et presque tous montrent un cynisme extraordinaire."²

But suffice it for the dangers of this life which begins at puberty. Various are the remedies which have been advocated. We may believe that the initiation rites of primitive people had as one object the prevention of excess and promotion of steadiness and self-control. He who can control him-

¹ Royce, "Rel. Asps. of Phil." p. 111.

² Macé, *Lundis*, pp. 308-9.

self is well equipped for this "storm and stress" period. To learn the right use of this influx of new life, physical and psychical, is a far higher ideal than that of the ascetics who advocate the extinction of all bodily appetites. This warfare against the body finds an advocate even in Plato. "Whence come wars and fightings and factions? Whence but from the body and the lusts of the body? Even if we are at leisure and betake ourselves to some speculation, the body is always breaking in upon us, causing turmoil and confusion in our enquiries, and so amazing us that we are prevented from seeing the truth. It has been proved to us by experience that if we would have pure knowledge of anything we must quit of the body. . . . Having got rid of the foolishness of the body, we shall be pure and hold converse with the pure."¹ But a better philosophy takes nature as it finds it, and seeks to utilize in its designs much which has not only been looked upon as useless, but absolutely harmful and dangerous. These appetites and cravings of the physical nature are but a power for good if they can be turned into the right channel. With this end in view, physical training is coming to be more and more recognized as fundamental to any complete and wholesome system of education.

Besides developing the muscles and thus giving shape and hardness to the body, physical exercise trains the motor centres of the brain and thus aids in developing the mental faculties. Physical exercise furnishes an important outlet for the great amount of energy at puberty. Activity of one kind or another is an absolute necessity for healthy adolescence. "At this period education must no longer be mere acquisition, it must give outlet for action. Youth must be given an opportunity to do something. With many this is necessary for mental balance and sanity: for all it is a means of saving waste energy. It is the period for manual training, for work in the laboratory, for physical training, sports, excursions, and the like. It is, moreover, the period for manifold activities. If ever Herbart's many-sided interest may be aroused, it is now. Balance should be obtained by presenting many things. Undue tendency to introspection must be checked by allurements to action." (⁴⁷) p. 193.

ADOLESCENCE, THE PERIOD OF "NEW LIFE" BY NATURE,
ALSO THE TIME FOR THE "NEW LIFE"
SPIRITUALLY.

There is need, in these "storm and stress" years which begin at puberty, of the very best means of regulating and de-

¹ Phædo, Jowett's Trans. II. pp. 205-6.

veloping life, both of body and soul. Physical training, modern industrial and laboratory methods, are supplying a long felt want. Students of criminology are emphasizing the value of industrial education as a preventive of crime. ⁽⁵⁵⁾ p. 212. For the youth who is approaching manhood, with his newly awakened feelings, impulses and ambitions, a new environment, interests and duties, hitherto unappreciated, are essential. It is no misguided instinct which has led tribes and races in all times and places, as I have already shown, to observe this period of life with instruction, rules and regulations, which indicate to some degree, a consciousness of the greatness of this change and its attendant dangers. Under the same instinct the church has recognized the necessity of an ethical and spiritual change, and as a result we find a parallel if not a remnant or natural development of this world-wide cultus of initiation rites, in the confirmation ceremonies in civilized nations. At the beginning of the adolescent life, savage peoples admitted to citizenship the youth who had proved their fitness for the duties and responsibilities of citizens. In the church the instruction and discipline of childhood culminate in the rite of confirmation, when the young are recognized as independent and responsible members of the Christian community. The Roman catechism suggests the twelfth year as a suitable time for confirmation; at any rate it should not be given until the age of reason. ⁽⁵⁶⁾ "In baptism a man is received to warfare, and in confirmation he is armed to fight: in baptism we are regenerated to life, after baptism we are confirmed to fight; in baptism we are washed, after baptism we are strengthened."¹ My main thesis is to show that these practices, both of civilized and uncivilized people, are founded on fundamental physical and psychological principles, and accordingly to emphasize not only the fitness but also the need of the spiritual change, which theology has formulated in the doctrine of regeneration, during the adolescent years. I have briefly indicated the physiological and psychological characteristics of this period of life for the purpose of showing the predispositions to such a change, and the reasons why it should occur at puberty. From what has preceded it is evident that with this new life which dawns at puberty, there are strong predilections toward vice and crime, and very powerful impulses toward virtue and goodness. Some writer has well said that in love, which is one of the principal questions of youth, there is ruin or salvation. I maintain that during the adolescent years there is a necessity, grounded

¹ Pope Melchiades quoted in Hornihold, "The Commandments and Sacraments."

in man's nature as a human being, of a new consciousness of one's relation to God, of a conscious choice of Christ as the chief object of his love and service, with an overmastering sense of the duties and responsibilities involved in this ideal life, in order that he may not only be saved from selfishness and the dangers which threaten youth, but also that his newly awakened capacities and powers may be controlled and used for the development of the noblest character.

I think it is possible to conclusively show by statistics that the majority of conversions in most churches occurred during adolescence. Several clergymen have told me that two-thirds of the Christians in their churches were converted under the age of twenty-five. Christopher Cushing in a pamphlet entitled, "The Methods of the Spirit," states that in his own ministry the average age of conversion was twenty-two and a half years: that of men being twenty-three and a half, and that of the women twenty-one and a half. Another writer estimates that of 1,000 Christians, 548 were converted under 20, and 337 between 30 and 40. He adds from careful observation that of 253 converts

138 were converted under 20 years of age.

85	"	"	between 20 and 30 years of age.
22	"	"	" 30 " 40 "
4	"	"	" 40 " 50 "
3	"	"	" 50 " 60 "
1	"	"	" 60 " 70 "

In what follows I hope to show by further evidence that this is the most natural period for conversion. Theology and methods of religious instruction have too often committed the mistake of disregarding the natural development of the human mind. They have sought to help man to a better life without knowing the conditions and stages of growth in the mental life. Old and young have been made to take their spiritual food in doses of the same compound and quantity. But if the so-called secular education needs to be guided by psychological principles, surely moral and religious instruction, so universally recognized as of supreme importance, require all that psychology can teach about the subject with which religion and morality are concerned. The nascent periods in a child's life must be studied and utilized; his interests at the different periods must receive careful attention, and the natural development of the mind not to be sacrificed for the sake of *a priori* theories.

"First the blade, then the ear, then the full corn in the ear," expresses as truly the order and development of the soul as that of nature; and therefore the necessity that teachings and beliefs be adapted to the natural instincts and appe-

tites. Lotze says truly that there can be revelation only when the divine influences are reflected upon by mind. The activity of the subject himself is one of the essential factors in revelation. "We cannot imagine the recognition of any fact as something that can be simply communicated, something that reaches the mind ready made without any activity on its part; we can only imagine that occasion can be given to the mind to, as it were, produce such recognition by exercising this activity, and in this it is that every appropriation of a truth must consist."¹ But beside this waste in teaching those truths which cannot be appreciated for lack of capacity in the child, there is great danger in the sphere of morals and religion of precocious development. I have mentioned the tendency to self-examination and morbidity of the moral sense in adolescents. Clouston has called attention to the same danger of forcing mental activity. The strong points in early childhood, like a keen moral sense or a strong, remarkable memory, are liable to become the weak points in later life. Over-haste for conversions makes the mistake of trying to reap without having sown good seed or given it time to ripen. (³⁰) p. 313. This forcing process can but produce a shallow, unhealthy Christian character, which never deepens nor broadens with the growth of years. Christians of this sort have been compared to early risers, of whom Mill remarked that they are conceited all the forenoon and stupid all the afternoon. The fruit that lasts the longest is not the first to show signs of ripeness.

Again this morbid tendency of the moral sense is kept alive and intensified by a great deal of the preaching and religious literature which cultivates introspection and constant self-examination. It has been well said that "excited preaching and revival meetings are only suited to stolid, healthy brains." Mr. Charles Francis Adams, in an interesting paper read before the Massachusetts Historical Society at the June meeting, 1891, states that in studying the records of some of the oldest churches in Massachusetts, in particular those of Dedham, Braintree and Groton, he finds a rapid increase in the number of confessions of sexual immorality beginning at about the time of the "Great Awakening." This "tide of immorality," which is usually attributed to the French wars which broke out in 1744 and closed in 1760, he suggests, might have been due to the religious conditions of the times. Admitting that a "state of morbid and spiritual excitement" and a keener conviction of sin would lead to the confessions of sins of such a character, he, however, raises the question "how far a *morbidly excited spiritual* condition may also be

¹ Mic. Bk. IX. Chap. IV. § 1.

responsible for the *sin confessed*." A satisfactory consideration of this question he leaves to others with the remark that "the connection between the animal and spiritual natures of human beings taken in the aggregate, though subtle, is close; and while it is well known that camp-meetings have never been looked upon as peculiar or even conspicuous for the continence supposed to prevail at them, there is no doubt that in England the license of the restoration followed close upon the rule of the saints." (57) p. 502. Mr. Adams's suggestion will doubtless meet with a great deal of opposition, and, unfortunately, the facts are of such a character that they cannot be easily obtained. It is certainly an interesting question, and there seem to be indications, at least, which strengthen the affirmative. Dr. Brinton has said that the stimulation of the religious sentiment arouses the passion for love, which will be directed as the temperament and individual culture prompt. (45) p. 73. Mr. Adams further says that this "morbid excitement," which is a common phenomenon of religious revivals, in the "Great Awakening" was a species of insanity. "This religious contagion of 1735 reveals a state of affairs bordering close on universal insanity." I quote Dr. Brinton again: "Every violent revival has left after it a small crop of religious melancholiacs and lunatics. Competent authorities state that in modern communities religious insanity is most frequent in those sects which are given to emotional forms of religion, the Methodists and Baptists for example; whereas it is least known among Roman Catholics, where doubt and anxiety are at once allayed by an infallible decree, and among the Quakers, where enthusiasm is discouraged, and with whom the restraint of emotion is a part of discipline." (45) pp. 75-6.

Another tendency of the "storm and stress" period, that of morbid doubting, is frequently excited by religious teachings which disregard pedagogical principles. Granting that doubt is a natural and healthy activity of mind, and that experience of unlearning many things toughens the mental and moral fibres, yet it is a sound principle to inculcate as far as possible such knowledge and beliefs in childhood as will fall in with later life, and thus aim to produce a gradual development and not one marked by revolutions. A narrow dogmatic religious training in many children only adds seriousness and danger to the crisis of early manhood when one is obliged "to think for himself." Frederick W. Robertson has said, "Let a child's religion be capable of expansion, as literature is systematic as possible; let it lie upon the heart like the loose soil, which can be broken through as the heart is into fuller life. If it be trodden down hard and stiff in

formularies, it is more than probable that the whole must be burst through and broken violently and thrown off altogether when the soul requires room to germinate."¹

But besides adding fuel to the flame, bad religious education dwarfs and even suppresses the natural life of childhood and early youth, which is spontaneous, instinctive, emotional and unreflective. Therefore instead of answering questions before they are asked, or forcing problems upon the young mind which it has not yet encountered in its own life, the aim should be to make the religious life as bright, hopeful and attractive as possible. The mysteries of religion, the sense of sin and guilt, cannot and should not be known by the mind that is instinctive with wonder, implicit trust and obedience. To the naïve curiosity of the child, nature and the supernatural are one. He can know no God who needs proofs of his existence. His interests and wishes are confined mostly within the domestic circle. But in early adolescence, as we have seen, a larger world opens. There is a tendency to break with the past, to form new friendships and confide secrets to other persons than parents. Then is the time when there is need, as I have already said, of a *new consciousness of God*, the recognition of duties as divine commands, of the supernatural and transcendental power manifesting its unchanging laws both in the natural and moral world. In this period of self-examination, inquiry and search for truth, the instinctive faith of childhood should develop into a deeper, more rational faith, grounded in the conscious personal need of the divine. It has been said that a child should grow up a Christian and never know himself as being otherwise. (⁵⁸) p. 10. So far as this emphasizes the need of early training and gradual growth, it is sound doctrine. But coincident with the great intellectual, moral and emotional changes at adolescence, a new conception of God and spiritual things would seem natural for the normal, active individual, and I believe that this is happily the fact. This is not inconsistent with the view which I have supported all along, that religious training ought to be such as will produce a gradual, steady growth. A growth by epochs is not a growth by revolutions. In the majority of well regulated lives there are epochs more or less clearly defined. Just as there are marked changes in the physical, mental and moral spheres at puberty, so there ought to be a more or less conscious change in the religious life. It is a period of transition in every sphere, and very important it is that this transition be in every respect not too sudden or violent. It

¹ Sermons, p. 55.

has been and is a far too common practice in some religious circles to depend largely upon seasons of special religious effort for the conversion of men. Horace Bushnell says of the "great revival" under Edwards, that while it had the great merit of displacing an era of dead formalism, it had the great defect of introducing an individualism which made too much of the personal experience element. I quote his own words: "It takes every man as if he had existed alone; presumes that he is unreconciled to God until he has undergone some sudden and explosive experience in adult years, or after the age of reason; demands that experience, and only when it is reached, allows the subject to be an heir of life. Then on the other side, or that of the Spirit of God, the very act or ictus by which the change is wrought is isolated or individualized so as to stand in no connection with any other of God's means or causes, an epiphany, in which God leaps from the stars, or some place from above, to do a work apart from all system, or connection with his other works. Religion is thus a kind of transcendental matter, which belongs on the outside of life and has no part in the laws by which life is organized, a miraculous epidemic, a fire-ball shot from the moon, something holy because it is from God, but so extraordinary, so out of place, that it can not suffer any vital connection with the ties and causes and forms and habits which constitute the frame of our history." (58) pp. 187-188.

I suspect that this presentation of the subject will call forth the criticism that no distinction is made between conversion and regeneration, the former being commonly regarded the act of the subject himself and the latter the work of the Holy Spirit. This is regarded as very fundamental in most systems of theology. But fine theological distinctions and hair-splittings are omitted with a purpose; for an exhaustive and systematic treatment of the doctrine, with perhaps some emphasis of the divine element which theologians insist upon contrasting with the human, is not intended. The early church was divided on the questions of the effects of the fall upon freedom and the exact relation between the human and divine will in regeneration, and which takes the initiative in the change of heart. But such questions were largely occasioned by the denial or ignorance of a conception which is influencing more and more at the present time the whole realm of theology, namely, the immanence of God. In the Latin theology, which has had such a predominating influence upon all successive theology, the dogma of original sin, according to which man was separated absolutely from God, was the fundamental doctrine. The will of man was corrupted and

rendered impotent by the fall of Adam and could be restored only by a divine act, and this took place in baptism. Hence there was an absolute necessity, for man's first step toward salvation, of a special interposition and act of God. On the other hand, in Clement of Alexandria, we find the conception that life is a process of education under a divine instructor, and a recognition of the immanence of God underlies my treatment of this subject. It is in God that "we live and move and have our being."

If the criticism is passed that this discussion makes regeneration a "natural process" and leaves out the supernatural element, such as the work of the Holy Spirit, in addition to the answer already contained in the statements that the physiological and psychological aspects are here and now the chief considerations, and that no attempt is made to define the nature or extent of the Spirit's influence, as is usually done in a treatment of this subject, my position may be made a little clearer by asking if there is not a "supernatural element," so-called, in the laws and phenomena of mental life already considered?

Right here is the error of many theories of regeneration. So much has been made of the deadening influence of the first sin, of the gulf between God and man beyond any human power to bridge, that the good in human nature has been overlooked. There are traces of this error in writers like Drummond.¹ Admitting as he does that "it is altogether unlikely that man spiritual, should be violently separated in all the conditions of growth, development and life from man physical," p. 35, he says later that "the inorganic world is staked off from the living by barriers which have never yet been crossed from within. . . . The door from the organic to the inorganic is shut, no mineral can open it: so the door from the natural to the spiritual is shut and no man can open it." P. 71. Does he mean that the natural man has no more ability to become spiritual and no more responsibility than the mineral has to become a plant? This view would make regeneration a literal and absolutely new creation, not recognizing if not denying the fact that man at bottom, in his capacities at least, is a spiritual being, made in the image of God. On the other hand, the burden of this discussion has been to show that man needs and has capacities for a change of heart. The questions, "How can a man be born anew?" "What will thou have me to do?" "O wretched man that I am! who shall deliver me from the body of this death?" but express the universal longings of humanity,

¹ "Natural Law in the Spiritual World."

which the Christian ideal of life seeks to satisfy. These questions reveal a better nature in man, "a diviner self" striving for mastery. It was these hungerings and thirstings of the human soul to which Christ appealed. He urged men to lay hold on that life which is life indeed. It was his life and example which attracted men and awakened in them the better self. To become his followers they must give up the old life for one of love and service. Hence in Christianity the doctrine of regeneration assumes the most definite and largest meaning, namely, likeness to Christ. This ideal was required of men, we may believe, because they were constituted for it and needed it to complete their manhood. This life, it seems to me, appeals to the young of that age which I have tried to describe. It is then that such an ideal in all its attractiveness can be appreciated, when the individual is so full of life and ambitions, with cravings and longings which the things of childhood cannot satisfy.

There is a positiveness in the Christian ideal which has a peculiar salutary and saving power. In Gautama's conception the higher life is knowledge. Sensuality and individuality are simply delusions of our ignorance. Spinoza's conception of the new birth was also intellectual, a transition from obscure to clear ideas. The only thing necessary is the knowledge of the nature and the cause of our passions.¹

With Schopenhauer, contemplation of the beautiful is the ideal. In Buddhism, as in Christianity, renunciation is a cardinal doctrine. But while in the former the main effort is to rescue life from desire and delusion, a salvation ending in the annihilation of the will to live, in the latter salvation is not only *from* something, but *to* something, namely, life eternal. Is there not a feeling of barrenness in much of the reflective, contemplative views of life which leave out action? Matthew Arnold's poem, "Self-Dependence," appeals to us by its beautiful description of the calm, serene life of the stars which—

"Demand not that the things without them
Yield them love, amusement, sympathy."

But the lesson which he makes them teach, that if we are to be as the stars are, we must live as they do:—"Resolve to be thyself, and know that he who finds himself, loses his misery"—lacks the truth *to live* by that the Christian ideal gives us. That which I have called the "positiveness" of the Christian life is expressed in the words of Paul, "Walk by the Spirit and ye shall not fulfill the lust of the flesh."²

¹ *Ethica*, III. Def. 1 and 2, IV. 2-5.

² *Gal.* 5:16.

It is a truth luminous on every page of gospel teaching. Not contemplation, not introspection and self-examination, but action is the golden truth of Christianity. Not only was it required of the disciples that they follow their Master in His deeds of love, in feeding the hungry and caring for the sick, but they were told that to understand Him and His teachings they must live out the life in action.

We have already seen that action of some kind was one of the most healthy and saving influences for adolescents. They need activity as an outlet for the rapid influx of new forces, something which shall absorb their interests and lead their attention and thoughts away from their own natures. Despine in his *Philosophy of Crime* says that thieves are usually lazy and have bad passions. If a man is lazy he is apt to have some outburst of passion. Work, instead of being man's curse, is his greatest blessing.

It is said that a regenerate life is a saved life. Such a life as the Christian ideal cannot but insure salvation: salvation not from the sin that some great ancestor committed in the dim past, but from the slavery and bondage of our own lusts, appetites and passions. "Man's natural sinfulness is really nothing more than the preponderance of his sensuous impulses and the insubordination of his fleshly nature to his spirit."¹ Christ likened sin to a disease which he came to heal,² and in a parable also to death,³ so that the new life is a living again. This contrast between the old life and the new is represented by Paul as a dying and a living again.⁴

The regenerate life is a "new creation," but one that fulfills the first; for "the spiritual is to the natural as the grain which ripens in the sunshine is to the seed that dies in the earth. The Christian character, in its perfect idea, is the nature of man completely ethicized through the indwelling of the Spirit."⁵

Paul's distinction between the "natural" and the "spiritual" man, finds an analogue in the difference between the life of childhood and manhood. The new life of manhood, with a new sense of sin, right and wrong, new faculties, demands a higher life than that of childhood.

The regenerate life is a changed life, but instead of coming like a lightning stroke to shock, shatter and even kill all existing life, it is a change marked by the consciousness of the person's own needs, and that the Christ life can satisfy them.

¹ Müller, *Christian Doctrine of Sin*, Vol. II. p. 280.

² Matt. 9:12.

³ Luke 15:24-32.

⁴ Rom. 6:1-14; Eph. 4:22-24.

⁵ Smyth, "Christian Ethics," p. 4.

Robertson has said, "To be a son of God is one thing; to know the fact is another—and that is regeneration."¹

"Because ye are sons, God sent forth the Spirit of His Son into our hearts."² When adolescents come to know themselves, their own resources and capacities, and the power of the Christian life to use and control them, and thus save life, religion will seem not only more natural and reasonable, but above all an absolute necessity. Then it will be said with Augustine, "Thou hast made us for Thyself, and our heart is restless until it find rest in Thee."³

¹ Sermons, pp. 273-4.

² Gal. 4:6.

³ Confessions, Bk. I. Ch. I.

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THE LANGUAGE OF CHILDHOOD.

The profound psychogenetic significance of the language function, not only as an *index* of mind development, but also as a *factor* in that development, justifies its treatment in a separate chapter. Such separate treatment would not otherwise be justifiable, inasmuch as language does not constitute a new psychic phenomenon, or class of phenomena, differing in any essential respect from those already treated. It rather partakes of the nature of them all, and constitutes a grand product of their conjoint operation.

In order to the employment of language of any sort,¹ there must be, in the first place, *sensation*. If sounds are to be intelligently uttered, they must first be heard. The child who is born deaf, and continues in that condition, does not learn to speak. In the second place, language presupposes *perception* and *judgment*. The sounds must not only be heard, they must be understood. A meaning must be attached to them. Otherwise they will never be given back by the child as the expression of his thought; *i. e.*, as his language. In the third place, it is essential to any advance beyond the merest linguistic rudiments, that abstraction and generalization take place; for the communication of thought, in its highest forms, cannot take place until there has been attained the comprehension of the general as distinguished from the particular, and of the abstract as distinguished from the concrete.² Finally, passing from the cognitive to the volitional aspect of mind, it is obvious that language, in its most essential characteristic—*i. e.*, as expression—belongs to the will. Every expression of thought, whether it be word or mark or gesture, is the result of an act of will, and as such may be classed among movements.

It is not, therefore, as constituting a new order of facts, different from thoughts and feelings and volitions, but rather as illustrating the development of these, and entering as a factor in that development, that language receives this separate place. We judge of the child's mental development largely by the rapidity of his progress towards a skillful manipulation of the instruments of expression.

¹ Although our chief attention is occupied here with the spoken word, this is by no means the only form of language. In its broadest sense, language includes every means by which thought is communicated; and therefore the gestures of the deaf-mute, and the hieroglyphic characters on Egyptian monuments, as well as the written manuscript and the printed page, are as really language as the most eloquent oral paragraphs, because they are the expression of someone's thought. As Broca says, language is "the faculty of establishing a constant relation between an idea and a sign," whatever that sign may be. All that can be said, therefore, concerning the psychological importance of the spoken word, applies equally, *mutatis mutandis*, to every other means of communication.

² On the other hand, thought itself cannot attain to any great degree of generality without the aid of language. Thought and language are mutually helpful, and conduce each to the development of the other.

I.—HEREDITY VS. EDUCATION IN LANGUAGE.

There is no psychological problem to the solution of which a study of the infant mind may be expected to contribute more largely than this: What is hereditary, and what is acquired, in the sphere of language? Long before maturity is attained, such an abundance of acquired material has been added to our original store, and through constant repetition, the two have become so transformed, modified and assimilated in character, that we are no longer able to distinguish the one from the other. But from the beginning it was not so. If a child executes a gesture, or utters a sound, at an age so early as to exclude the possibility of imitation or spontaneous invention on his part, we may conclude that the sound or the gesture—or, at least the disposition to express himself in this manner—has been born with him. Here only, then, are we able to apply the logical method of difference to the solution of the problem.

It is obvious, at a glance, that speech is a product of the conjoint operation of these two factors: *heredity* and *education*. If, on the one hand, we observe the initial babbling of the infant, and notice its marvelous flexibility, and the enormous variety of its intonations and inflections—and this at an age so early as to preclude observation and imitation of others,—it will be apparent that the child has come into the world already possessing a considerable portion of the equipment by which he shall in after years give expression to his feelings and thoughts. If, on the other hand, we carefully observe him during the first two years of his life, and note how the intonations, and afterwards the words, of those by whom he is surrounded, are given back by him—at first unconsciously, but afterwards with intention—and how, when conscious imitation has once set in, it plays thenceforth the preponderating rôle,—we shall readily believe that, without this second factor, but little progress would be made towards speech-acquirement.

It may be well to consider briefly how these two factors enter at every point in the development of language. For example, in order to speak, the child must possess first of all a sensory and motor physiological apparatus. This physiological apparatus, including the auditory structure for the reception of sounds, the inter-central and centro-motor cells and nerve tracts for the accomplishment of connection between the impression and the expression, and the organs of vocal utterance (larynx, palate, tongue, lips, teeth), is his inheritance from the past, but in the new-born child it is all imperfect, both in structure and in functioning; and its development requires the constant moulding influence of those educating agencies by which the human being is surrounded from the moment of his entrance into the world.

Again, the *disposition* to utter sounds of all sorts, and to express states of feeling by them, is undoubtedly inherited, since, from the very beginning of life, and quite independently of all example, the child constantly exercises his vocal organs.¹ But in spite of this, so inadequate is heredity alone, that the child will not learn the language of his parents, unless he be in the society of those who employ it. If brought up among savages, he will speak their language; if among wolves, he will howl.²

¹"Le langage est en nous une faculté si naturelle, que dès la première enfance, l'exercer est un besoin et un plaisir."—Egger.

²"It is found that young birds never have the song peculiar to their species, if they have not heard it; whereas, they acquire very easily the song of almost any other bird with which they are associated."—Alfred Russell Wallace, *Natural Selection*.

In making this statement, we do not overlook those remarkable cases in which children have invented a language of their own, quite different from that spoken around them; and persisted for some time in using the former and entirely ignoring the latter. Mr. Horatio Hale gives an account of five different cases in which this has occurred, two in the United States and three in Canada. In one case this invented vocabulary consisted of twenty-one root-forms, out of which, by combination and modification, the children developed a complete language, by which, with the aid of gesture, all their wants could be communicated; and in all the cases the invented language was sufficient for all intercourse as between the children themselves; and was persistently used until the children were finally broken of it, by being separated or sent to school⁽⁶⁶⁾. In all these cases, it is to be observed, the child did not learn the language of his parents in the absence of those who employed it. It is also to be noted that the new language was invented, not by one child, but by two. Language is *possible* in all normal children; it becomes *actual* only in the presence of a companion. But given the companion, and scarcely any limit can be set to the possibilities of development. Indeed, Mr. Hale has given us a theory of language, in which the origin of linguistic stocks is attributed to the inventiveness of children who have become separated from their tribe when very young; and in the light of such facts as those given above, the theory seems highly probable. On the other hand, that the child shall speak any specific tongue now existing, depends on his education. He does not inherit any particular tongue or dialect. Some think he will acquire his mother-tongue with greater facility than any other⁽⁶⁷⁾, yet even this may be doubted. "Speech is hereditary, but not any particular form of speech"⁽⁶⁸⁾. There may be an inherited tendency to find certain sounds difficult, such as *sh* to the ancient Ephraimite, or *th* to the modern Frenchman, but this is only a tendency, and does not prevent the child from learning any language perfectly, if his education begins early enough.

Again, the careful study of the language of signs makes it quite clear that many gestures are inherited (*e. g.*, holding out the hands to express desire, which is world-wide, and is executed by children who have never seen it done), but the development of gesture into anything like a complicated system of expression, is quite dependent on the social environment. Of course this is only another way of saying that language, being the instrument for the communication of thought, is not developed in the absence of beings to whom thought can be communicated.

Thus, then, the case seems to stand with regard to the respective spheres of heredity and education in the production of language. As regards the child's present endowment and capabilities at the moment of his entrance into the world, "he is the product, the result of the generations which have preceded him; he is the visible link which connects the past with the future"⁽⁶⁹⁾; but with regard to that which he is to be, and the legacy which he in his turn shall transmit to those who shall succeed him, he is very largely dependent on his physical and social environment; and all those who compose that environment, assist, whether they will or no, in his education.¹

¹ "La mère, au reste, ou la nourrice, ne sont ici que des institutrices en chef; car tous ceux qui entourent l'enfant au berceau qui convergent en sa présence, participent, sans s'en douter, à cette éducation fondamentale" (90:32).

II.—THE PHYSIOLOGICAL DEVELOPMENT.

If the question were asked, "Why does not the new-born child talk?" two answers might be given. In the first place, there is a psychological reason, viz., he has, as yet, no ideas, and has, therefore, nothing to say (²⁷). In the second place, there is a physiological reason, viz., his speech-apparatus is as yet so imperfectly developed that he could not express ideas if he had them.

In the same way, if the question were asked, Why does any person ever lose the power of speech? similar answers might be given. He either loses his ideas, through some mental disorder, or he loses the power of expression through some physiological disorder. The two cases are, moreover, parallel in another sense, inasmuch as the acquirement of ideas in the one case, and their failure in the other, are closely associated with, if not indeed quite dependent upon, the presence or absence of the physiological functions.

The physiological reason, then, why the child does not yet speak, lies in the undeveloped state of the speech-apparatus. "The lungs are not yet developed in a degree and manner sufficient for articulate speech. The expiration needs to be strong, and exactly regulated. Now, in the infant, the pectoral muscles are still developed in a very small degree; the breathing is accomplished much more through the fall of the diaphragm than through the active extension of the pectoral cavity. Hence, breathing movements are more superficial and more irregular than in later years. Artificial speech requires complete control of the breathing mechanism, which the child has not yet got. To his speech-instrument is still wanting a large number of strings, whistles and registers. The organs of speech are the lungs, air tubes, larynx and vocal cords, the mouth, with tongue, palate, lips and teeth. The lungs create the stream of air; the tone and voice are formed by the larynx; according as the vocal cords open wider or come nearer, arises the deeper or higher tone. The form of the tone (i.e., vowel *a* or *o*, etc., consonant *b* or *f*, etc.) depends on the form of the mouth at the time. Now the larynx is still very small and undeveloped in its form, and so with the tongue, the lips, and the muscles moving them; and as for the teeth, they are still entirely wanting" (²⁷). The undeveloped condition of the auditory apparatus, and of the brain, have also to be considered in this connection.

On the other hand, it needs to be borne in mind that the relation between the organs of speech and speech itself is a reciprocal one. If speech depends on the organs, it is also true that the organs depend on speech, and are not developed, except by exercise. As one learns to play on the harp by playing on the harp, so the child learns to speak by speaking. The exercise of the vocal organs develops those organs, so that they become capable of higher exercise.

The lungs first appear, early in the embryonic stage, as a single median diverticulum from the ventral wall of the œsophagus, which soon becomes dilated towards the two sides in the form of primitive protrusions or tubercles, while the root, communicating with the œsophagus, remains single. The foetal lungs contain no air, and lie, packed in a comparatively small compass, at the back of the thorax. They undergo very rapid and remarkable changes after birth, in consequence of the commencement of respiration. They expand so as to completely cover the pleural portions of the pericardium, their margins become more obtuse, and their whole form less compressed. The entrance of the air changes their

texture so that it becomes more loose, light and spongy, and less granular; while the great quantity of blood, which, from this time on, circulates through them, greatly increases their weight, and changes their color. The proportion of their weight to that of the body becomes nearly twice as great as before, while, at the same time, their specific gravity, after the beginning of respiration, becomes very much less (s: 887).

The *trachea*, or windpipe, which connects the lungs with the larynx, is in the embryo almost closed, its anterior and posterior walls being very near each other. The small space remaining is filled with mucus. With the exercise of respiration, the mucus is expelled, and the tube itself gradually becomes more distended, but its anterior wall does not for some time become convex. With the growth of the child, the cartilages which form the "ribs" of the trachea, become stronger and better able to bear their part in the forcible expiration of air which is required for speech (s: 808).

The *larynx*, which is the organ most directly concerned in the production of "voice" or "tone," is an exceedingly complicated mechanism, consisting of a framework of cartilages comprising no less than nine distinct parts, connected by elastic membranes or ligaments, two of which, from their specially prominent position, are named the true vocal cords. In speaking and singing, these cartilages are moved relatively to one another by the laryngeal muscles. The larynx is situated at the upper end of the trachea, the mucus lining of the two organs being continuous. At the time of birth, this organ is very small and narrow, and continues comparatively insignificant up to the period of adolescence, when rapid and remarkable changes take place, especially in the case of the male, where it becomes much more prominent, and the *pomum adam* protrudes so to be perceptible at the throat (s: 822).

The *tongue* is composed very largely of muscular fibres, running in various directions, such as the superior and inferior lingual muscles, which move the organ up and down, and the transverse fibres, by which it is moved from side to side. Besides these, we have the various glossal muscles, which, though extrinsic to the tongue itself, yet are implicated in its operations. These muscles are all more or less flabby in the fœtus and the new-born, and become strong only by nutrition and exercise. A similar remark applies to the lips; while the teeth, without which the dental and labio-dental consonants can never be properly pronounced, are at the beginning of life entirely absent, though the first steps toward their formation take place as early as the seventh week of the period of gestation (s: 866).

The *brain* of the fœtus is comparatively deficient in convolutions, and presents a smooth, even appearance. The first of the primary fissures to appear is the fissure of Sylvius, which is visible during the third month. The other primitive sulci also begin to appear about this time, and by the end of the fifth month are well established. The secondary sulci make their appearance from the fifth or sixth month on. The first of these to be seen is the fissure of Rolando. "By the end of the seventh month, nearly all the chief features of the cerebral convolutions and sulci have appeared. The last fissures to appear are the inferior occipito-temporal, and a small furrow crossing the end of the calloso-marginal" (s: 836). But long after the extra-uterine life begins, the child-brain is still deficient in many of the higher processes, the association fibres being the last to develop. The convolutions are for a long time comparatively simple, and their increasing complexity as life

advances stands to the exercise of the various faculties, partly in the relation of antecedent, and partly in that of consequent.

Speech, then, in the little child is a potentiality, though not an actuality. He is, as it were, in possession of the machine, but the belts have not yet been adjusted to the pulleys, nor has he yet learned to handle the instrument. The inability to speak is not, therefore, an *abnormal* state at the beginning of life, any more than the inability to write or swim or play the piano (^{3:30}). It is merely an *imperfect* state. But the inability to *learn* to speak is abnormal, and its cause must be sought, not in immaturity, but in abnormality, of the physiological or psychological structures and processes involved. The one is an unnatural condition, into which the child has fallen; the other a natural condition, out of which he will gradually rise.

III.—PHONETIC AND PSYCHIC DEVELOPMENT.

We shall here, first of all, give a sort of outline history of the speech-progress of the average child during the first two years, generalizing from a large number of actual observations (made by different persons on different children) and proceeding by periods of six months each; then we shall give summarized statements of a number of child-vocabularies that have been carefully compiled at different ages; and finally, we shall examine what general conclusions may be drawn from the material at hand, and set down as empirical laws, awaiting further substantiation. I say "empirical laws," because children differ so much from each other, and reliable observations are so comparatively scanty that, for the present, general statements must be held in abeyance, or made only tentatively.

First Six Months.—"In Thuringia," says Sigismund, "they call the first three months 'das dumme Vierteljahr,'" and during the second three months, according to Schultze, no advance is made on the first. It might seem, then, that in this first half-year there is nothing worthy of our attention in the matter of language. This, however, is very far from being the case, for in this period a most important apprenticeship is going on. The little child, even in the cradle, and before he is able to raise himself to a sitting posture, is receiving impression every waking moment from the environment; is hearing the words, seeing the gestures, and noting—in a manner perhaps not purely involuntary—the intonations of those around him; and out of this material, he afterwards builds up his own vocabulary. Not only so, but during this period, that peculiarly charming infantile babble (which Ploss calls "das Lallen") begins, which, though only an "awkward twittering" (⁶), yet contains in rudimentary form nearly all the sounds which afterwards, by combination, yield the potent instrument of speech. A wonderful variety of sounds, some of which afterwards give the child difficulty when he tries to produce them, are now produced automatically, by a purely impulsive exercise of the vocal muscles; in the same way as the child at this age performs automatically many eye-movements, which afterwards become difficult, or even impossible (⁴²). M. Taine thinks that "all shades of emotion, wonder, joy, willfulness and sadness" are at this time expressed by differences of tone, equaling or even surpassing the adult (³⁷).

The child's first act is to cry.¹ This cry has been variously inter-

¹"Sobald das Kind zur Welt geboren ist, fängt es an gellend zu schreien" (1). "The child is born into the world! He enters it struggling; a scream is his first utterance" (57).

ed. Semmig calls it "the triumphant song of everlasting life," describes it as "heavenly music" (*himmlische Musik*); Kant it was a cry of wrath, and others have spoken of it as a wailing on entering this world of sin; or as a foreboding of pains and sorrows of life. It seems more scientific, though less so, to accept the explanation of the "unembarrassed naturalist," who sees in it nothing more nor less than the expression of the fullness of the first breathing—the rush of cold air upon the lungs⁽¹⁾.

more important point is the relation of this first vocal utterance to the speech that is to follow. The cry at first is merely an automatic or reflex "squall," without expressive modulation or active timbre; the same cry serves to express all sorts of feelings.

But very soon it becomes differentiated and assumes various meanings to express various mental states. This differentiation occurs at different times in different children. A girl only fifteen months old expressed her desire to be fed by a particular sort of cry.

In another case, the cry had ceased to be a mere squall by the end of the first month⁽¹⁾. In another, the feelings of hunger, pain, joy and desire were expressed by different sounds by the end of the fifth week⁽²⁾. Others report the transition from the "cry" to the "voice"⁽³⁾, involving coöperation of the hands and tongue, at different times, but all within the first three months⁽⁴⁾.

These cries are variously described. According to one, "the cry in infancy is generally longer continued than the cry of fear"⁽⁵⁾. Another speaks of the cry of fear as "short and explosive," while the cry of pain is expressed by a long drawn out wail (M). Another child at six months expressed pleasure and pain by different forms of the vowel *a*. Sigismund's boy, in his sixth month, expressed his desire to be fed by a peculiar crowing shout, accompanied by kicking and waving his hands.

The next step is taken when these cries and babblings assume an articulate character. The alphabetic sounds begin to be heard. First, the vowels usually precede the consonants; and of these, *a* with its various shadings is generally the first to appear.¹ In one case the following series was developed: *ä-a-u*⁽²⁷⁾. In another, the sound of *a-a*, as an expression of joy, was heard by the end of the fifth week⁽³⁾. According to Löbische, the vowels developed in the following order: *a-e-o-u-i*⁽²⁸⁾. One child began with *a*, and then passed to *ai-ä-au-ü*, while the pure sound of *ö* was late in coming. In another case all the vowels were heard in the first months, *ä* being the most frequently employed; and in another, the primitive *a* (of which the child's first cries largely consisted) was differentiated into the various vowel-sounds during the

It is necessary at this point to adopt a system of diacritical marks, as in all that follows the child's pronunciation is of great importance. We shall, therefore, adopt the following system, and shall take the liberty of changing, wherever necessary, the marks of the recorded observations, for the sake of uniformity:

<i>e</i> calm.	<i>ē</i> or <i>ee</i> as in <i>eat, feet, etc.</i>	<i>oo</i> as in <i>food</i> .
<i>i</i> fat.	<i>i</i> as in <i>pit</i> .	<i>ōō</i> as in <i>foot</i> .
<i>e</i> fate.	<i>ī</i> as in <i>ice</i> .	<i>u</i> as in <i>up</i> .
<i>o</i> owl.	<i>o</i> as in <i>pot</i> .	<i>ü</i> as in <i>use</i> .
<i>erman</i> a umlaut).	<i>ö</i> as in <i>old</i> .	<i>ü</i> (German <i>u</i> umlaut).
<i>e</i> pet.	<i>ö</i> (German <i>o</i> umlaut).	

Changes will also be made in the use of the consonants. For example, such words as *corner, chorus, coffee, etc.*, will be spelled with a *k*; words like *cigar, center, etc.* with an *s*; and in such words as *write* the silent *w* will be omitted. Other changes will be indicated as they are made.

first month (¹²). Preyer reports the use of the vowel-sounds in the following order: *uā-ao-ai-uao-ā o a-ā a a-o ā ō-ū-e-ā-i-u*; and Sigismund in the following: *a-ā-u-ei-o-i-ō-ū-āu-au*.

Long before the sixth month, the primitive vowels are combined with one another (as we see) and with consonants, to produce the first syllabic utterances. These first syllables are, for the most part, mechanical. In a great many of the cases under consideration, the first consonants to make their appearance are the labials, *b-p-m*, and these are almost always initial at first, and not final. The easy consonant *m*, combined in this way with the easy vowel *a*, yields the familiar combination *ma*, which, by spontaneous reduplication, becomes *mama*. In a similar manner, *papa*, *baba* (afterwards *baby*) and the like, are constructed. The labials are not always, however, the first consonantal sounds uttered. Sometimes the gutturals (*g* or *k*) precede them; and the two consonants which are usually the last to appear (viz., *r* and *l*) are used by some children quite early. In the case of the boy A., the first sounds were guttural, *gg*, though the earliest combination was *mam-mam*, used in crying. At five months "he dropped the throat-sounds almost entirely, and began the shrill enunciation of vowels;" and at six months he lowered his voice and began to use lip-sounds, simultaneously with the cutting of his first teeth. In another case, *m* appeared as the first consonant in the second month and was followed by *b-d-n-r*, occasionally *g* and *h*, and very rarely *k*; the first syllables were *pa-ma-ta-na* (¹³). Löbische observed the consonants in this order: *m-(w)-b-p-d-t-l-n-s-r*; Sigismund in this: *b-m-n-d-s-g-w-f-ch-k-l-r-sch*; and Dr. Brown in this: *b-p-f-r-m-g-k-h-t-d-l-n* (¹⁴). In some cases nearly all syllables have been correctly pronounced during the first half-year (¹⁵); while in others progress is much slower, very few syllables being certainly mastered before the ninth month (¹⁶).

We may sometimes observe here also the beginnings of vocal imitation. The boy A. was observed to "watch attentively the lip-movements of his attendants;" and other observers have remarked, from about the fourth month, "a curious mimicry of conversation, imitating especially the cadences, so that persons in the adjoining room would think conversation was going on" (¹⁷). The same thing was observed in A. a little later.

Second Six Months.—Most children make a very marked advance during this period in the imitation of sounds, in the intentional use of sounds with a meaning, and in the comprehension of the meanings of words and gestures. The actual vocabulary of most children at this age is, however, exceedingly small. Many children, a year old, cannot speak a single word, while the average vocabulary does not probably exceed half a dozen words.

A new advance accompanies the rise of active hearing, and the increasing power of attention in the third three months. The child begins to keep a sort of time to music, in which he shows pleasure, and this strong excitement stimulates the production of new sounds (¹⁸). He delights in being carried about with a galloping rhythmic motion, and will smack his lips and make other sounds in imitation of chirping to a horse (M). He pats his hands together in imitation of the accompanying motions in a nursery rhyme, and sometimes makes an attempt to say the words also. He shows a fondness for ringing the changes on certain syllables which he has learned, varying and reduplicating: e. g., *mama*, *baba*, *gaga*, *nana*, etc., and other less intelligible combinations.

He understands many words which he cannot pronounce, and he

pronounces some in a mechanical way without understanding. He knows each member of the household by name, and will reach a biscuit to the person named to him. He knows the principal parts of his own body, and will point to them when asked (M) ⁽³⁾. The words *papa* and *mama*, whose surprising universality may be partly accounted for by the physiological law of ease (the sound most easily produced and, therefore, earliest used, being naturally associated with those persons whose presence arouses the earliest and most vivid emotions and ideas), are by many children at this time intelligently used, though some are later in this.

Imitation usually makes rapid strides in this period. In one case gestures were imitated at eight months, and words at nine. If someone is being called, the child also calls loudly. In another case, towards the end of the child's first year, he began to imitate the sounds made by animals and inanimate objects ⁽³⁾. Sigismund observed the instinct of imitation showing itself in the third quarter of the first year; the reduplication of syllables composed of a labial or dental consonant and the vowel *a*; and the more frequent occurrence of syllables in which the vowel is initial. Champney's child distinctly imitated the *intonation* of the voice when any word or sentence was repeated to him several times. This has been observed also in other cases (m).

Children who are able to use a few words at this age, show by their use of them how inadequately defined is their meaning. A little girl, who had learned to say *â gâ* (all gone) and *gâ gâ* (gegangen), applied the latter term to herself when falling down (M). Humphreys says the child he observed was able, at this time, to name many things correctly, and to pronounce all initial consonants distinctly, except *th-t-d-v* and *l*. Some final consonants were indistinct. Another child, at eleven months, knew what *guten tag* meant, and responded with *tata*; he also answered *adieu* with *adaa*. In this case, the first association of a sound with a concept was *ee*, which meant *wet* ⁽³⁾. A boy of ten months used intelligently the words *mama*, *Aggie* (*Maggie*, this afterwards became *Waggie*) and *addie* (*auntie*). At eleven months, *Waggie* was shortened to *Wag*, and *addie* to *att* (A). Another at seven months used to wave his hand and say *tata* at parting; and one day he did this when a closet door was opened and shut again ⁽³⁾. Taine's little girl at twelve months, on learning the word *bébé*, as connected with the picture of the infant Jesus, afterwards extended it, curiously enough, not to all babies, but to all pictures. Occasionally a word is invented, such as the word *mum*, reported by Mr. Darwin, which the child used with an interrogatory sound when asking for food, but also "as a substantive of wide signification." I observed a similar general use of *da*, in the case of F. In another case, the word *bo* was used to signify anything that pleased the child. The words *mama*, *papa* and *babé*, which had been used for some time mechanically, were dropped about the middle of this period, to be resumed five months later, "when they were applied to their proper objects" ⁽¹⁹⁾. Sully observed in the beginning of this period (which he calls the *la la* period) the rise of spontaneous articulation. Combinations of syllables were suddenly hit upon, and repeated without any meaning, except as indications of baby feeling. Long *a* indicated surprise, and "a kind of o, formed by sucking in the breath, indicated pleasure at some new object" ⁽⁶⁴⁾. In one case, a little sentence—which really consisted of two words—was uttered by a child at the close of this period. He said: "*Papa—mama*," which meant: "*Papa, take me to mama*" ^(3:251).

The wide differences among children make it unsafe to venture any generalizations, except one, viz., this second half-year seems to be *par excellence* the period of the rise of imitation. Some children, however, are as far advanced at the beginning of this period as others are at its end. Perhaps it ought also to be remarked that the child who shows a great precocity in imitation, or in learning to speak, will not necessarily, on that account, turn out a more intelligent child. Imitation does not require a very high degree of mental acuteness, and the child who has been slow in this *may* in the end surpass his more precocious companion.

Third Six Months.—While the child is learning to walk, there is very often a standstill, or even a retrograde movement in the matter of speech. After walking is mastered, the acquisition of language goes forward again with greater facility than ever.

During this third period, marked progress is usually made in the understanding of words, and in their intelligent application, though the vocabulary is still very limited, and the pronunciation imperfect. Difficult sounds are omitted, or replaced by easier ones. Sometimes the change in one consonant has an influence on another which precedes or follows it. In longer words and combinations, only the prominent part—the accented syllable, or the final sound—is reproduced. A final *ie* is often added to words. The child says *dinnie* for *dinner*, *innie* for *drink*, and *beddy* for *bread*. Other imperfect pronunciations are: *apy tee* (*apple tree*), *piecy book* (*picture book*), *garny* or *nannie* (*grandma*), *pee* (*please*), *pepe* (*pencil*), *mo-a* (*more*), *hō* or *hā* (*horse*), *Balbert* (*Gilbert*), *Tot* (*Topf*), *Ka-ka* (*Carrie*), and *Kakie* (*Katy*).

Most children at this age understand a great deal of what is said to them. Such phrases as “bring the ball,” “come on papa’s knee;” “go down;” “come here;” “give me a kiss,” are perfectly understood and obeyed. Parts of the child’s body, as eyes, nose, ear, other ear, hand, etc., other person’s eyes, ears, etc., are pointed to when named. Articles are fetched, carried and put where one commands (A), (F), (W).

Some children begin, towards the end of this period, to express themselves in short sentences, which are usually elliptical, or, as Romanes says, “sentence-words,” only the most prominent word or words in the sentence being pronounced. *E. g.*, *ta* (*thank you*), *nee* (*take me on your knee*) (²⁸); *det off*; *det up*; *where cows George?* (*where are Uncle George’s cows?*) (M); *mo-a*, *mama* (*give me more, mama*); *dao* (*take me down from my chair*) (²⁹). Many combinations of words are made, which fall short of the dignity of sentences. *E. g.*, *mama dess*, *ding-a-ling*, etc. A boy of eighteen months “knows the last words of many of Mother Goose melodies, as *moon O*; *place O*; *bare, bare, bare*; putting them in at the right time, enthusiastically” (³⁰).

Some words are invented by the child. *E. g.*, the word *tem*, which Taine’s little girl spontaneously used as a sort of general demonstrative, “a sympathetic articulation, that she herself has found in harmony with all fixed and distinct intention, and which consequently is associated with her principal fixed and distinct intentions, which at present are desires to take, to have, to make others take, to look, to make others look” (³¹). The same child invented the word *ham* to signify “something to eat,” just as Darwin’s boy used *mum* for the same purpose.

The love of reduplication shows itself very distinctly now, as indeed it has almost from the beginning; no doubt for the physiological reason that it is easier for the vocal organs to execute a

movement over again, to which they are adjusted, and which they have performed once, than to adjust themselves to a new movement. Very probably the love of repetition and "jingle" which is so noticeable in children (and which, as Sigismund says, lies at the foundation of rhyme), also enters as a factor here. Numerous examples of the onomatopoeic naming of animals and things may also be observed at this time, though many of these are, no doubt, imitated from grown-up people. One or both of these tendencies may be observed in such expressions as the following: *dada*, *mama*, *papa*, *warra* (*water*), *wah wah* or *oua oua* or *bow wow* (*dog*), *es es* (*yes*), *nī nī* (*nice*), *ko ko* (*chicken*), *puff* (*wind*), *quack quack* (*duck*), *golloh* or *lululu* (*all rolling objects*), *bopoo* (*bottle*), *too too* (*cars*), *tuppa tuppa tee* (*potato*), etc. The child imitates (often spontaneously) the sounds made by the dog, cat, sheep, ticking of clock, etc., while many sounds are reduplicated. The opposite process, a spontaneous curtailing of certain words, may be sometimes noticed. In one case a boy of fifteen months contracted *papa*, *mama* and *addie* into *pa*, *ma* and *att* respectively, having never heard any of these latter words (A).

Imitation is now very strong. The child attempts to repeat everything he hears; but some sounds give him difficulty, and the shifts to which he resorts in such cases are of very great interest. The boy R. used to say *nana* for *thank you*, and *ditt tatt* for *get caught* (in play); but the phrase *excuse me* was too much for him; he therefore used *ōhō* in its place, with a rising inflection on the second syllable. Singing is often imitated better than speech. A boy of fourteen months "gave back a little song, in the right key" (1:130); and another, in the sixteenth month, knew some simple little hymns (16).

But perhaps the most interesting thing of all at this time is the gradual "clearing" of the childish concepts, as indicated by the steady circumscription of the application of names. Even yet, however, names are applied much too widely; much more experience is necessary before they acquire, in the young mind, a clear and definite connotation. (Even in mature life, most of our concepts are still very hazy and ill-defined; and it might be allowable to describe the whole process of intellectual education as a process of the clarification of the concept.) It is interesting, also, to note how the principle of association enters as a factor in the determination of the application of the name. When a child calls the moon a lamp, or applies his word *bō* (*ball*) to oranges, bubbles, and other round objects; calls everything *bow wow* which bears any sort of resemblance to a dog (64) (including the bronze dogs on the staircase, and the goat in the yard) (27); applies his words *papa* and *mama* to all men and all women respectively; makes his word *cūtie* do duty, not only for *knife*, but also for *scissors*, *shears*, *sickle*, etc. (66); says *bā* (*bath*) on seeing a crust dipped in tea (64); applies *atī* (*assis*) to *chair*, *footstool*, *bench*, *sitting down*, *sit down*, etc. (23); *peudu* (*perdu*) or *atta* (*gone or lost*) to all sorts of disappearances;—it is evident that one great striking resemblance has overshadowed all differences in the objects. Another child, who had learned the word *ōt* as a name for objects that were too warm, extended it to include, also, objects that were too cold (association by contrast). Later, on looking at a picture, he pointed to the representation of clouds and said *ōt*. The clouds reminded him, no doubt, of the steam from the tea-kettle (64). This aptitude for seizing analogies, which Taine believes to be the source of general ideas and of language, has numerous illustrations, not only in the

language of the child just learning to speak, but also in the use of words by uncivilized or semi-civilized peoples.¹

Fourth Six Months.—During the latter half of the second year linguistic progress is usually so rapid as to render a detailed account impossible. We can only call attention, with examples, to some of the most striking features.

"By the end of the second year," says Schultze, "the normal child can make himself understood in a short sentence." His own child was able, at nineteen months, to use sentences containing subject, predicate and object. In another case, quite a complicated sentence (but very elliptical, only the nouns being uttered), was heard in the twentieth month (^{1:147}). In the case of A., a genuine sorrow was the occasion of his first sentence. His father, of whom he was very fond, had been playing with him for some time, and finally, being called away, put him down and went out, closing the door behind him. The child gazed for a moment at the closed door, and then, throwing himself on the floor, cried out, *I want my papa*. Before this, he used to express himself chiefly in elliptical sentences and sentence-words. When slightly over two years of age, he used to weave little stories of his own; e. g., *mama fã wite downy toppy houf*. One day, while the dinner was waiting for his father, who was expected home on the train, the child said: *Toot-toot comy wite up lair, iny doh, uppy tûpool; toot-toot make big noise*. Another of his sentences was: *Take a badie bidy to; badie tiehd, feeppy*. The boy C. uttered his first sentence in the twenty-first month: *Pees mama*. Two months earlier he had used sentence-words; e. g., *papa cacker* (*papa has fire-crackers*). In the twenty-fourth month he told quite an extensive story, in which the verbs were not expressed. Even compound sentences, and sentences containing subordinate clauses, are often mastered before the close of this period (⁶⁴) (⁶⁷). Sometimes verbal inflections appear; e. g., *naughty baby klide (cried)* (³⁶). Another day the same child said *comed for came*, thus unconsciously rebuking the inconsistent English language. Interrogative sentences appeared in another case; e. g., *where's pussy?* and negation was expressed by an affirmative sentence, with an emphatic *no* tacked on at the end, exactly as the deaf-mutes do. Many of these primitive sentences are in the imperative mood, and many are still "sentence-words." Most children talk a great deal, and gesticulate profusely, at this time. Their expressions are concrete, and abstract words are avoided as far as possible. A little boy, on seeing the picture of a half-grown lad, spoke of it as a *little baby man* (A). Anything that has rhyme or rhythm is most easily picked up. A little nephew of my own was able, at this age, to sing a large number of little songs and hymns, giving the melody quite correctly. Another boy, at twenty-one months, on hearing the milkman's bell in the morning, used to say: *Mik man mik cow, crump horn, toss dog, kiss maid all florn*; or peeping through the fence at the cows, would sing: *Moo cow, moo cow, how-de-do cow* (⁶⁵).

The child's progress is marked here by his gradual mastery of the personal and possessive pronouns. These are peculiarly difficult for the average child, and, according to Egger, are not usually attained until near the close of the second year; according to others, much later still (thirtieth month, according to Lindner). Previous to mastering the *I*, the child calls himself by his proper name, or by the name *baby*, as he may have been taught. When *I*

¹See Romanes' *Mental Evolution in Man*, chap. 8.

appears, it is frequently employed,—quite consistently from the child's point of view,—not in the first person, but in the second; i. e., he calls others *I* and himself *you*. One child used the word *I* as early as the nineteenth month, but often exchanged it for her proper name (²⁷). Another, in the twentieth month, called himself by his proper name, but, a month later, said *me* for the first time (²⁸). Another spoke of *me* as a personality in her twenty-second month (²⁹). Another, at two years, often used the word *my*, meaning *your*; e. g., *let me get up on my lap* (³⁰). Another, at the same age, still speaks of himself as *baby* in ordinary converse, but in great desire says, *I want it*, and in great fear says, *child*.

In some cases, almost all the sounds are mastered by the end of the second year, but from the observations at hand, this may be considered the exception. Most children still have difficulty with certain sounds. Some of these difficulties are seen in the following: *apoo* (apple), *zhat is* (there it is), *es* (yes), *yleg* (egg; note difficulty with initial vowel), *oken* (open), *tash* (mustache), *sh'ad* (sawed), *dam* (gum), *v'al* (shawl), *uppervator* (elevator), *nobella* (underella), *bannicars* (banisters), *av yi* (all right), *setto* (cellar), *potato*, *it da* (sit there). One observer reports a special difficulty with *s*, *z*, *d*, *g*, *k*, *l*, *n*, *g*, *r* and *t* (³¹). Another says that at sixteen months, the sounds *s*, *sh*, *ch* and *j* were generally indistinct; while *w*, *r* and *f* were formed, but not well developed. On the other hand nasal *g* appeared, *o* was mastered, *l*, *p* and *t* as final consonants began to be used, and *k* became a favorite sound, used in many words. Sibilants were more at command when final than when initial, while short *a* was just beginning to be formed. In the twenty-second month the sounds of *ch*, *j* and *th* were still imperfect, the hard sound of *th* being replaced by *s* and the soft sound by *t*. A month later, *r* was still generally replaced by *l*; when *s* came where another consonant, one or the other was dropped, and *k* was sometimes confused with *p* or *t* (³²). In another case, the double consonant *sp* made its first appearance at the end of the second year (³³).

There are still many examples of the inadequate limitation of the concept. In one case the word *poor*, which was learned as an expression of pity, was applied on occasion of any sort of loss or damage whatsoever, and was even used in speaking of a crooked gum. *Dam* (gum), with which toys were mended, became a universal remedy for all things broken or disabled; and afterwards, when the child acquired the word *sh'ad* (thread), broken things were divided into two classes, viz., those that were to be mended with *dam*, and those that were to be mended with *sh'ad* (³⁴). *Behwys*, in another case, was at first the name for all small fruits, but afterwards became restricted, yielding a portion of its territory to *gape* (grape) (A). Another little boy extended his word *gee-gee* (horse) to a drawing of an ostrich, and a bronze figure of a stork; and his word *apoo* (apple) to a patch of reddish-brown color on the mantelpiece (³⁵, ³⁶). The boy C. applied the word *bōke* (broke) to a torn pocket-handkerchief; and R. extended his word *dō* (door) to everything that stopped up an opening or prevented an exit, including the cork of a bottle, and the little table that fastened him in his high chair.

Healthy children of two years or age will usually attempt all sorts of sounds in imitation of others, and will practice on new and difficult combinations with great perseverance, sometimes carrying the process through several stages of transition, until it finally assumes

the perfect form. The boy A. first heard the word *pussy* when seventeen months old; he at once undertook to say it, but called it at first *goohet*, then *goofie*, then *poopoohie*, then *poofee*, until finally, after much persevering practice, he was able to say *pussy*, when he seemed to be satisfied, and discontinued its use, except when *pussy* was in sight. Schmitze gives, among others, the following examples: The German word *wasser* passed through these stages, — *uunuff* — *fuffuff* — *uuffuuff* — *uasse* — *uasser*; the word *grossmama* was first *umama*, and then *diamama*, before assuming its final form. The strength of the reduplicating tendency, and the influence of the initial consonant on the remainder of the word, is seen in the following imitations: *uusa* (*Mary*), *dudu* (*Julia*), *th th* (*little*), *ba ba* (*blanket*), *fist* (*finger*), *mama* (*master*), *papa* (*pasture*), *nana* (*naughty*) (*a*).

I have taken the trouble to collect, for purposes of comparison, a number of vocabularies of children, which have been recorded by careful and competent observers, with as much completeness and accuracy as possible. I will now give these in summarized form, so as to show the relative frequency of the various sounds as initial, and also the relative frequency of the various parts of speech. In order the more accurately to show the sounds actually made by the child, I have been obliged to use an alphabet differing somewhat from the ordinary English alphabet. The following changes are made: *c* is dropped out altogether, such words as *corner*, *candy*, etc., being classed under *k*; words like *centre*, *cigar*, etc., under *s*; and words like *chain*, *cheese*, *chair*, etc., forming a new series under *ch*. Words like *George*, *gentleman*, etc., are classed under *J* instead of *G*; words like *Philip* under *F*; words like *knife*, *knee*, etc., under *n*; and words like *wrap*, *write*, etc., under *r*. Other new letters besides *ch* are *sh* and *th*. In short, it is sought to classify the child's words according to his pronunciation, and not according to the English alphabet. If he says *tutle* for *potato*, the word is classed under *t*. I am convinced that this is the only way to obtain reliable and valuable results.

I. A child of nine months is reported as speaking "nine words plainly." The words are not given (¹⁸).

II. A boy at twelve months has "four words of his own" (¹⁹).

III. A child of twelve months uses ten words with meaning. Six of these are nouns, two adjectives and two verbs (²⁰). The initial sounds are *m* (three times), *p* (four times), *n*, *a* and *k* (each once).

¹⁸ I cannot forbear quoting the following from Sigismund in this connection. A child of twenty-one months attempted to repeat, line by line, a piece of poetry after another person. The first line in each pair represents the pronunciation of the adult, the second the imitation of the child:

Guter Mond, du gehst so stille,
Tute Schnd, du tehs so tinn.
Durch die abendwolken hin,
Duch die aten-honten in.
Gehst so traurig, und ich fühle,
Tehs so tautech, und ich bñne.
Dass ich ohne Ruhe bin,
Dass ich eine Ule bin.
Guter Mond, du darfst es wissen,
Tute Schnd, du str es bitten.
Weil du so verschwiegen bist,
Bein do so bñten hitz.
Warum meine Thränen fliessen,
Amum meine tñnen bñten.
Und mein Herz so trätzig ist,
Und mein Aetz so atich iz (1:144).

IV. A child of one year used eight words, seven of which were nouns, and one an adverb. The initial sounds are *b* (four times), *m*, *p*, *d* and *u* (once each) (T).

V. The boy R. had at command about twenty words, thirteen of which were nouns, and four or five interjectional words. For initial sound *b* was preferred, then *p* and *t*.

VI. Another child is reported, at fifteen months, as having "syllables, but no words" (¹⁶).

VII. A girl of seventeen months is reported as using thirty-five words, twenty-two of which are nouns, four verbs, two adjectives, four adverbs and three interjections. The initial sounds are *d* (eight times), *s* (four), *m*, *b* and *ch* (three each), *p*, *t*, *k*, *a* and *y* (two each), *i*, *j*, *n*, *o* (one each) (L).

VIII. A girl of twenty-two months uses twenty-eight words, distributed as follows: Nouns sixteen, verbs three, adjectives three, adverbs and interjections five. The initial sounds are *b* (six times), *d* (five), *m* (four), *p* (three), *g*, *h* and *k* (two each), *e*, *i*, *n* and *o* (one each) (G).

IX. A girl at two years employs thirty-six words, distributed as follows: Nouns sixteen, adjectives four, pronouns three, verbs seven, adverbs three, interjections three (G). Initial sounds are *p* (five times), *m*, *b* and *w* (each four times), *g*, *k* and *h* (each three times), *d*, *i*, *n* and *r* (each twice), *a* and *o* (each once).

X. Summary of the vocabulary of a little boy in Washington, D. C., aged nineteen months (K).

XI. Vocabulary of a girl of twenty-one months; the daughter of an Andover professor (K).												
Nouns.	Adv.	ProN.	Verb.	Adv.	Prep.	Conj.	Interj.	Total.				
A	1		1					4	A			
B	2							15	B			
Ch	2							1	Ch			
D	1		1					8	D			
E	4							1	E			
F	2							4	F			
G	1		1					10	G			
H	3							8	H			
I	1		1					1	I			
J								13	J			
K	8		3					2	K			
L	5		2					7	L			
M	2		1					4	M			
N	2							5	N			
O	1							12	O			
P	6							2	P			
Q	1							2	Q			
R	1							25	R			
S	13		9					9	S			
Sh	7		1					2	Sh			
T	2							9	T			
Th	2							1	Th			
U	4							6	U			
V	1							5	V			
W	1							1	W			
Z	3							2	Z			
XII. Vocabulary of a girl of twenty-two months in Worcester, Mass. (F).												
Nouns.	Adv.	ProN.	Verb.	Adv.	Prep.	Conj.	Interj.	Total.				
A	6							3	A			
B								9	B			
Ch								5	Ch			
D	3							10	D			
E	1							6	E			
F								7	F			
G	1							10	G			
H	1							2	H			
I								20	I			
J	1							3	J			
K								10	K			
L	3							7	L			
M	3							2	M			
N	3							12	N			
O	3							9	O			
P								18	P			
Q								12	Q			
R								5	R			
S								14	S			
Sh	2							11	Sh			
Th	9							2	Th			
U								2	U			
V								7	V			
W	3							1	W			
Z	2							1	Z			
XIII. Vocabulary of a girl in Brooklyn, N. Y., in her twenty-third month (S).												
Nouns.	Adv.	ProN.	Verb.	Adv.	Prep.	Conj.	Interj.	Total.				
A	2							1	A			
B	10							25	B			
Ch	4							8	Ch			
D	1							10	D			
E	1							5	E			
F	4							1	F			
G	1							3	G			
H	2							2	H			
I	3							5	I			
J	2							1	J			
K	1							8	K			
L	8							3	L			
M	5							4	M			
N	2							13	N			
O	1							1	O			
P	11							16	P			
Q	2							2	Q			
R	1							9	R			
S	13							11	S			
Sh	3							6	Sh			
T	7							2	T			
Th	2							1	Th			
U								1	U			
V								1	V			
W	4							1	W			
Z	1							3	Z			

XVIII. Vocabulary of a San Francisco girl, in her twenty-seventh month (K).

XIX. Vocabulary of a Chicago boy, in his twenty-eighth month (D).

XX. Vocabulary of a girl of twenty-eight months (K).

XXI. Vocabulary of a Boston boy of thirty months. Probably not complete (T).

	Nouns.	Adj.	From.	Verb.	Adv.	Prep.	Conj.	Interj.	Total.		Nouns.	Adj.	From.	Verb.	Adv.	Prep.	Conj.	Interj.	Total.
A	4	2			3				15	A	4	3		1	2		1		11
B	13			9	8				65	B	28	3		9	1				41
Ch				1					12	Ch	7			1					7
D	7			10	1				38	D	16	1		7	1				25
E				1					11	E	5	1		1					7
F	4	3		1	4				27	F	13	1		6					21
G	6	1		2					15	G	7	1		4					15
H	8	2		6	2				32	H	16	2		7	1				31
I	1	2		2					9	I	2	2		4					6
J				1					11	J	4			1					4
K	11			11					55	K	37	2		8					47
L	4	1		7	4				30	L	8	2		6					14
M	9			4					34	M	15	1		5					24
N	7	3		2					23	N	9	1		3					13
O	1			2					15	O	2	1		1	2				8
P	12			4	2				52	P	30	1		4					35
Q	1			14					1	Q	1			8					20
R				11					27	R	10	2		8					19
S	4	1		20	1				77	S	35	5		8	2				50
Sh				4					14	Sh	7			2					8
T	14			11					41	T	17	1		4	2				24
Th				3					11	Th	4	2		1					12
U				2					5	U	2			2	1				4
V				2					8	V	1			1	1				4
W	6	1		14	1				43	W	7	1		5					18
Z	1			2					6	Z	2			2					4
Total.	113	16	2	27	10	1	1	1	467	Total.	287	29	13	89	19	6	1	7	451

Summarizing these vocabularies, we find some interesting facts bearing on language-growth, both on the physiological and on the psychological side.

For example, with regard to the relative frequency of the various parts of speech, the following table is instructive. Of the five thousand four hundred words comprising these vocabularies.¹

60	per cent.	are nouns.
20	" "	verbs.
9	" "	adjectives.
5	" "	adverbs.
2	" "	pronouns.
2	" "	prepositions.
1.7	" "	interjections.
0.3	" "	conjunctions.
<hr/>		
100.0		

Of the nouns, less than one per cent. are abstract. Nearly all are names of persons or familiar objects. The majority, in the earlier months, seem to be used almost with the force of proper nouns, as Schultheiss has also observed (⁷⁵). The adjectives are mostly those of size, temperature, cleanliness and its opposite, and similar familiar notions. This table also corroborates Sigismund's observation that the conjunction is especially difficult (^{1:135}). Another interesting point is the comparison of the above table with a similar table, showing the relative frequency of the various parts of speech in ordinary adult language. Professor Kirkpatrick says that of the words in the English language,

60	per cent.	are nouns.
11	" "	verbs.
22	" "	adjectives.
5.5	" "	adverbs.

An important consideration is involved here. If we look only at the first of these two tables, and consider the child's words by themselves, it will seem that the nouns have greatly the advantage over the other parts of speech. But such a conclusion obviously cannot be drawn, unless a comparison of the child's vocabulary with that of the adult justifies us in so doing. In order to show that the child learns nouns more easily than verbs, we must be able to show that the number of his nouns bears a larger proportion to the number of nouns he will use as an adult, than the number of his verbs bears to the number of verbs he will use in adult life. To represent the matter symbolically,

Let	n	=	the proportion of nouns in the child's vocabulary.
And	N	=	" " " " " " man's "
Let	v	=	" " " " " " verbs " " child's "
And	V	=	" " " " " " " " man's "

Then, if the child learns nouns more easily than verbs, the proportion of *n* to *N* will be greater than that of *v* to *V*. But on comparing the two tables, the very opposite is found to be the case.

¹In all the calculations that follow, I have taken the liberty to include, along with my own vocabularies, those of Professor Holden (⁶⁸), and Professor Humphreys (⁶⁷), which I have re-arranged phonetically for the purpose.

6. per cent. begin with the sound of t.						
5.2	"	"	"	"	"	w.
4.	"	"	"	"	"	f.
4.	"	"	"	"	"	n.
3.2	"	"	"	"	"	g.
3.1	"	"	"	"	"	l.
3.	"	"	"	"	"	a.
3.	"	"	"	"	"	r.
2.	"	"	"	"	"	i.
2.	"	"	"	"	"	sh.
1.3	"	"	"	"	"	th.
1.2	"	"	"	"	"	e.
1.1	"	"	"	"	"	o.
1.	"	"	"	"	"	ch.
1.	"	"	"	"	"	j.
1.	"	"	"	"	"	y.
0.8	"	"	"	"	"	u.
0.5	"	"	"	"	"	v.
0.2	"	"	"	"	"	q.

A glance at this table shows how prominent a place the explosive consonants occupy as initial sounds in child-language. The vowels, on the contrary, though undoubtedly the earliest sounds to be used in most cases, are very infrequent as *initial*, not only absolutely but relatively. In the English dictionaries the vowel *a* occupies fourth place as initial letter (⁶⁶), (⁶⁸); in my tables it occupies fourteenth place; while the other vowels stand still lower. The reason of this is not far to seek. It is simply a case of the operation of the law of physiological ease; as anyone may verify by pronouncing, in succession, the following syllables: *ap, pa, ab, ba, ak, ka, am, ma, ad, da*; and observing how much more easily those syllables are pronounced in which the consonant leads and the vowel follows.

Another interesting feature of this table is the high place occupied by the guttural *k* as initial sound. It stands above *p* and *m*, and next to *s* and *b*. This fact does not bear out the theory propounded by several writers on child-language, that those sounds are selected by the child for earliest acquirement whose pronunciation involves those portions of the vocal apparatus which are most easily seen, such as the lips (⁶⁶), (⁷⁶). According to this theory, not only the labial *p*, but the sounds *d, m, f, sh, th*, etc., ought to stand high in the list, because the movements involved in their pronunciation are plainly visible; while the guttural *k*, whose movements are absolutely out of sight, should stand very low. The contrary is the case; *k* stands third in the list of initial sounds, while *th*, whose movements are exceedingly obvious to sight, occupies the eighteenth place. This seems to prove that the child does not learn to utter sounds by watching the mouths of those who utter them in his presence; and this opinion is confirmed by the observation of Schultze, that the child does not usually look at the *mouth*, but at the *eyes* of the person speaking to him. On the other hand there seems no sufficient ground for the statement that the law of least effort is overturned by this frequency of the sound of *k*. This guttural sound is, for most children, no more difficult than the labials. Often it is one of the very earliest sounds employed. I know one child with whom it is more frequently used than even *b*. In short, so far as my observations go, I have no hesitation in saying that the child's earliest vocal utterances are not acquired

by imitation at all, either of sound or of movement, but that they are purely impulsive in their character. They are simply the result of the overflow of motor energy, which we have seen so prominent in other departments of the child's life; and they proceed at first along the lines of least resistance.

In the following tables I have given the results of a careful examination of seven hundred instances of mispronunciation which I have found in the above vocabularies. The first table shows the various sounds in the order of the number of times they are misused as well as the ways in which they are misused; the second and third tables enter into more detail.

In the following table the first column gives the sound misused the second shows the number of times it is replaced by another sound; the third shows how often it is dropped, without being replaced; and the fourth shows how often it is brought into a word to which it does not belong (not as a substitute for some other sound, but as a pure interpolation, for no apparent reason).

Sound Misused.	Replaced.	Dropped.	Interpolated.	Total.
R.	51	87	4	142
L.	35	70		105
S.	25	34	1	60
G.	25	6		31
T.	13	17	1	31
Sh.	26	4		30
K.	20	8		28
Th (hard).	21	2		23
F.	15	4	1	20
D.	5	12	2	19
Th (soft).	14	4		18
Ng.	15			15
N.	7	7	1	15
W.	7	5	3	15
Ch.	13			13
Y.	1	10	1	12
V.	8	2		10
E.	2	5		7
H.	2	5		7
J.	5			5
P.	4	1		5
A.		4		4
M.	4			4
Wh.	3			3
O.	3			3
B.	3			3
Z.	1	1		2
Q.	1			1

wing table shows the relative frequency of replacement of when initial, medial and final, and also (in the case of the) when occurring as one member of a double consonant (cream). It also gives the relative frequency of the substitutes:

When Initial.	When Medial.	When Final.	When Double.	Replaced by.	Times.	Examples.
1	21	9	4	w	29	kweem (cream).
				l	6	tommollä (tomorrow).
				y	3	all yite (all right).
				e	8	tumblie (tumbler).
				v	1	voom (room).
				t	1	tautech (traurig).
				m	1	pipe (ripe).
				p	1	Kaka (Carrie).
8	8	19	3	k	1	
				e	9	minnie (milk).
				w	7	tabie (table).
				u	7	singu (shingle).
				n	4	setta (celery).
				t	3	bampe (lampe).
				b	2	degen (legen).
				d	2	apoo (apple).
17	2	7		oo	1	
				s	19	fis (fish).
				h	4	hooogar (sugar).
				b	1	tooz (shoes).
				t	1	
				n	1	
18	4	3	6	t	8	tweet (sweet).
				h	8	hlate (slate).
				f	3	poofee (pussy).
				b	3	dide (side).
				d	3	
19	5	1	4	d	17	dass (glass).
				k	2	hookoo (sugar).
				t	2	toss (gross).
				b	1	bavy (gravy).
				w	1	dettin (getting).
				j	1	
				n	1	
11	3	7	5	f	10	free. (three)
				t	4	mous (mouth).
				s	3	tank (thank).
				p	1	harf (hearth).
				d	1	nuppin (nothing).
				n	1	
				r	1	

Sound Replaced.	When Initial.	When Medial.	When Final.	When Double.	Replaced by.	Times.	Examples.
K	11	7	2	7	t s g d	15 2 2 1	bastet (basket) sun (come) untle (uncle) tanny (candy)
F.	7	4	4	2	p s k t	6 5 2 2	nup (enough) buttersey (butter) kork (fork). öt (off).
Ng.		5	10	1	n e a	12 2 1	finner (finger) tockies (stockings) lockatair (rocking)
Th (sth).	11	3			d m	13 1	altogedder (altogether) dare (there)
T.		6	7		e k w g p	6 4 1 1 1	dockie (doctor) bankie (blanket) jackie (jacket) coak (coat). wawer (water)
Ch.	9	2	2	1	s t sh	7 4 2	sair (chair) tillens (children) shick (chick)
V.	1	5	2		b f d	5 2 1	gib (give). shufer (shovel) Dadie (David)
N.		1	6		e m l	4 2 1	buttie (butter) pim (pin). lemolade (lemon)
W.	6	1			v l	6 1	go vay (go away) lalla (water)
D.	1	4			n t k	2 2 1	towntownt (down) vinner (window) kankie (candy)
J.	4	1			d g	4 1	demidon (demijohn) Gekkie (Jessie)
P.	3		1	1	b t	2 2	bee (please) patie (paper)
M.	2	2			k n w	2 1 1	hankie (hammock) Waggie (Maggie)

When Initial.	When Medial.	When Final.	When Double.	Replaced by.	Times.	Examples.
3				f h	2 1	feel (wheel). haiah (where).
		3		a ē	2 1	winna (window).
1	2			d m	2 1	badie (baby). Milly (Billy).
		2		ä oo	1 1	vera (very). cookoo (cookie).
1	1			t l	1 1	torns (horns). lä lo (la haut)
	1			ē	1	bēwo (bureau).
	1			d	1	Döderfeen (Josephine).
	1			k	1	skeeze (squeeze).

The following table gives similar information with regard to *dropping* of difficult sounds:

Sound Dropped.	When Initial.	When Medial.	When Final.	When Double.	Examples.
R.	2	61	24	50	each (reach). apicot (apricot). dotta (daughter). baselet (bracelet).
L.	10	37	23	39	etta be (let me be). peeze (please). fa (fall). butterfy (butterfly).
S.	27	4	3	30	poon (spoon). Bottie (Boston). gā (gas). ābewie (strawberry).
T.		9	8	8	dissance (distance). bonny (bonnet). sottin (stocking).
D.	1	5	6	12	sanny (sandy). gamma (grandma). bines (blinds).
Y.	6	4			ard (yard). panna (piano).
K.	4	2	2	2	opf (kopf). basset (basket). boo (book).
N.	1		6	1	p; (pin). burr (burn).
G.	6			1	atten (garten).
W.	5				ont (want). oodn't (wouldn't).
E.	3		2		nuff (enough). koff (coffee).
H.	5				eah (here).
Sh.	4				litta (schlitten).
F.		3	1	2	satie pin (safety pin). natanoon (afternoon).
Th (soft).	3	1			at (that). ober air (over there).
A.	4				fade (afraid). nudda (another).
Th (hard).			2		ba (bath). mao (mouth).
V.	1		1		ammum (warum). Duttie (Gustave).
P.	1				tatie (potato).
Z.			1		no (nose).

A word of caution is perhaps necessary here. These tables do not show accurately the order of difficulty of the various sounds, inasmuch as they indicate the misuse of the sounds, not relatively to the number of *correct* pronunciations of each sound, but only relatively to the total number of mispronunciations. For example, in the first table *q* seems an easier sound than *b*, because it is only misused once, while *b* is misused three times. But if we remember that in the vocabularies *b* occurs fifty-five times as often as *q*, the case is entirely altered. Considered in this way, the order of difficulty, according to my observations, is approximately the following: *r, l, th, v, sh, y, g, ch, s, j, e, f, t, n, q, d, k, o, w, a, h, m, p, b*. The most difficult sound is *r* and the easiest *b*.

It will be observed also that, according to these tables, mispronunciation is very frequent in the case of double consonants, and most frequent of all in those combinations which belong to what Mr. Pitman calls the *pl* and *pr* series. Such words as *cream*, *bracelet* and *fly* are almost always mutilated; sometimes *r* and *l* are replaced by *w* or some other sound; sometimes they are omitted altogether.

Another thing to be observed is that the choice of a substitute for a difficult sound is often determined by the prominent consonant in the preceding or succeeding syllable. This leads to a reduplication of the easier sound in preference to the use of the more difficult one. The child says *cawkee* for *coffee*, *kork* for *fork*, or *li lo* for *la haut*. The number of these reduplications is very large, and the device is adopted also in the case of difficult vowels; e.g., *Deddie* occurs for *Edie*, and *Dida* for *Ida*.

Another significant thing is the frequency with which the sound of *i* is used as a substitute for difficult sounds, both vowel and consonantal, especially at the end of a word. The child says *ittie* for *little*, *finnie* for *finger*, and *ninnie* for *drink*.

In addition to the mispronunciations tabulated above, I find a large number of miscellaneous mispronunciations difficult to classify, such as the following: *medniss* for *medicine*, *Mangie sag* for *American flag*, *skoogie* for *excuse me*, *kidlie* for *tickle*, *pā-tū-soo* for *patent leather shoes*, etc., etc., etc.

If we seek now to discover some principle underlying the development of child-speech from the psychic point of view, we shall find, I believe, that principle of transformation, which we have already observed so frequently elsewhere, operating in this sphere also. The earliest utterances of the new-born have little or no psychic significance. As expressions of his thought, they have none at all. But by slow degrees these primitive utterances, modified, increased and combined, are associated with ideas, which are also modified, increased and combined, until finally the instrument of language is completely under control, and becomes the adequate medium for the expression of thought.

Not only may we make this statement in this general way, but it seems possible to trace, with approximate minuteness, the progress of a sound upward, from the earliest unexpressive condition to the highest, latest, most expressive state, and to indicate the principal stages on the way. These stages appear to be the same as those through which movements pass, viz., the *impulsive*, the *reflex*, the *instinctive*, and the *ideational*. The first sounds uttered by the child are simply the spontaneous will-less, idea-less manifestation of native motor energy. They do not require a sensory, but only a motor process, and that motor process is automatic. The same overflowing energy, the same muscle-instinct, which impels the child to grasp with the hands, to kick with the feet, etc., impels

him also to the exercise of his lips, tongue, larynx and lungs (30-5). This is the impulsive stage. Then we find him uttering sounds in response to certain sensations. He sees a bright light, hears a peculiar sound, feels a soft, warm touch, and these sensations call forth certain sounds. These sounds are still only babblings, not involving the cooperation of will, but they do involve sensory as well as motor processes. The reflex arc, in its simplest form, is complete. Here imitation takes its rise. This is the reflexive stage. In the next place we can detect certain sounds which are expressive of the child's needs, and though still uttered probably without conscious intention, yet have a purpose and an end, viz., the satisfaction of those needs. The cry, which was at first monotonous and expressionless, now becomes differentiated to express various states of feeling, hunger, pain, weariness, etc. Here we have the instinctive stage. Finally the will takes full possession of the apparatus of speech, the child utters his words with conscious intention; imitation of sounds, from being passive and unconscious, becomes active and conscious; and words are joined together to give expression to ideas of constantly increasing complexity. Here we have reached the ideational or deliberative stage.

As an example of the transformation of a single sound through all these successive stages, let us take that sound which is, in the majority of cases, the first articulation, the syllable *ma*. At first this is pure spontaneity. The child lies contentedly in his cradle, motor energy overflows, the lips move, gently opening and closing while the breath is expired, and this sound is produced, *mamamama*. As yet it has no meaning; it is a purely automatic utterance. But by and by the same sound is called forth by certain sensations, one of which is very probably the sight of the mother, or of some other person. The word as yet has no definite meaning, but is merely sort of vague demonstrative ejaculation, a pure reflex. Later it becomes the expression of certain bodily needs and conditions, and now the hungry child utters this sound as the expression of the need of his natural nourishment. By this means, the word becomes firmly associated with the mother, first probably with the breast only (36), but afterwards with her person in general, and so the final step in the transition is made, and the word *mama* now passes out of the semi-conscious, instinctive stage into the ideational. It becomes firmly associated with the mother, and with her only, it is used with a conscious purpose of communicating to her the child's wishes and ideas and, finally, in her absence, it is used in such a way as to show that her image is firmly stamped on his mind, and retained in his memory. In later life, more abstract and complex applications of this word are gradually mastered; but we have followed it far enough in its development for our present purpose. This word was chosen because it probably exemplifies better than any other the principle which we desired to illustrate, being associated with those feelings which arise earliest, last longest, and take the deepest hold upon the human soul; but almost any primitive utterance of infancy could be employed to exemplify, in a less complete manner, the principle enunciated.

A. A little Boston boy, whose mental development was observed and recorded by Miss Sara E. Wiltse.

B. Observations made by Professor J. M. Baldwin, of the University of Toronto, at whose suggestion the present work was undertaken.

C. A little Vermont boy, whose mother, a graduate of Smith College, made a very careful record of his mental development.

D. Vocabulary kindly sent me by Professor H. H. Donaldson, of the University of Chicago.

E. Observations made by a student of Wellesley College.

F. A little girl in Worcester, Mass., whom I observed for some time, and from whose parents I received some valuable notes.

G. Two little girls in Springfield, Mass., aged respectively twenty-four and twenty-two months. Observations made by their mother.

K. Observations kindly sent me by Professor E. A. Kirkpatrick, of Winona, Minnesota.

L. A girl in North Carolina, aged seventeen months. Notes taken by her mother.

M. Observations made by Professor and Mrs. J. F. McCurdy, of the University of Toronto.

R. A strong, healthy Canadian boy, whom I observed during a large part of his second year.

S. Notes on a little girl in Brooklyn, N. Y., sent me by her father.

T. A little boy in Boston. Vocabulary recorded by his mother.

W. A little girl in Worcester whose development was recorded by her mother.

Y. References to the lectures of the late Professor G. P. Young, on Philosophy and Psychology, delivered in the University of Toronto, but as yet unpublished.

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F. TRACY, Fellow in Clark University.

PSYCHOLOGICAL LITERATURE.

Les Origines de la Vie. LUCIANI, M. LUIGI. *Revue Scientifique*,
Tome 51, pp. 97-107.

This paper embodies the inaugural address of Professor Luciani at the Royal Institute for Higher Studies at Florence. In a most interesting manner he discusses the problems of life as they present themselves to the biologist and to the physiologist—protoplasm, amoeba, leucocytes and phagocytes, physiological experiment, heliotropism, electric stimulation, galvanotropism, chemical excitation, sensibility, psychic growth. Following are a few of the more striking passages:

"In spite of the apparent great diversity of mass, of organization, of structure, of functions, an intimate and mysterious law unites all beings and brings them back to one common origin. In the final analysis, the species and divers groups of living beings are but various degrees of differentiation of one entity (I might almost say of one incarnate idea) originally unique. The law of continuity in nature, the principle of evolution in life, in whatever way we may conceive of the mechanism, are the necessary basis of the animate world." "In nosce te ipsum—the knowledge of human nature—is resumed all the science of the physiologist."

"The question of the origin of intelligence is but the question of the origin of life, for the whole busy world is animate and psychic functions extend to all protoplasmic substance, in other words, to every living element." "With man, this diversion and this degradation of the soul (cf. the amoeba) is observed as the result of accidents, diseases or criminal acts. In such cases the psychic individuality of the man is split in two, one superior, conscious, the other inferior, subconscious." "With materialism, as well as with spiritualism—we cannot repeat it too often,—we reach the limits of science, we pass beyond it. To the question, 'what is life in itself?' I, a physiologist, can only reply: seen from without, it is matter, felt from within, it is mind. The secret mingling, or better, the confusion of the real and the ideal in nature: that is life in its highest form, that is the great mystery, which we must ever celebrate and which science can never explain."

A. F. CHAMBERLAIN.

Psychopathia Spiritualis, Friedrich Nietzsche and die Apostel der Zukunft. Von KURT EISNER. Leipzig, 1892, pp. 99.

Nietzsche, the lyric poet of thought, must not be confounded with Nietzsche, the would-be philosopher. As a poet he may be safely read and regarded as one of the most brilliant and remarkable men of his day. "Zarathustra" is in its own way a work of genius

comparable to Faust: suggestive, original and full of fire and vigor. One who acknowledges the remarkable power of the man may, however, be permitted to express his keen sense of the dangers of so persuasive a writer when his eloquent paradoxes are adopted as laws of conduct. The school of his enthusiastic followers is a large and growing one. Most prominent among these is the author of "Rembrandt als Erzieher," a work which nowhere acknowledges the source of its inspiration, but betrays it on every page. In spite of much that is subtle and profound in this much discussed book, its power has been greatly exaggerated, and it is unworthy of the profound interest it has aroused. But the widespread influence of this and kindred productions proves the necessity for an examination into Nietzsche's position on matters of vital importance. In the opinion of the writer both he and his followers are placing obstructions in the way of the solution of the great problem of the age—the question of social freedom. For this great task, cool and clear thinking is needed, not a perverse, confusing and distorted view of life and its obligations.

Nietzsche's habit is to overthrow that which he has just set up, hence to judge him fairly one must take the general drift of his writings rather than isolated expressions of opinion. He is a Romanticist, indignantly as he would deny the charge. There are many points in common between him and his spiritual brethren of the beginning of the century. As they opposed the Aufklärung, so do he and his disciples array themselves against socialism. The ironical and oracular tone, the duty of "emancipating the flesh," and the conviction that the golden age is behind and not before us—all these characterize both schools,—Hellenism is indeed Nietzsche's ideal. Greece represented to him the best the world had known, and looking upon the degenerate present he asked, "What has destroyed our happiness? What have we that the Greeks had not?" He found Christianity, compassion, asceticism, and thenceforth thundered against these sources of the misery of to-day. The decadence in which he loudly proclaims we are now living—to what is this due but to the relaxing effects of centuries of Christianity? Semitic slave morality, as he scornfully calls the law of love, must be replaced by Aryan master—morality as alone worthy to guide the conduct of the monstrous "Uebermensch," whom he holds up as the ideal of the future compassion, must be expunged from our vocabularies and our lives. "Because hard, cruel," he reiterated, "give the ego its rights, the senses full play. Cast aside the sickly asceticism which has robbed life of its beauty and freedom. Return to nature!" This call to "emancipate the flesh," what is it but an appeal to the base and animal in man—"the ape and tiger" which should be "let die"—to reassert their savage habits and to fling away all the hard-won spiritual victories of the ages? And is it a return to nature or any genuine obedience to her laws to revel in sensuality, which must inevitably end in disease of body and mind, in premature death? The same self-destructive tendency is present in the egoism which he urges, in the brutal hardness which ignores the rights and claims of others. We are all parts of a social organism and any injury inflicted upon its feeblest member must affect the whole. Because he hated humanity, Nietzsche is an anti-socialist. His views on socialism are vulgar and unintelligent. His saying "social democrats are working men who *will* not work," is worthy of some ignorant capitalist. He is indeed practically the philosopher of capital, hateful as that title would be to his soul.

Such extravagant and perverted views are a natural reaction against a dull and lifeless period. Because Nietzsche's whole theory of things rests on an unstable foundation in that he undermines the notion of truth itself, and because he is throughout a reactionary, his power must wane, little as the number and enthusiasm of his disciples now seem to warrant any such prophecy. Time is the great avenger and leveller. To "mindless socialism" belongs the future, in which Nietzsche and his school shall be only remembered as representatives of a curious and perverted phase of thought.

May 18, 1893.

C. H. S.

Die ethische Bewegung in Deutschland; Vorbereitende Mitteilungen eines Kreises gleichgesinnter Männer und Frauen zu Berlin. Zweite vermehrte Auflage. (Sommer, 1892), pp. 52.

In March, 1892, Dr. Felix Adler, founder of the American Society for Ethical Culture, addressed a small gathering of men in Berlin on the subject of this organization. The address was followed by discussion, which resulted in a second and larger meeting in April. Men and women of the most varied political and religious creeds, who were interested in the question of the formation of an ethical society in Germany, came together to hear full details as to existing societies and to decide upon some future course of action for themselves. Both these meetings were of a more or less private character. Many of those who took part in them preferred that for the present their names should remain unknown, as previously assumed obligations made them feel that a public avowal of their position might be misleading. In October, however, a general and public meeting of representative men and women from all parts of Germany was to take place in Berlin, at which time the form of organization suitable to an ethical movement in Germany should be decided upon, as well as its relation to similar organizations in other countries. After a definite plan had thus been formed, a general appeal would be made to all serious-minded men and women to unite and work together in the good cause.

The leading features of the ethical movement in this country are too familiar to need repetition here. In Germany, as elsewhere, the speculative confusion and uncertainty of this transition period have threatened to undermine morality itself. There, as elsewhere, there is, therefore, need for an organization which seeks a basis of morals, independent of religious dogma, and yet both firm and vital; and the bond of union between whose members is simply the hearty recognition of duty as the supreme law of life. The far-reaching and beneficial results of the schools established under the auspices of societies already in existence, in which the moral training of the young has been made a chief feature; the lectures and open discussions upon moral questions; and in fact, the general plan of work as now carried on, has proved so satisfactory that it will probably be adopted in Germany with such changes as the different conditions demand. The success of the movement everywhere must depend upon the enthusiasm and earnestness of those who share in it, and who, like the eloquent founder of the original society, aim at nothing less than the moral perfection of man.

C. H. S.

Einführung in die Moralphilosophie. By GEO. SIMMEL. Berlin, 1892, pp. 467.

Oughtness is analogous to the categories of being, and is a mode of thought like it. It is absolute, but hard to prove. The forms of

obligations give the social types. Egoism is more natural than altruism. It is empty and cannot evoke the maximum of will. The maximum of life is the chief egoistic duty. Resistance of temptation conditions desert. There is no guilt without an impulse to good. The ethical norm is the increase of the sum of happiness, which must be maximized. Happiness and virtue do not necessarily belong together.

L'activité des Animaux. By W. TENICHEFF. Paris, 1890, pp. 195.

The author, whose work is here translated from the Russian, considers first the conditions necessary to life, then to reproduction, and then how these conditions are fulfilled. The activity of animals is said in conclusion to be based on these conditions. Animals that most resemble men act more in accordance with our sentiments, and their actions can be predicted and explained. There are fifty-one cuts. The volume is neither scientific, philosophic, nor is it a successful popularization, nor does the author show familiarity with the literature of his subject. Why it was translated when there are so many better things in French, it is hard to see.

Les Facultés Mentales des Animaux. Par L. DE FOVEAU DE COURMELLES. Paris, 1890, pp. 352.

This volume, with its thirty-one commonplace cuts, regards insects as highest of all animals in the scale of intelligence. Beasts have the germs of all our faculties, even moral sense. We should be kind to animals. The author is evidently a lover of them.

Les Sociétés chez les Animaux. Par L. DE PAUL GIROD. Paris, 1891, pp. 342.

This volume, with fifty-three cuts, describes coalescence of lower forms: reciprocal association, like beavers; permanent association, like apes and flocks of mammals. Bees and ants occupy nearly one-third of the book and commensalism and parasitism are described. In seeking the causes of association the author starts with sexuality. The volume is a convenient compend, but contains little that is new, and the author undertook no observations of his own.

Les Industries des Animaux. Par FR. HOUSSAY. Paris, 1889, pp. 312.

This volume contains thirty-eight cuts. Nearly all the industries important for man are described, among animals whose work is determined by the form of organs. Man's intelligence and industry, although not essentially different, are higher.

L'Esprit de Nos Bêtes. Par E. ALIX. Paris, 1890, pp. 656.

This book and the one hundred and twenty-one pictures are new and interesting. Many stories are told, arranged under the convenient caption of impulses, instinct, will and sensibility, but there is no research, and the author proceeds as if the best of all proofs of a remarkable story were to draw a picture of the act. He has lived twenty years surrounded by animals, and holds beasts to be less brutal than we are wont to think. There is no fundamental difference that separates the acts of animals from those of man if the latter were similarly circumstanced. To establish this conclusion is the end of his book.

Der Zweck im Recht. Von RUDOLPH. Von IHERING. Two vols. pp. 570+723. Leipzig, 1893.

The third edition of this well written work of the great Roman Jurist at Göttingen, was in part only revised by him before his death. The present editor, V. Ehrenberg, now promises what is complete of the third volume. The work grew out of the author's conception that "purpose is the creator of all law," and this is his motto. The first part of the first volume is of especial interest to psychology. Zweck is defined as the inner stage of will and involves a sense that all being is conditioned,—contract, wages, credit, compulsion, all that is customary in morals, and even politeness is a product of teleology. Man cannot act without interest, Kant to the contrary notwithstanding. Ends become coincident; organized and egoistic purpose is identified with that of others. This work is the best illustration we know of the unity, intelligibility, and in a word "science," brought into a vast field by the use of a thorough-going psychological principle. Many other domains ought thus to be treated. Politeness is a form of personal protection, and raises the conditions of life far above mere decency. Respect is a sense of worth applied to personality, which rank and title are meant to defend. Good-will goes yet further and may even sanction conventional lies. The psychology of forms of courtesy was never better treated. Tact, taste and conscience anticipate law. The future ethics will include worth and rank, be the queen of social sciences, drop speculations and absoluteness, and show man, what he most needs to know, the next step. Ottingen's social-ethics first based Christian morals on an empirical foundation. Dueling, *trinkgelder*, drinking healths, Sunday customs, payment of bets, parliamentary rules, conventional mourning, smoking customs, illustrate different degrees of development of common purpose up to consciousness and formal enunciation.

Les Régicides. Par DR. E. RÉGIS. Paris, 1890, pp. 97.

This interesting pamphlet, with twenty portraits of regicides, treats them almost as if they were a class of criminals by themselves. False regicides are those who do not attack eminent persons as such. Such were Mariotti and Perrin, who shot at eminent men to win fame for themselves, and redress imaginary griefs against the state. Real regicides are Poltrot, who slew the Duke of Guise to remove an enemy of the church and thus to gain paradise; Balthazard Gérard, who killed William of Nassau to become a hero and martyr of the Church of Rome; Ravallac, who killed Henri IV. to prevent war on the Pope; Charlotte Corday, who slew Marot to save the republic; and so on down to Karl Sand, Orsini, Nobiling, Guiteau, etc. All are disharmonious natures, and degenerate. Some are half idiotic, others mystic in either politics or religion, others hallucinated or have delusions of persecution. Their methods, acts after the deed, writings, love of declamation, pride, courage, previous crimes, writings, etc., are described at length.

Des Hallucinations de l'Ouïe. Par G. DESCOURTIS. Paris, pp. 110.

This memoir received the Cervieux prize at the Academy of Medicine. It is a convenient compend of pathogeny, localization, effects of general hyperæsthesia, psychic or sensory hallucinations, consequences, credence, acts done resulting from relations to dreams, prognosis and treatment. Its relations to legal medicine constitutes the final chapter.

In the *Medical Record* of May 13, 1893, Dr. C. L. Dana of New York described a male patient, aged 36, with chorea, hereditary for five generations, always through the maternal side and generally developed after 30. He was trephined and a piece of skull 2x3 inches was removed; improved after it for a few months and then relapsed as before. With his consent a brain electrode was inserted in the shoulder and arm center. There was a convulsive movement of the arm and shoulder, the shoulder being fixed and the whole arm raised and drawn back a little. There was also a slight movement in the left foot, and a little twitching in the left face. The arm felt heavy and numb, as if the nerves were pressed. These sensations came and went with the movement. Repetition with a stronger current produced the same results intensified, but with no pain. From this the author infers a sensory correlative to the motor cause seated in the motor cortex.

University Studies. L. A. SHERMAN. Nebraska University. Vol. I. No. IV.

The words in the sentences of several writers of English classics were counted, to determine the normal average. Of recent writers, De Quincey was found not to deviate for any considerable period from an average of 32.73 words in the sentence; Macaulay, 23; Channing, 25.35; Emerson, 20.71; Bartol, 16.63. Of older authors, Chaucer gave 48.99; Ascham, 49.60; Lyly, 36.83; Fabyan, 63.02; Spenser, 49.82; Joseph Hall, 52.60. The same author shows no deviation from his average in his earlier and later writings. There is a distinct rhythm running through the works of an author. Long sentences may prevail for a few pages, but they are to be followed by several pages of short sentences in sufficient number that he does not differ from his norm for any considerable period. The greater sentence length among the older authors reveals greater predication, more past and present participles than in recent writers. The development of English prose, is toward the average for oral speaking. There is a larger per cent. of simple sentences in the later writers as compared with the older. In Chaucer and Spenser he finds 8 and 4 per cent. of simple sentences and in Macaulay and Bartol 40 and 45 per cent. respectively. The habit of dictating to stenographers is assisting in this movement toward the oral norm. The writers for *Fireside Companion* appreciate this and conform their style more closely to the style in oral speaking. 500 periods from *Saturday Night* yielded an average of five words to the sentence. This analytic process which appears in the development of English prose shows somewhat in the individual. High school and college students tend strongly to a heavy style, and the work of English instruction is largely the taming of the students down to practical oral standards. Children string their first articulate utterances together with many "ands." Later they learn to subordinate by conjunctions and then leave their conjunctions without verbs. The writer promises communications of further study. He does not touch upon the variable error—a point that would be interesting in the comparison of the older with the more recent writers.

T. L. BOLTON.

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RHYTHM.

BY THADDEUS L. BOLTON,

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INTRODUCTION.

Although experimental psychology began more than a hundred years ago through the discovery of the personal equation, it has as yet covered but a small portion of the field of mental phenomena. The nature of sensation, the time-relations of mental phenomena, memory, association, space and time concepts have been carefully studied by many eminent scientists. But the whole field of the emotions has been practically a *mare clausum* for psychologists. Several attempts to study and determine the nature of the simplest æsthetic forms have been reported; theories of pleasure and pain, supported by some experimental observations, have been advanced, but no serious attempt has been made to submit the emotions to experimental investigation. Every psychologist recognizes the necessity of doing so and that it is the most important field of mental phenomena, and that, until psychologists can reduce the motions to some semblance of order or more ultimate principles, experimental psychology can be said to cover only a part of the field of mental life. Every one is waiting for some one else to point out the way. There seems to be a general feeling that when once an entrance has been effected, the greatest difficulty will have been

surmounted and the whole field will yield to experiment. In a measure this is the true view to take, and yet certain of the emotions are as distinctly separated from others as the whole field is from that of the memory.

When the demand for such a study is so great, and students are being urged to make a trial, that one who does attempt it, though he fail, cannot be accounted rash.

The experimental study of rhythm which is to be presented in this paper, is an attempt to push the lines of exact science a little farther forward into a field that borders more closely upon the field of æsthetics than any other that experimental psychologists have tried. The attempt is to be made to reduce rhythm to a more fundamental activity of mind. The pleasure that individuals take in the rhythmic flow of words and sounds has been ascribed by one to the "Unifying Activity of the Feelings," by another to a "Sense of Order," and by still another to "The Feelings of Equality." Such explanations as these do not meet the question at all, unless it can be shown that such activities or feelings are ultimate facts of mind. If they are ultimate facts of the mind, it will be necessary, in order to make the explanation complete and valid, to show how they underlie other activities, for it is not to be supposed that any fundamental activity will manifest itself in a single phenomenon which bears no relation to other phenomena. Such does not seem to have been done by those who have offered explanations of the rhythms in speech, and the problem remains just where it was taken up. To regard rhythm as the manifestation or the form of the most fundamental activities of mind, seems a clearer view and to offer less difficulties than to regard it as an ultimate fact in itself. The problem, then, is to show how and to what extent it underlies mental activity, and, as preparatory to this, what part it plays in physiology and nature. Is there not some universal principle which is adequate as an explanation of rhythm in general?

Rhythm is so universal a phenomenon in nature and in physiological activity, and underlies so completely speech, that I desire to call attention to some of its manifestations in detail before presenting the experimental study.

Rhythms in Nature.—Natural phenomena very generally, if not universally, take a rhythmic form. There is a periodic recurrence of a certain phenomenon, sometimes accompanied by others, going on continuously in all that pertains to nature. Motion, whether in the broader field of the universe or upon the earth, is very generally periodic. Light, heat, sound, and probably electricity, are propagated in the form of waves. A falling body does not follow a straight line,

does a rifle bullet describe a simple curve which is the result of the combined forces of gravity and the initial velocity. Mr. Herbert Spencer has treated this subject in "Principles of Philosophy" at considerable length, and says but little that can be said here. Although he does not say so in so many words, he seems to hold that it is the most possible form of activity; continuous motion is an immutability.

Cosmic rhythms, however, are the most fundamental and important of natural phenomena. They may be shown to exist in a measure and be the cause of many other rhythms in inorganic and animal life. The regular alternation of light and darkness due to the rotation of the earth upon its axis is the most striking rhythm in the cosmos. The two periods of light and darkness constitute a unit—the day—which repeats itself always the same in length. Days are grouped into months by the revolution of the moon about the earth, and years by the revolution of the earth about the sun. These periodic changes have had a tremendous influence upon animal and plant life, and have stamped their impress upon all living organisms in the most striking manner; but, however, upon certain organisms more than upon others. In the vegetable kingdom some plants show a daily growth and repose; their flowers bloom in the morning and close in the evening. Some turn their petals towards the sun, and make a daily revolution in order to keep them so. In the higher latitudes all vegetation shows normal periods of growth and fruitage which are not necessarily cut short or retarded by early or late frosts. It requires a certain time for development without regard to the character of the season. The lunar period is known to influence the blooming of flowers. A species of Chinese roses blooms with a monthly regularity during the season.

The influence of these cosmic rhythms is not less upon the animal kingdom. The daily rhythm causes the daily periods of sleep and waking, from which no terrestrial creatures of higher types are exempt. The periods of sleep and waking are not determined by the effect of light and darkness as are the movements of many plants. The lunar period has had a far-reaching effect upon animal creatures, especially as regards reproduction and the nervous system. The periods of gestation and the recurrence of heat and menstrual flow in human beings and animals bear a very close and striking relation to the lunar period. The period of gestation in the lower mammalian animals is one month. In the higher it is a certain number of months. The time of incubation with some species of fowls a month, but it seems to

conform in general to a period of days which is a certain multiple of seven, seven being one-fourth of a lunar month. Fourteen, twenty-one and twenty-eight days are very common periods of incubation. The year exercises a still wider influence upon the animal kingdom. The normal life of most species of insects terminates in a single year. The frog becomes nervous and irritable with the approach of spring, although the conditions under which it is kept may not change. The polar bear goes into hibernation, even though he has not made the proper preparation in the way of a store of fat. The migrations of birds are not necessarily prompted by the signs of approaching winter. Animals breed generally in the spring—a fact which cannot find sufficient explanation in the influence of a warmer temperature. It has been fairly established that growth is more rapid during the summer months.

Although we find that these cosmic rhythms have stamped themselves upon the organism more or less permanently, they have wielded a far mightier influence upon the minds of men. Among primitive peoples that were rich in imaginative power, they have given rise to the most elaborate and beautiful systems of mythology and worship that the world has ever seen. It is a common speculation in childhood that, endowing animals at birth, as children do, with rational intelligence, but with a total lack of experience, the young creature must be driven to strange thoughts and speculations when the first light of day breaks in upon him, or when darkness approaches for the first time. What can be the thoughts of such a creature when he experiences the change of seasons or the first snow storm? No objects that are presented to the child so stimulate his thought and become such food for his fancy as the heavenly bodies and cosmic phenomena. Many of their minds are filled with myths about the stars that are as original and beautiful in conception, though lacking in detail, as much of the Greek mythology.

The recurrence of the day of the year upon which some event has happened is commemorated as a day of joy or sorrow according to the nature of the event. All national and religious festivals recur once a year. Among primitive peoples worship takes place always at the same time of day or year, and the same might be said of most enlightened people. There seems now, and always has seemed, a peculiar appropriateness in performing certain duties at the same time of day or year, although it does not necessarily depend upon the nature of the weather or of the event. The Christian Sabbath and other religious festivals, both savage and civilized, find their origin in the nature worship of the sun and the moon.

There are still other rhythms in the cosmos which seem to exercise an influence upon mankind. Sun spots make their appearance in great numbers once in about eleven years, and the attempt has been made to connect these with great financial disasters and religious awakenings which seem to recur in the same time. The social customs of the race show similar changes, which may prove to have some connection with sun spots. The coincidence warrants an investigation and allows speculation.

Upon the morbid side science has made discoveries of the most striking character. Even from the earliest times a periodicity has been observed in certain forms of insanity and in other mental diseases. These have been confirmed by later investigations.¹ Both crime and suicide show a periodicity which corresponds with the year, and another which corresponds to the larger period of sun spots.

Physiological Rhythms.—No fact is more familiar to the physiologist than the rhythmic character of many physiological processes. In physiology it means the regular alternation of periods of activity and periods of repose or of lesser activity. The term is also applied to any alternation of activity and repose, whether it is regular or not. These periods of activity and intervals of repose may succeed one another at very small intervals of time, as in the case of a clonic contraction of the muscle, or at very much greater intervals, as in the case of sleep and waking, or better still, in the periods of growth in children. Several of the most vital and important bodily activities are distinctly rhythmical, and will serve as types of all physiological rhythms. Of these, might be mentioned the pulse, respiration, walking and speech. The first two are involuntary actions, which in the very nature of the organism must be more or less rhythmical. Such actions are controlled by the lower nerve centres, and the organs concerned in them are connected in a reflex arc with these nerve centres. Habits are in the nature of involuntary actions. Of these, walking and speech are the most important and are true types of rhythmical activity. In each there is a series of coördinated muscles in which the contraction of one is the signal for the contraction of the next in the series, the last acting as a stimulus to the first.

Independent of the regular beat of the heart and forming a kind of higher grouping of these beats, the arteries undergo continuously rhythmical contractions and dilations of their

¹ Dr. Koster, "Über die Gesetze des periodischen Irreseins und verwandter Nervenzustände." Bohn, 1882.

Dr. Ludwig, "Periodischen Psychosen." Stuttgart, 1878.

walls, now increasing and now decreasing the blood supply. These may be observed with a glass in the arteries of a frog's foot or a rabbit's ear, occurring about once a minute. They may be made to cease entirely by cutting off the nerves going to these organs.¹ These arteries are controlled by the vaso-motor system, and the rhythmic contractions of the arteries seem to indicate a rhythm in the activity of the nerve centres. As we shall see later, there is some ground for believing that all nervous action is rhythmical. Regular contractions occur in the heart of some animals after they have been removed from the body, and are found to be due probably to the presence of nerve ganglia in these organs.² The effect of deficient arterialization upon the vaso-motor system is to cause a rise in the curve of blood pressure. This curve, then, shows certain undulations, which have been called Traube-Hering curves, from their discoverer. This result is obtained by cutting the vagi nerve and stopping respiration. The venous blood then acts as a stimulus upon the vaso-motor centres in the medulla, which causes these rhythmic movements. "This rhythmic rise³ must be due to the rhythmic contraction of the arteries, and this is caused by a rhythmic discharge from the vaso-motor centres."⁴ "The vaso-motor nervous system is apt to fall into a condition of rhythmic activity." A similar phenomenon has been thought to be observed in regard to the spinal cord.

When the spinal cord⁴ of a dog, cat or rabbit was cut, rhythmical contractions of the sphincter ani and of the vagina appeared. These contractions vary in number, but are generally about twenty per minute for the sphincter ani and four per minute for the vagina. The centre for these contractions was found to be in the spinal cord, about the level of the sixth and seventh lumbar vertebrae in rabbits and of the fifth lumbar vertebra in dogs.

¹ Foster's "Physiology," 6th Ed. p. 307.

Dr. Ellis, working under Dr. Bowditch, has studied these contractions in the web of a frog's foot with the microscope. He says that cutting the sciatic nerve does not stop them, and concludes that they are due to peripheral centres, unless he be allowed to suppose that automatic contractility is a property of smooth muscle tissue. Plethysmographic and vaso-motor experiments with frogs. *Jour. of Phys.* Vol. VI. No. 6, p. 437.

² Foster's "Physiology," 6th Ed. p. 357.

³ Foster's "Physiology," 6th Ed. p. 622.

⁴ Isaac Ott, "Observations upon the Physiology of the Spinal Cord." *Studies from Biol. Lab. at Johns Hopkins University.* No. II.

Fatigue shows itself to be a rhythmical process. Dr. Lombard¹ worked upon the flexor muscle of the second finger. After contracting the muscle several times, lifting each time a weight, he gradually lost the power of further contraction, but he continued to make the effort at regular intervals of two seconds. In a short time he regained his former power, which he maintained for several minutes, and then gradually lost it again. About five periods of alternating loss and recovery took place in twelve minutes. By variations in the methods of experimentation, the different factors are eliminated, and he is able to conclude that the centre of voluntary control is unaffected, but that this periodicity is dependent upon "alterations which take place in some of the mechanisms between the areas of the brain originating the will impulses and the centrifugal nerves." Dr. Hodge² found that when he stimulated the spinal ganglia of a cat continuously with an interrupted current, no change of the cell took place. When he applied his interrupted current for a quarter of a second and allowed the cell to rest three-quarters, a change took place in the nucleus of the cell. These experiments are inconclusive, as in the first case the animal was given curari and in the second it was not. Dr. Burgerstein³ tested a number of school children by their ability to multiply and add figures for four successive periods of ten minutes, with five minutes' interval between the periods of work. During the third period there was a marked falling off in the amount of work accomplished and an increase again during the fourth period. He argues that the pupils became fatigued during the first two periods, and that the third was a period of recovery, since the normal amount of work was shown again in the fourth.

The secondary rhythm observed in the circulation occurs also in respiration. Under ordinary circumstances respiration follows a rhythm of about fifteen or twenty a minute. During certain diseases and sleep a secondary rhythm—Cheyne-Stokes⁴ curves—appears in respiration. The respiratory movements decrease in depth until they disappear entirely. After an interval of a few seconds a slight movement occurs. This is followed by others, which increase in strength until they become normal and sometimes abnormally strong. Two explanations are offered: first, a waxing and waning in the nutrition of the respiratory centres, and second, a rhythmic

¹ Warren P. Lombard, M. D., "Effect of Fatigue upon Muscular Contractions." AMER. JOUR. OF PSY. Vol. III.

² "Microscopical Study of Changes due to Functional Activity in Nerve Cells." *Jour. of Morphology*, Vol. VII.

³ Die Arbeitskurve einer Stunde. *Zeitschr. f. Schulges.* IV. 9, 10.

⁴ Foster's "Physiology," 6th Ed. p. 605.

increase and decrease in the inhibitory impulses playing upon the centres. The latter explanation is favored. This, however, simply assigns the rhythmic action to some other centre and does not explain the phenomenon. A certain amount of secondary rhythm takes place in the breathing of hibernating animals. Respiration appears almost to cease and then to start again, but it is generally slower during hibernation.

Growth appears to take place rhythmically. Distinct periods of activity and rest occur in the embryonic development of some species that have been observed. This has been seen in the segmentation of pulmonates' eggs.¹ It is no less true of the amblystoma. In these the periods of activity last from five to fifteen minutes, and are succeeded by intervals of repose lasting about forty-five minutes. The activity of the protoplasm offers a resistance which must be overcome by the energy arising from the assimilation of the granular food material, which disappears as development proceeds. During the period of repose the energy is accumulating from this assimilation, which, when it becomes sufficient, overcomes the resistance, and activity sets in. This is taken to be a type of physiological and nervous activity, which will serve to explain certain phenomena of rhythm. This rhythm in growth, which is observed in the embryonic development, is characteristic of the physical and mental growth of children. For several years previous to puberty, great increase in stature is observed, puberty itself being a period of slow growth. From fifteen to eighteen is another period of growth, in which the full stature is generally reached. The mental character of children shows also periods of activity and repose.² The bright child becomes dull and the tidy slovenly. The leader in the athletic sports is now lazy and moping. Memory is now predominant, and now reason. The child passes from one form of activity to another. The line of development goes zigzag to its goal.

Other examples of involuntary action might be mentioned. These are the peristaltic contractions of the intestines, labor pains, the recurrence of heat and of the menstrual flow, and the secretions of the digestive cell. In these cells the secretions are kept up for about six hours, when a period of repose of about twenty-four hours follows.³

¹ W. K. Brooks, "Fresh Water Pulmonates." Studies from Biol. Lab. at Johns Hopkins Univ. Vol. II.

² G. Siegert, "Die Periodicität in der Entwicklung des Kindes-natur."

³ J. M. Langley and Sewell, "Histology and Physiology of Pepsin-forming Glands." Phil. Trans. Vol. CLXXII. pp. 663-711. London, 1882.

From this review it may be safely said that nervous action in general, and especially of the lower and vaso-motor centres, is rhythmical. This form of activity results from the resistance which the nervous substance offers to a stimulus. A certain amount of energy is necessary to overcome this resistance. This fact is brought out by the experiments of Helmholtz¹ and Sterling upon the summation of stimuli. Helmholtz found that when he stimulated a nerve going to a muscle by a submaximal stimulus and then added another stimulus at any time afterward within four seconds, he obtained a contraction. If he used a maximal stimulus in the first place and then added another stimulus during the latent period, it produced no effect upon the contraction due to the first stimulus. But if the second stimulus was added after the latent period, the effect was a greater contraction than that which followed the first stimulus alone. Submaximal stimuli² following one another, even as slow as one per second, will produce a contraction after a time. As the frequency of the stimulus increases, the effect is much more marked. It is much better to increase the frequency of the stimulus without increasing the strength than to increase the strength alone. Sterling adds further that all muscular and nervous action is due to summated stimuli—a conclusion that denies the possibility of contractions due to one instantaneous shock or at least does not explain them. Dr. Ward³ determined that between the rates of .4 sec. and .03 sec. a contraction always followed a given number of stimuli. Above and below these limits the number might vary. In the same line is the work of Drs. Kronacker and Hall.

It has been held by Sterling and others that when a stimulus is applied directly to the cortex, no matter what the rate, the brain sent out rhythmic impulses always at a constant rate. Dr. Limbeck⁴ conducted a series of experiments upon the brain and spinal cord, in which he finds that the brain and spinal cord send out just as many impulses as they receive. Faster rates than thirteen shocks per second for the cortex and thirty-four for the cord, gave smooth curves.

¹Helmholtz, "Berichte der Berliner Akad." 1854, p. 358.

²W. Sterling, "Über die Summation electrischer Hautreize." Berlin. Berichte d. Sachsgesellschaft d. Wissenschaft. December, 1874, p. 372.

³Dr. Ward, "Über die Auslösung von Reflexbewegungen durch einer Summe schwacher Reize." Archiv für Anatomie und Physiologie. 1880, p. 72.

Hugo Kronacker und G. Stanley Hall. Die willkürliche Muskelaction. Archiv für Anatomie und Physiologie, 1879.

⁴Dr. R. U. Limbeck, "Über den Rhythmus centraler Reize." Archiv für experimentale Pathologie. Bd. XXV. H. 2.

The difference between the rates for cortex and cord is worthy of note in consequence of the close correspondence of the number of shocks for the cortex and the rate of the most rapid voluntary control; while involuntary and clonic contractions which find their seats in the lower centres and in the cord may be much faster. In this connection the attention of the reader is called to that portion of the experimental study in which the rate of clicks at which rhythmical grouping ceases is set forth. It is not far from ten a second. This is also near the lowest rate at which air vibrations give the impression of a musical tone.

The theory of summated stimuli which was advanced by Wundt, and which is generally accepted, is based upon the resistance which a central cell offers to a stimulus. The incoming stimulus is not communicated directly to the cell. The afferent nerve does not terminate in the cell, but breaks up into branches, which form a kind of envelope about the cell. The efferent nerve takes its rise in the nucleus of the cell and proceeds towards the periphery. If the stimulus is weak, it does not penetrate through the surface of the cell to the nucleus, but only part way. It sets up a kind of disturbance around the surface of the cell and, should another stimulus follow before the disturbance has subsided, it adds to the effect already produced. Repeated stimuli still further increase the disturbance until it penetrates to the nucleus of the cell, when it causes the cell to discharge into the efferent nerve. This serves very well for summated stimuli, but other phenomena of just the opposite nature require explanation. There are the soothing effects of slow and gentle stroking or patting, such as hypnotizers and nurses use upon their subjects. The general fact seems to be that the stimulus must not rise much above the threshold, and be sufficiently slow, that there shall be no summation. As we shall see later, any repeated stimulus tends to take the form of a muscular movement accompanying it. If this stimulus becomes gradually slower, it leads finally to the concept of rest, and being accompanied by muscular movements, these movements must finally cease. Increased quietude follows the slowly decreasing movements, until before a great interval of time has elapsed the body falls into a state of rest. The stimulus must in any case be sufficient to command the attention of the subject to the exclusion of the disturbing effects of other stimuli coming from without and from the involuntary processes of the body. Let us return now to the nerve cell, to find if there are any processes going on which will throw light upon the problem. A weak stimulus is continually playing upon the cell from without, but never rises sufficiently

in strength to penetrate beyond the periphery of the cell or in rapidity to bring about a summation. The effect of each stimulus subsides before the following one reaches the cell. The peripheral area must soon become fatigued so that it is no longer able to respond to the stimulus, and yet it is sufficiently strong to command the attention in so far as to distract it from other stimuli coming from within. This is the condition of quietude in the cell which is manifest in the muscle.

Attention and Periodicity:—The most casual observer will discover that his attention is discontinuous and intermittent. It manifests itself in a wave-like form. It is a series of pulses. The mind does not rest for any length of time upon a single object. New phases and relations must continually appear, or the object is dropped, that another may be taken up. "No one can possibly attend continuously to an object that does not change."¹ This process has been described as a "fly and perch." Charles Pierce says in his "Philosophy of Attention" that there is "no continuum." This periodicity in attention has been observed by Helmholtz² with the stereoscope and commented upon at considerable length. The phenomenon is called retinal rivalry. Mr. T. Reed³ records some observations which he made in combining two stereoscopic views, which were ruled, the one with vertical, and the other with horizontal lines. He finds that the whole field will be occupied for a time with one view, and then this gives way for the other, which lasts an equal time. They seem to change without voluntary effort and even in spite of one's efforts to keep one view in the field. The full time for a change from one to the other and back again is from twelve to sixteen seconds for different subjects. The pulses of attention, however, seem to succeed one another at much shorter intervals. Two seconds seem a long time to hold any object which has no relation before the attention. James says: "There is no such thing as voluntary attention sustained for more than a few seconds at a time." Does it not, then, seem reasonable that during each wave or pulse of attention only one undivided state of consciousness can arise? The waxing and waning of attention seem to mark a change from one object of consciousness to another. The object of the state may be very complex, but it stands as a unit in consciousness. The problem of the relation of the parts of the object by which a great many may be allowed to stand as

¹ James, "Psychology," Vol. I. p. 420.

² "Physiologische Optik," Sec. 32.

³ "Nature," August 1, 1887.

a unit in consciousness and be grasped in a single state, is of the most vital importance, but it must be deferred until later, when the normal period of a wave of attention will also be discussed.

Rhythmic Speech:—The most distinguishing, and in many respects the most important, function of the human body is vocal utterance and articulate speech. Being an involuntary and habitual function in a large measure, it might be expected upon *a priori* grounds to be rhythmical. Speech becomes rhythmical not simply by sounds succeeded by pauses, but also by the regular recurrence of strongly accented sounds in a series. Aside from the simplest shout or exclamation of joy or pain, all vocal utterances are primarily rhythmical. Every word that contains more than one syllable consists of strong and weak syllables. These accents occur upon every other syllable in varying intensity, or at most the accented syllables are separated by two unaccented syllables. As regards vocal utterances, they can be considered from four different aspects—their regular succession, intensity, pitch and quality. The problem in a philosophical treatment of rhythmic speech is to determine the value of these properties of sound as unifying elements in a rhythmical production. It will be necessary first to inquire which is the most fundamental, and secondly, where each enters and the part it plays in the development of literature. We must seek also other unifying principles, if such there be. Of these, we might now mention the logical meaning of words—the theme—and æsthetic forms. As we are concerned in speech in so far only as rhythmical effects are aimed at, we shall speak only of poetry. By what coördinations and subordinations of sounds with respect to their properties and meanings is the whole structure of the poem held together? It is the same problem which Plato discussed as the one and the many. Kant put the same question by asking how the mind made a unity out of a manifold. We have to ask how the mental span becomes so enormously increased as to grasp such a poem as Wordsworth's "Intimations of Immortality from the Recollections of Childhood," or Milton's "Paradise Lost." How is the carrying power of the mind increased to such an extent? The answer is to be found in the fact that unities are formed out of the simplest elements of speech by coördinating some with others in respect to their time relations; secondly, unities are formed of unities by subordinating them with respect to their intensities, and sometimes, their time values; thirdly, by coördinations and subordinations with respect to intensities and qualities, higher unities still are formed; and

fourthly, by coördinations and subordinations with respect to theme and æsthetic forms, the greatest unities are accomplished. In the first place vocal utterances are related as regards time, that is, the same sound may recur at regular intervals, in which case the series thus formed might be termed a *rhythmic* series—a series which may become *rhythmical*. In the next place this series might be made up of louder and weaker sounds alternating with each other. The series would then be composed of groups of sounds and might be called a *rhythmical* series. This is a rhythm in speech. If now the louder sounds in each group were given different intensities, these smaller groups might be brought into larger groups still. In this way the mental span may be made to extend itself over a very large number of simple impressions. The principle is very clear, and one will see at a glance that if intelligible sounds were used and qualitative changes employed, the mental span might be almost indefinitely extended. The carrying power of the mind, however, does not rest wholly in any case upon a single fact, if we make the exception that vocal utterances must be carefully timed in a *rhythmic* series. Quality and pitch changes accompany changes in intensity, so that the subordination of one sound to another and their consequent unification with respect to intensity is always dependent upon pitch and quality changes as well. For this reason it is impossible to treat each properly by itself.

Time-relations:—In order for vocal utterances to form a *rhythmic* series, they must occur at regular intervals of time which cannot exceed or fall much below certain limits. We may, however, upon the analogy of physiological rhythms, regard a series of sounds recurring at stated intervals as a *rhythmical* series, and also regard the recurrence of accented sounds as forming a secondary rhythm out of the primary. This is carrying the *rhythmical* idea farther than has been customary, and while it is more nearly correct, it would not be generally understood. The question of the time values of vocal utterances for *rhythmical* purposes cannot be answered upon an examination of poetry itself. Although the Greeks and Romans assigned exact values to all syllables in their language, there is reason for believing that such values did not arise naturally, but were assigned when they began to speculate upon poetry. No such relations exist among the syllables of modern languages, and in English they never did. We must then dismiss the subject of time and its significance and revert to it as the subject permits.

Intensity of Sounds:—The mind accomplishes its first real unification of sounds by subordinating them with respect to their intensities. A rhythm in speech means a series of groups of sounds. Each group may contain two or more sounds, generally not more than four. Two sounds, one strong and one weak, the one succeeding the other in time, cannot give an idea of a rhythm, but two groups of two such sounds certainly can. This being the simplest possible rhythm, we should expect that it would be the earliest form in which literature appeared. Since we have not probably any extant specimens of the first literary productions, for they were not committed to writing, we must judge from those which have come down to us from later periods, and from the literature of primitive peoples and of children, what the earliest form was. In this way it has been proved that our surmise, which was made upon *a priori* ground simply, is correct. The oldest extant specimens of English poetry are generally composed of verses of two sections, which are separated by a pause in the middle. Each section generally contains four, sometimes six, syllables, two of which are unaccented and two accented. The first section was emphatic and corresponded to the accented syllable in the smaller division; the second section received less stress and was less important. The two formed a kind of balance structure, in which the first section contained a rise and the second a fall.

helle heafas: hearde nithas.
wer leas werod: waldend sende.
graes ungrande: gar secg theahte.¹

"Our Anglo-Saxon² poems consist of certain versicles, or, as we have hitherto termed them, sections, bound together in pairs by the laws of alliteration. . . . For the most part these sections contain two or three accents, but some are found containing four, or even five. The greater number of these sections may be divided into two parts, which generally fulfill all the conditions of an alliterative couplet. . . ." These are the rules that Guest gives according to which the elementary sections were constructed: 1. "Each couplet of adjacent accents must be separated by one or two syllables which are unaccented, but not by more than two." 2. "No section can have more than three or less than two accents." 3. "No section can begin or end with more than two unaccented syllables." "When the accents of a section are separated

¹ These lines are copied just as they appear in Guest's "History of English Rhythms," p. 189.

² Guest's "History of English Rhythms," p. 158.

by two unaccented syllables, the rhythm has been called triple measure; and the common measure, when they are only separated by a single syllable." The greater proportional number of accents makes the movement slower, and adapts the measure for more solemn and graver subjects. The triple measure is more suited to lighter themes. The verse of the common measure is made more energetic by being begun and closed with accented syllables. They are abrupt when too short, and become feeble when too long. There was considerable variety of rhythm as early as the fifth century, "as there certainly was in the seventh century, when Cædmon wrote."¹ "It is, however, probable that the rhythms were of a simpler and of a more uniform character." "Most of the alliterative couplets have only four accents — very few, indeed, have so many as six."

The phenomenon of accompanying the changes of intensity in a series of sounds with muscular contractions, led to the early association of dancing with musical and poetical recitation. Indeed, if we accept the current theory of the origin of language as arising during the celebrations of victory, dancing precedes even language. Just as an animal jumps and frisks about as an expression of pleasure at seeing his master, so our ancestors danced for joy over a victory, or in the worship of their deity. They emitted certain vocal utterances in company with the tramping of the feet, which in time came to have definite meanings and also took on the rhythm of the dance. This rhythm was scarcely more than the simple swaying of the body or the lifting of one foot and now the other. Variations in the dance might occur either in taking several steps forward and then several backward, or to the right and to the left. These variations would produce corresponding effects in the vocal accompaniment. The step of one foot would be stronger and a more intense sound made to correspond to it. In the same way either the forward or backward movement would become the more important and give rise to the distinction of thesis and arsis of the verse. Further groupings of the verses might take place in the same way. The two-rhythm was apparently the prevailing rhythm in the history of our language, if not in some others. The most common foot in our literature of all times, and a very common foot in the Greek literature, consisted of two syllables; two feet entered into the section, and two sections formed an alliterated couplet or verse. It is the simplest possible rhythm, and corresponds to the leg-pendulum with which the language was so intimately associated in its earlier history.

¹ Guest's "History of English Rhythms," p. 169.

Noiré¹ believes that language took its rise in the concerted action of many persons. In this way the individual finds that what belongs to him is the common character of others. Such utterances as "hi-ho" are taken to be the first beginnings of language, and they originate during concerted action. Any sound that is to become intelligible must first be experienced in company and then by the individual alone. But, as the example shows, such utterances are rhythmical. Here it is the rhythm of heaving sails or anchor, which is seen among sailors.

Variations in the number of syllables to the accent would be a necessity as a relief from the monotony of two syllables to the accent, and so, too, the number of accents to the section would be increased on account of the abruptness of the doubly accented section. Taine² in speaking of early Saxon poets says: "His chief care is to abridge, to imprison thought in a kind of mutilated cry." "They (Saxons) do not speak, they sing or rather shout. Each little verse is an acclamation which breaks forth like a growl. Their strong breasts heave with a groan of anger or of enthusiasm. A vehement or indistinct phrase or expression rises suddenly, almost in spite of them, to their lips." After the people became settled down in their new homes, they lost the ruder and rougher characteristics, and such wild outpourings would be no longer suited to their milder spirits. The changes that took place in the development of our literature are due in some measure to the change in the life and habits of the people.

There still remain in our poetical compositions certain evidences of some, at least, of the stages through which our poetry has passed. The choruses in many of our hymns are still made up of non-sense syllables. Irish melodies and popular songs retain this feature. Children's poetry — by that I refer to such poetry as they enjoy and recite for their own amusement — has a large element of purely unmeaning sounds in it. Savage dances are often accompanied by recitations in which no meaning has been discovered. Again, savages and children are frequently found repeating for their own amusement a series of non-sense syllables in rhythmical form. The accents are very strongly marked, and frequently enforced by alliteration. The incoherent chatter of a maniac, or the sound of a foreigner speaking his language to one who is unacquainted with the language, is distinctly rhythmical. It is more like a chant, and children frequently remark upon

¹ Ludwig Noiré, "Max Müller and the Philosophy of Language."

² Taine, "Introduction to the History of English Language."

it. It appears, then, that vocal utterances which are kept up for a considerable time fall into a rhythmical form. Such being the natural tendency of speech, it would conform itself to any rhythm with which it might be associated, and as vocal utterances were always accompanied by the dance, it would take on the rhythm of the dance, which in its earliest forms we have seen reason to believe was the leg-pendulum.

The poetry of children shows a character very similar to early English poetry. It consists often of a two-section verse which is strongly alliterated, and in which the rhythm is perfectly clear. The familiar incantation rhyme shows this characteristic very well.

Sticks and stones
May break my bones,
But names will never hurt me.

Again,

Jack and Jill
Went up the hill
To fetch a pail of water;
Jack fell down
And broke his crown
And Jill came tumbling after.

At the beginning of each couplet there is wanting one syllable. Their poetry is usually accompanied by marching or by clapping of the hands, so that they require an accented syllable at the beginning. The verse is, then, an alternation of accented and unaccented syllables; occasionally only two unaccented syllables occur between two accents. We have in the first couplet what was found to be a prominent characteristic of early English poetry.

E. B. Taylor in his "Anthropology" asserts that while meter, and by that he means lines regularly measured in syllables, is an evidence of civilization, one of its earliest developments is matched and balanced sounds. The Australian savage sings at the end of his verse, "A bang! A bang!" Certain of the North American Indians sing in choruses, "Nyah eh wa! Nyah eh wa!" The chorus of a New Zealand song is "Ha-ah, ha-ah, ha-ah, ha!" A feature extremely common in barbaric song is a refrain of generally meaningless syllables. Guest,¹ speaking of our early poetry, says, "I have hazarded the opinion that these short, abrupt and forcible rhythms were the earliest that were known to our language. They are such as would naturally be prompted by excited feeling, and well fitted for those lyrical outpourings which form the earliest poetry of all languages." The abruptness

¹Guest's "History of English Rhythms," p. 365.

is felt by children, so that not more than a single couplet appears without the intervention of a different kind of verse. Shakespeare¹ adopted this measure in his descriptions of fairyland, and it is now become the fairy dialect of the English language.

Qualities of Sounds:—Qualities of sounds are quite as important as unifying elements as their time and intensity relations, and were quite as early regarded. This is manifest from the frequent recurrence of the same sound at the beginning of Anglo-Saxon and Germanic verses. This is alliteration. The two sections of the verse, while contrasted in intensity, were coördinated by the recurrence of the same sound. The origin of alliteration is involved in some mystery, and yet the savage shouts just quoted point out a possible origin. The emotional shout of an animal for a given state is always the same; but for the savage, who possesses greater powers of utterance, emotions find various expressions, or at least, if the expression begins with the same sound, it ends differently. Although the New Zealand savage shouts "Ha-ah" several times in succession, he closes with "Ha!" When the child torments his companion in the midst of misfortune, he says "Goody, goody gout." Other expressions of a similar character, but used with a different purpose, are "higelty, pigelty," "hee-ho," etc. In modern poetry alliteration has given place in a very large measure to final rhyme, which has become the unifying factor for the verse generally in English poetry and always in French. The qualities of sounds gave rise to melody in speech, which is common to both poetry and music, and it is as melody that the qualities of sounds play the most important part.

Spencer holds, in his essay upon the origin of music, that different emotional states produce different intonations and changes in pitch, quality and loudness of vocal utterances. In the savage dances of victory, worship, and love, emotional speech grew up, and from this music arose. Originally music was recitative—a mere chant. Chinese and Hindoo music is still so. This recitative speaking grew "naturally out of the modulations and cadences of strong feeling." The Quaker preacher who speaks only when moved by religious emotion, speaks with a recitative intonation, and church services of the present day are generally read so. This is really melody. Recitative speaking, or emotional speech, constitutes the whole of savage poetry.

Poetry and music among primitive peoples were the same. Poetry was either sung or chanted, and it was not until a

¹ Guest's "History of English Rhythms," p. 179.

later period that they became separated. With the discovery of the musical instrument, the people saw that a melody was just as well expressed by simple tones as by intelligible syllables, and music took up its own lines of development.

The Emotional Effects of Rhythm upon Savages and Children:—There is no more striking fact in the whole field of rhythm than the emotional effect which rhythms produce upon certain classes of people, savages and children. Attention has already been called to the psychological phenomenon of accompanying the changes of intensity in a series of sounds by muscular movements. So strong is this impulse in all classes of people that no one is able to listen to music in which the rhythm is strong and clear without making some kind of muscular movements. With some people these movements tend to increase in force until the whole body becomes involved and moves with the rhythm. The accents in the rhythm have the effect of summated stimuli, and the excitement may increase even to a state of ecstasy and catalepsy. Although the regular recurrence of the accented syllable is the most important element, the qualitative changes aid in bringing about the emotional states. Soothing effects result from certain rhythms, as is shown in the lulling and patting of the baby to sleep. The early hypnotizers resorted to the gentle stroking of their subjects. Savages are well aware of the exciting effects of certain rhythms, and are accustomed to use them to bring about the state of frenzy in which their priests give their prophecies and in which religious dances are danced. Mr. Ellis,¹ who has made a study of some tribes in Africa, says, "Music amongst the Thsi-speaking tribes is limited to airs possessing an obvious rhythm. Such airs seem to appeal to the primitive sense common to all people, but upon savages, that is, upon children with the possession and power of men, its influence is immense, and the state of excitement into which an assemblage of uncivilized people may be wrought by the mere rhythm of drums and the repetition of a simple melody would hardly be credited. . . . With some races this known emotional influence of music has been utilized with three objects, viz., to stimulate the religious sentiments, the martial spirit, and the sexual passions."

In the Yatiati² dance among the Indians of British Columbia, the tribe assembles outside of the chief's house in which the dance is to be held, and with fists and sticks they beat the time on the walls as they enter, singing the dancing

¹ A. B. Ellis, "The Thsi-Speaking Peoples of the Gold Coast of West Africa," p. 325.

² Franz Boas, *Jour. of Amer. Folk-lore*, Vol. I. p. 49.

song. The dancers who are on the inside are worked up into a frenzy. The gentle striking at first, gradually increasing in violence, and the slow approach and the assemblage of the tribe, wrought in the dancers a pitch of excitement which forced them to rush out after a time and begin the dance, jumping about in the wildest fashion. Such dances cease only with the complete exhaustion of the dancers.

The Patagonian wizard¹ begins his performance with drumming and rattling, and keeps it up till the real or pretended epileptic fit comes on by a demon entering him. Among the wild Veddas of Ceylon the devil dancers have to work themselves into paroxysms to gain the inspiration whereby they profess to cure their patients. With the furious dancing to music and the chanting of attendants, the Bodo priests bring on a fit of maniacal possession. The excitement is allowed to continue until the prophet falls to the ground in a swoon. When the Alfurus of the Celebes invite their deity to descend among them, the priests, standing about the chief priest, upon whom the deity is to descend, chant some legends. A slight twitching of the limbs marks the beginning of the possession. The priest turns his face towards heaven, the spirit descends upon him, and with terrible gestures he springs upon a board and beats about with a bundle of leaves, and leaps and dances, chanting some legends. He falls in a swoon, and the sounds he emits are interpreted as the will of the spirit.

George Catlin² says dancing is always accompanied by the singing of mysterious songs and chants, which are perfectly measured and sung in exact time to the beat of the drum, always with an invariable set of sounds and expressions.

The religious services and singing among the Shakers are often accompanied by dancing, and more frequently by beating of the time by all the members of the congregation. The excitement among them never rises to an extreme degree. A highly civilized people is not easily affected by mere rhythms. A simple tone is not so expressive as it is to the lower classes of people. The negro preacher often resorts to recitative speaking to produce the desired emotional state in his hearers, which is generally known as the "power." He selects some short sentence, often unimportant, such as "Moses went up into the mountain," and repeating this, at first softly, he gradually raises his voice to the highest pitch, at the same time increasing his gesticulations. The more excitable of his audience are thrown into a paroxysm; the con-

¹ E. B. Taylor, "Primitive Culture."

² George Catlin, "Letters and Notes upon the Manners and Customs of North American Indians."

tagion spreads so that sometimes the whole audience is involved. Evangelists among all classes of people rely more or less upon the emotional effect of rhythmical speaking. Street hawkers and fakirs generally speak with a recitative intonation. Their success depends very largely upon their success in alluring and holding the attention of the crowd by the manner and intonation with which they speak.

The effect of rhythm and clearly accented music is no greater upon primitive peoples than upon children. Although children are not allowed to go into ecstasies, the clapping of the hands to the recitation of "Peas porridge hot" is akin to the terrible leaping and gesticulations of the savage to the accompanying tom-tom and the chanting of his ancient legends. The child usually begins his recitation of "Peas porridge hot" rather slowly, and as he continues he grows in excitement and enthusiasm, his gestures become more violent and rapid, until he breaks down in the excitement. It is a well-known fact among school teachers that young children become excited whenever they sing rhymes with a strongly accented rhythm. Several have made this observation during the singing of a certain line in Theodore Tilton's "Baby Bye." The line in which the excitement reaches its climax is,

There he goes
On his toes
Tickling baby's nose.

This is a type of the fairy measure. The accents are strong, and every line is preceded by a pause, and at the same time all the lines are rhymed. Both the rhyme and the pause lend an intensification to the rhythm that is sufficient to call out the greatest excitement in the fairy people. In Robert Browning's poem of the "Pied Piper of Hamelin," whose charm was rhythm, occurs this remarkably rhythmical passage, and taken with the context might easily cause some emotional excitement:

Into the streets the piper stept,
Smiling at first a little smile,
As if he knew what magic slept
In his quiet pipe the while.

I have the testimony of an eminent educator that, when he read these lines, and he is an effective reader, his boy, a youngster of five or six years, would run away and hide where he could not hear the reading. He was apparently unable to bear the strain of the excitement. In later years the boy could not tell why he did so, except that it disturbed him.

The use by children of incantation rhymes for purposes of injury and torment to their companions is interesting in this connection. The habit of rhyming is almost instinctive with them. Imagine the effect of such a couplet as this upon the child to whom it is addressed :

Good night,
Rosie Wright.

Again, any name may be put in certain adaptive rhymes which are current among children. These, however, are not so effective as the instance cited above. They admit of retort. The drawing out of a name in a sing-song measured tone is very effective, and the easy adaptation of some names makes the child who is unfortunate in having such a name an object of torment.

The Place of Rhythm in Music and Poetry. *Music*.—We have seen how music and poetry took their rise together from the emotional utterances of savages during the dance, and how these emotional utterances gradually took the form of recitative speaking. This gave rise to the melody, though it was not disassociated from the meaning of the words. With the discovery of the musical instrument came the discovery that a melody might be sustained by simple tone intensities. Although music finds its essential basis in rhythm, its distinctive feature is the melody combined with harmony. The melody is constituted of a succession of tones which are significant of an emotional state, and when several melodies are combined and sung together, they give rise to harmony. This combination of melodies depends upon the pitch of the sounds. The melodies in harmony are all subordinated in different degrees to one dominant melody which is higher in pitch than the others. The unifying element here is pitch. This is the only distinctive use that is made of it in either music or poetry. The most important and fundamental unifying principles underlying music is the time, without which there can be no music. Musical tones must be exactly timed, if one is to get the conception of a melody from a series of tones. When they are exactly timed they may be farther unified by regular changes of intensity which group the sounds into measures. The most common measures that occur in music are 2-4, 3-4, 4-4, and 6-8 time. In what might be termed the natural system of accents, the first note in each measure receives a strong accent. This is really the only accent in 2-4 time. In 3-4 time the second note also receives an accent, but it is weaker than the first. In 4-4 time there are four grades of intensity. The first note is the strongest, the third next, the second is weaker still,

and the fourth is the weakest of all. In 6-8 time the third, fifth and sixth are of about equal intensity, and weak. The first is strongest, the fourth is next, and the second weaker though stronger than the third. An equal amount of time is given to each measure—that is, the strong accent occurs at regular intervals—but the distribution of this time among the notes in a measure may be greatly varied; the separate notes, however, always bearing constant and simple relations to one another. The smallest fraction that may express the relations of these notes is $1/64$, and this appears only in instrumental music. In poetry, as we shall see, there is not so much freedom; it has deviated less from the primal rhythmic stock from which both spring. For many centuries music consisted wholly of melodies, or of a single melody. The idea of combining or singing several melodies at the same time came very much later. This is harmony. It reached its highest development about Elizabeth's time, when the attempt was made to combine as many as forty melodies. A much smaller number was found to give better effect, and the number now used is generally only four. Symphony was a still later development, but the general feeling among musicians now is that it culminated in Beethoven, and its further development in music is impossible. Although the term has had several significations in the history of music, in Beethoven it was the combination of several themes in such a way as to bring about a succession and combination of strong emotional states. The musician who desires now to produce new effects, turns to the Volks-Lieder for a theme. He aims at variations of the rhythmical effects and introduces new harmonies. Mendelssohn is said to have remarked, when he heard some of the negro melodies of our slaves, that here was a field for a great musical talent. Wagner, taking the suggestion, has made such an adaptation of the Hungarian melodies, and with what success the musical world is well aware. Wagner has made a real advance, and for some time musical composition will follow his lead. Although there is a feeling among musicians that rhythm is distasteful, it is more apparent than real. It is the regular monotonous recurrence of the same rhythm without sufficient variations that is displeasing and not the rhythmic flow itself.

Rhythms in Poetry:—We have already seen that when language appeared as literature, it took the form of the simplest possible rhythm. Even then it was the vocal accompaniment of a dance, and there are many analogies to the simple swaying of the body or the tramping of the feet in the march. There were no fixed rules in regard to the number of syllables to the measure. The verse, so far as we can

speak of a verse, consisted of an alternation of accented and unaccented syllables. Very generally it began and ended with an accented syllable, so that a pause occurred between each verse. The line of development along which poetry followed was an increase in the number of unaccented syllables as compared with the accented, and also an increase in the number of accents to the verse; the verse preserving for some time the same balance of structure that it had in the beginning. The number of accents then might be four, six or eight; the latter number never became popular, for the reason, it would seem, that it exceeded the normal mental span. This even and balanced structure could not hold out forever; a demand for variety and the influence of foreign rhythms contributed to overthrow it, so that Chaucer wrote altogether in a verse of five accents, but he still retained the middle pause. This came after the second accent or just before the third, though sometimes after the third also. There were many verses in which the first section more generally contained three accents.

Guest takes no account of the measures or feet in English verse. He divides lines into three general classes: Those that begin with an accented syllable, those that begin with one unaccented syllable, and those that begin with two unaccented syllables. The varieties in each of these classes depend upon the position where the variation occurs from the form in which the verse sets out. Should the verse begin with an accented syllable and continue with an alternation of accented and unaccented syllables, it would constitute one variety. If, however, two unaccented syllables occur between any two accents, it would constitute a different variety according as the two unaccented syllables occur between the first and second accents, the second and third, and so on through the verse. Early poetry was sung to the accompaniment of the harp and hence was sung in exact time. On this account Guest says that up to the fourth century, English rhythms were temporal and then became accentual. Previous to that time the syllable had a time value. This, however, is not to be taken in any absolute sense. Poetry was chanted in a kind of trance state, and the reciter aimed to produce such a state in his audience. For this purpose the thought was of minor importance. Great dependence was placed upon the rhythmical flow, and doubtless a very exact time was given to the syllables that the movement might be clearer. A rhythm which depends wholly upon either the time element or the accent, is certainly less forcible than one which combines both factors. It must be conceded that though some regard was paid to the time of syllables, no such exact time was main-

tained as modern musicians keep in their music. Perfect time is the result of the application of scientific methods to music. Poetry has never lost the time element entirely, for accents that occur at irregular intervals could not have been but very displeasing, and they are now. It is reported of some of our modern poets, and especially of Tennyson, that they read their poems with the strictest observance, not only of the accents, but of the time, showing that they regarded the time element of great importance. Many readers and teachers of English poetry pay little heed to the regular recurrence of the accent. For them the thought is the chief element in poetry, and in attempting to bring that out, they disregard the rhythmical flow. But when the proper observance of the thought does violence to the rhythm, the poet must be adjudged lacking poetic inspiration, and to that extent his poetry is not true poetry. It is to the great renown of Chaucer, Milton and Shakespeare that there is such a perfect adaptation of the rhythm to the theme in hand, and any lack of observance of the accents by the reader betrays his want of understanding of that which he reads. The strict observance of time in music and the unity of origin of poetry and music, which argues that time was once an essential element of poetry, show that the time element is still there, unless it can be shown when and why it has dropped out. Poetry has admitted fewer variations and allows a greater prominence to the rhythmical flow than music. It must be admitted, however, that the thought has taken the place of the melody to a great extent as the unifying element, but it cannot be allowed to take the place of other factors. Whenever it does, just so soon the composition fails of being in any sense poetry.

Alliteration, which was very prominent in Anglo-Saxon, was gradually lost. The influence of the church and of Latin scholarship aided somewhat in this movement, but as the Anglo-Saxon element prevailed against all foreign influences in the political and social affairs, it won the day in the struggle against the Norman and Latin languages. Our language remains essentially Anglo-Saxon, and alliteration, though less common, is still a prominent feature of our poetry. Originally, alliterated syllables marked the beginning of the section and constituted the unifying factor of it, but there was no strict observance of such a principle, except that the alliterated syllables were accented. They might come anywhere within the section. The use of alliteration by later English poets was to place the alliterated syllables away from the beginning of the section and to put them in the same verse. The purpose of alliteration is not to coördinate two sections or two lines, but, by intensifying certain accents in the verse,

to make a more perfect subordination of them, or to make a more perfect unity of the line. Final rhyme succeeded alliteration. The chief reason seems to have been for a more emphatic or distinguishing mark of the rhythm than could be obtained through accents alone; especially when run-on lines came to be used and the thought was about to usurp everything. When two successive sentences or words begin with the same sound, it interferes with the understanding of them. Both the reader and hearer are more likely to confound them. For this reason alliteration must give way, except for purposes of emphasis, when the thought becomes of the first importance. Simple intensities are not sufficient as unifying factors; they cannot be properly subordinated to give unity to the line. It is interesting to note how the change from alliteration to rhythm has come about. In the early poetry, the alliterated syllables came at the beginning of the verse, but in modern poetry the rhymed syllables, which are their successors, come at the end. We shall see later how the beginning and end of rhythmical groups run into one another and become indistinguishable. The same is to be observed with reference to the feet. The accents in the feet become transposed. Although it seems probable that the foot in early poetry and the measure in all music began with the accented sound, the accented syllable in English poetry is more generally the last, and in Latin and Greek poetry it was quite as frequently the last as the first. The series of accented syllables in the verse and of articulate sounds in the foot seem to appear as a series of stimuli which are to be summated.

The two sections of the verse in old English were made to rhyme with their last syllables, and were then written as two verses. Two such couplets together form the most common stanza in English poetry. Instead of writing the members of each couplet next to each other, they are made more frequently and quite generally to alternate.

Æsthetic Forms:—That which binds the four verses into a stanza is not wholly the interrelation and balance of the two rhyming couplets. The members of the two couplets are frequently made to begin, the one with an accented syllable and the other with an unaccented syllable. Sometimes this, and sometimes a less number of accented syllables, make the lengths of the alternate lines less—a fact that gives artistic form to the verse when it is properly printed. It becomes then an appeal to the eye as an æsthetically beautiful form. This principle was seized upon by our poets during the sixteenth century, and carried to an extreme as regards form alone, which could not be sustained by the thought. The

poem had nothing but form. The principle of form becoming a unifying factor for a poem is perfectly true, and effective use is made of it in modern poetry. Among the older poets, George Herbert introduced many novelties into the forms of stanza. He relied upon both rhymes and artistic forms. Some of his stanzas take the form of a vase, an hour-glass, a pyramid and an inverted cone. Although they read smoothly, one cannot help but feel that his attempt at æsthetic forms has destroyed the beauty of the poems.

The sonnet¹ is probably the most organic of all poems. While the theme is very essential in binding the whole together, the lines are coördinated in the most intricate way by rhymes. A rhyme-scheme runs through the whole, which, when represented by letters, or dots of different sizes, or lines of different lengths, forms an artistic group, obeying the laws of principality, subordination, etc. The number of accents to the line is varied in some cases in such a way that it lends a kind of subordination of some lines to others, or of all to one or two.

Theme:—Little or no regard is paid to the thought in a poetical recitation by children or by primitive peoples. They delight in the emotional effect of sounds properly measured and balanced. With the growth of literature the thought has gradually become more important until it is about to usurp everything. The unity of the stanza and of the verse very generally depends upon it. Higher unities of the stanza—poems—depend entirely upon the theme. The attempts to coördinate stanzas by rhyming their last lines have not proved a great success. The strength of the connection is often lost. If the thought in the verse or stanza is allowed to become the prevailing element, the poetry becomes measured prose. Poetry arose in a kind of trance or highly emotional state, and for centuries it was used to produce such states in others. The whole structure is calculated to produce emotion, and for that reason it cannot easily become the medium of expression for the intellect. There must be a mutual dependence between the thought and the form, or they result in mutual destruction.

Under the influence of the church and Latin scholarship, English poetry became, or at least the attempt was made to conform it to certain rules of Latin prosody. English critics, misunderstanding probably both English and Latin poetry, tried to make the former conform to the rules of the latter. And there are many persons now who cannot see

¹ The reader is referred to Prof. Corson's "Primer of English Verse" for a treatment of the stanza and sonnet.

why the rules of Latin prosody are not universal. A verse beginning with an accented syllable and consisting of an alternation of accented and unaccented syllables, was trochaic measure, and the accented syllable was double the length of the unaccented. If two unaccented syllables were used between two accents, it was the substitution of a dactyl for a trochee. In this case, if the syllables preserved their proper time values according to the Latin prosody, four time-units—the dactyl—would appear in the place of three—the trochee. This, however, did not strike the critics as forming a defect in the rhythm, and the error has gone on. It is the current view among respectable English authorities to-day. In order not to keep the reader in suspense about so disputed and important a point, let me say what seems to be the true view. As the simplest time-unit of Greek poetry was a short syllable, and whatever value in time was given to it in a verse, that value must be maintained throughout, so the simplest unit of English poetry is the time between two accents—the foot is the simplest unit in the verse—and this must be constant. The time is apportioned among the syllables that are present between the accents, whatever the number. From the very nature of the accent the syllable receiving it will be longer relatively, though it does not bear a constant and simple relation to the length of the unaccented syllables. "Besides the increase¹ of loudness and the sharper tone which distinguishes the accented syllable, there is also a tendency to dwell upon it, or, in other words, to lengthen the quantity. We cannot increase the loudness or the sharpness of the tone without a certain degree of muscular action; and to put muscular action into motion requires time."

Another fact which has been greatly overlooked in the study of English rhythms, and which has led to much confusion and erroneous speculation and criticism of some poets, is the sectional pause, which allows two accented syllables to stand together in the verse. It was very common in Anglo-Saxon poetry, and disappeared almost entirely under the influences spoken of above. Shakespeare made free use of it, and for a lack of this knowledge, critics assert that he made use of false accents. Cædmon placed it before words upon which he desired to have a strong emphasis. It occurs before names of the deity. Guest says it owes its existence to the "emphatic stop," and is really the greatest departure from the rules of accent, which were observed with much care by the Saxon poets. It has been revived by more

¹ Guest's "History of English Rhythm," p. 75.

recent poets, and effective use is made of it. The value of this pause is the great emphasis it lends to the word following, and my purpose in dwelling upon it now is that it will come up again prominently in the experimental study.

Another question which connects itself very closely with this point of accent and pauses, is the foot or measure division of the line. Guest does not recognize such a division as the foot. The line is an alternation of accented and unaccented syllables, and he does not mention the fact of these forming groups which in Greek prosody were called feet and in music are termed measures. There seems to be no question that readers do make such groups by placing a slight pause either after or before the accented syllables. The Greeks associated these groups with a complete step in the march, and since in matters of æsthetics it is a rash thing to dispute or deny the accuracy of their judgments, we must regard the foot as a real division of their verse and inquire whether the lack of quantity in English syllables has anything to do with the absence of the foot division. The English verse is made up of a series of syllables in which every other one is uttered with greater intensity than the rest. The accented syllable requires more time, and the unaccented syllable unites or fuses with it into an organic group. These groups are then apparently separated by pauses. In French poetry there are no accented syllables, and the foot division is not recognized at all. This gives English poetry a kind of intermediate position between Greek and French poetry. The question of a foot division cannot be finally answered from an examination of our poetry, except as has already been said, such divisions are invariably made. The question will find its final answer in the experimental investigation.

Another problem which follows closely upon this is, what is the inherent nature of a group in a rhythmical series, or, what is the relation of the different syllables to one another in the poetical foot, and what determines the length of it?

The length of sentence¹ in prose is found not to deviate long from an average. Long sentences may prevail in an author for a few pages, but they are sure to be followed by short ones in sufficient number to balance the long ones. There appears a kind of rhythm in which long and short sentences succeed one another. This rhythm is constant for the same author; his earlier and later writings show no difference in the length of sentences. The writers of the more ancient prose show a greater average length of sentence than our more recent writers.

¹L. A. Schurmann. University Studies, Nebraska University, Vol. I.

There have been several attempts in late years to construct philosophies of English verse. Several of these will be taken up and their more salient features presented. The purpose is not to give a complete review of the books, but to call attention to a few facts which will supplement the work that has gone before.

The Science of English Verse by Sidney Lanier:—A simple auditory impression recurring at regular intervals of time furnishes the essential conditions of a rhythm. Of the four properties of sounds—duration, intensity, pitch, and tone-color—the mind can and does form exact coördinations of duration, pitch, and tone-color; intensities cannot be compared with exactness. The regular recurrence of sounds and silences constitutes primary rhythm, and a grouping of these sounds by means of intensity, pitch, or tone-color, constitutes secondary rhythm—the bar in music and the foot in poetry. For purposes of verse, syllables correspond to sounds and bear relations to one another in point of time, which are expressed by the simple numbers 1, 2, 3, 4, etc. The regularly recurring syllables of a sentence, whether prose or poetry, constitute a primary rhythm, "which the rhythmic sense of man tends to mould into a more definite, more strongly marked and more complex form, that may be called secondary rhythm." "The tendency to arrange any primary units of rhythm into groups, or secondary units of rhythm, is so strong in ordinary persons that the imagination will even affect such a grouping when the sounds themselves do not present means for doing it." Accent simply arranges the "materials already rhythmical through some temporal recurrence." As the comprehension of a series of sounds is rendered more easy by grouping, so the comprehension of a series of these groups is rendered more easy by again grouping these groups into tertiary rhythms. Alliteration, the recurrence of emphatic words and punctuation marks signify the tertiary group. The fourth order of rhythmical grouping is the line which, except in the case of run-on lines, completes a logical division of the sentence. Lines are again grouped into couplets by tone-color coördinations. The fifth order of rhythmical grouping is the stanza, and a complete poem is spoken of as the sixth order.

The effort of the author, in his treatment of the foot, is to make the rhythmical accent and grouping correspond to the logical accent and meaning. For this purpose he treats at length the iambic foot, it being the most common in English poetry. Making use of musical terms, this foot is equivalent to three eighth notes, and its typic form is one eighth note

followed by a quarter note. Instead of the eighth note, the foot may contain two sixteenth notes, and instead of a quarter note, there may be two eighth notes, or a dotted eighth and a sixteenth. The foot may also contain three syllables, each being equivalent to one eighth note, or four syllables, but the four must be read in the time of three eighth notes. In the place of any note, may be substituted a rest of equal length. An anapæst or dactyl cannot take the place of an iambic or trochaic foot, since the former are equivalent to four time-units and the latter to three. He says there are two kinds of rhythm only—3-rhythm and 4-rhythm. All other kinds resolve themselves into these two; 2-rhythm is really 4-rhythm, and 5-rhythm is equivalent to a 3-rhythm and a 2-rhythm combined.

A Primer of English Verse by Hiram Corson:—The object of verse to him is "the expression of impassioned and spiritualized thought." It originates in "the unifying activity of feeling and emotion." Upon whatever objects "feeling¹ or emotion is projected, or with what it is incorporated—it is unifying." "The insulated intellect, in its action, tends in an opposite direction—that is, in an analytic direction. When feeling is embodied in speech, that speech is worked up . . . into unities of various kinds." The primal unity is the foot, which is combined "in a still higher unity which is called the verse, and this in turn is combined into a still higher unity, which is called the stanza." "Rhythm is a succession and involution of unities, that is, unities within unities." It applies to a succession of either feet, verses or stanzas. Each class of unities has its combining principles; that of the foot is accent. Melody is the combining principle for the syllables. Alliteration is a common and effective form of consonantal melody. The combining agencies of the stanza are harmony and rhyme. Individual verses may be melodious, but when several are taken together they lack harmony. Rhyme is also an enforcing agency of the individual verse, and the emphasis resulting is neutralized in proportion as the verses are separated. Blank verse depends "upon the melodious movement of the individual verses, pause melody, and the general harmony or toning." Variations of the theme-meter produce important effects. "The feelings of the reader of English poetry get to be set, so to speak, to the pentameter measure, as in that measure the

¹ This is the author's great mistake. No such distinction can be drawn between feeling as unifying and intellect as analytic. Both analysis and synthesis are equally properties of the intellect, and it is difficult to conceive how the feelings can accomplish a synthesis or unify anything.

largest portion of English poetry is written." The introduction of any other than the theme-meter gives an emphasis to the thought. The substitution of a different foot gives a variety "which is essential to harmony." The shifting of the regular accent gives a special enforcement, either logical or æsthetic. "There should never be a non-significant departure from a pure monotony."

Rationale of English Verse by E. A. Poe:—Verse originates with the human enjoyment of equality. Unpracticed ears appreciate simple equalities. Practiced ears appreciate equalities among equalities; they are able to compare two sets of equals. The rudiments of all verse may possibly be found in the spondee. In this, the mind finds its first pleasure in the equality of two accented syllables. A collection of two spondees—two words of two equal syllables—forms the second step in the development of the verse. A third step would be found in the juxtaposition of three words. This, however, gives the idea of a monotone, a relief from which is found in words of different accents—iambics. A dactyl might be employed as a further relief from the monotone. A sequence of words of any sort would form a monotone, if they were not curtailed or defined within certain limits. This gave rise to the lines, the terminations of which are again determined by equalities in length, and marked by equalities—likenesses in sound. Every foot in the same verse requires equal time. A three-syllable word may appear as iambic or trochee, providing that two syllables can be read in the time of one. Blending is an unwarranted liberty. He states this general principle: The substitution of a foot, the sum of whose syllabic times is equal to the sum of the syllabic times of the foot substituted, is allowed with this restriction only, that the regular foot shall continue long enough or be sufficiently prominent to leave no doubt of the kind of verse. He says "that rhythm is erroneous, which any ordinary reader can, without design, read improperly." The real test of the perfection of a verse is the pleasurable feeling it yields.

Classical Poetry:—Classical Greek poetry was either chanted or sung, and for that reason was exactly timed. There was really no difference between a poetical recitation and a song. The simplest elements in the measure, according to which poetry was sung, was a time-unit equivalent to one eighth note. By combining these time-units into groups, they formed the measure or foot. A group of several feet constituted the section, and two sections entered into the line, a certain number of which were united into strophes or stanzas. A time value was given to all syllables and words

in the language; they were either long or short. A short syllable was equivalent to one time-unit, and a long to two. Various measures were employed. They might be equal to two, three, four, five or six time-units. The most common measures contain three or four time-units. The three time-unit foot most generally contains two syllables, one long and one short, or one short and one long. The four time-unit foot contains two or three syllables, generally two long, or one long and two short, or two short and one long. When only one long syllable occurred in the foot, it received an accent; when there were two in the foot, the first received the accent. The accented portion stood as the thesis, and the unaccented as the arsis. In the same way the two sections of the verse stood as thesis and arsis. The thesis came first. The middle pause did not usually divide the verse into two equal divisions. The first was the shorter, the pause coming within the third foot. Except as showing a perfect subordination to a chief accent, and a slight anacrusis at the close, the verses had no distinguishing marks; they were not rhymed, and very rarely alliterated.

The number of feet in a verse varied with different kinds of poetry, two being the smallest and six the greatest. The kind of foot with which the measure set out was not always maintained. Any other foot agreeing with the theme-foot in position of accent, and in the number of time-units, might be substituted. As such agreements in the kind of feet were few, there could be very little variety in the verse.

Greek poetry was not allowed to develop long untrammelled by rules. A rigid philosophical system was imposed upon it, and all future poetry was made to conform to this system. But it would be difficult to say that Greek poetry suffered from the restriction. It prevented novelty for novelty's sake, but allowed great freedom where freedom was most needed.

There are several facts in the history of rhythm that are interesting, both for the subject in hand and for psychology in general. Soon after the idea of varying the number of syllables in a foot had become known, and its effects appreciated, there arose a kind of mania for verses which contained a variety of feet. They were characterized as "tumbling verses" from the peculiar effect they gave rise to. This was a discordant and unpleasurable feeling. There was really no rhythm to them, and they never became popular. The same took place in regard to the length of line. Various novelties were introduced, when a longer line than that of the earliest poetry was found more pleasing and less abrupt. Verses of

six and seven accents were tried, and verses containing two sections, each of which was an alliterated couplet, having four or six accents, appear in some authors. No new combining agency was employed, and probably for that reason the verses exceeded the mental span. Had the older poets grasped the principles of unifying their lines by rhyme, or by proper subordination of the sections, they might have made such long verses a success. In the same line were the attempts at æsthetic forms, which have already been spoken of.

EXPERIMENTAL INVESTIGATION.

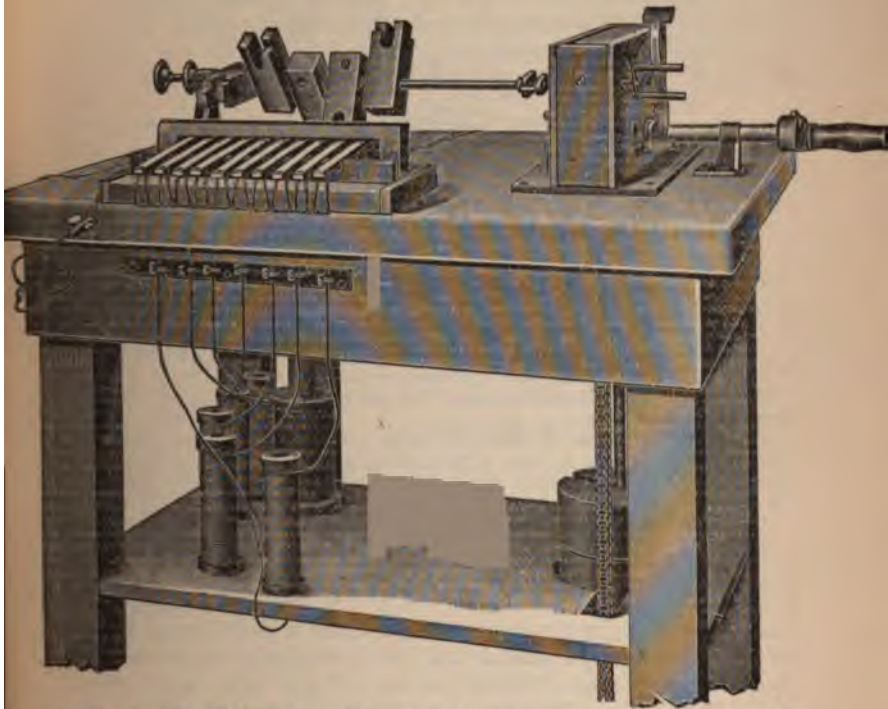
¶ This work was undertaken with several objects in view. The first and most important object was to determine what the mind did with a series of simple auditory impressions in which there was absolutely no change of intensity, pitch, quality or time-interval. Each separate impression was to be indistinguishable from any or all the others. Regular variations with respect to the intensity or time-interval of the sounds in this series, which will be called a rhythmic series, were then to be tried separately and together, with the purpose of determining what values these properties of sound have in forming a rhythmical series—that is, a series of groups of impressions—out of a rhythmic series. It was seen at the outset that it would be practically impossible with the apparatus at our disposal to employ pitch variations, and for that reason no attempts were made with variations in pitch. Variations in quality or tone-color were contemplated, but the experiment was not carried out, first on account of a lack of time, and secondly of proper apparatus. The results of the first experiment anticipated much that was to be tried in the later experiments. As the work progressed, new problems were suggested for investigation until the narrow limits within which the work was begun were greatly overstepped. These problems will be taken up in what seems to be their proper order, and the results presented.

Apparatus:—The click of an electric telephone when connected in an induction circuit is constant in intensity, pitch and quality, when breaks occur in the primary circuit, providing the primary circuit is constant. The click is not the same in intensity when the primary circuit is made as it is when the primary circuit is broken. For this reason, the sound at the break only could be utilized. It is perfectly constant and stronger in intensity than the click at the make. It varies directly in intensity with variations in the strength

of the current and changes slightly in pitch and quality with variations of intensity, but the pitch and quality are always the same with the same intensity of current. A break at regular intervals in the primary circuit, when the secondary circuit is closed, the secondary circuit being open when the primary was closed, was all that was necessary to furnish the required series of auditory impressions with which the investigation might begin.

A chronograph after the pattern devised by Wundt¹ and

FIGURE I.



built by C. Krille, furnished a constant power. Figure I. gives a general view of the whole apparatus as it was used in this experimental investigation.

The drum-shaft was slipped off the drum and five arms, two and one-half inches long were put upon it by passing the shaft through a hole near one end. Each arm was provided with a set screw, that the arm might be held in position and

¹This apparatus will be found fully described in the second volume of Wundt's *Physiologische Psychologie*, p. 279. 3d ed.

its position changed at will. They were set at equal distances apart along the shaft, and their points separated by 72 degrees, so that the space about the shaft was divided equally into five divisions. (See Figure II.) Corresponding to

FIGURE II.

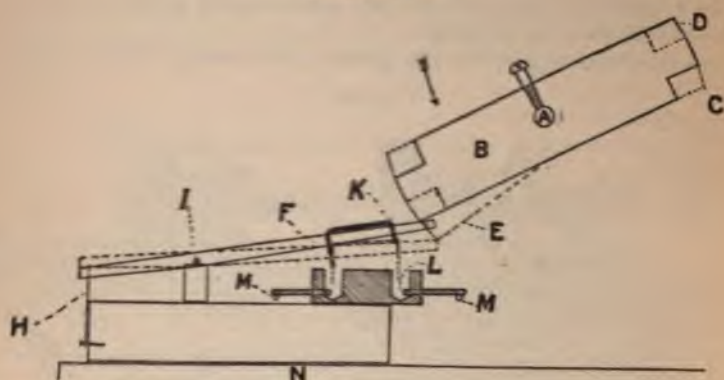


Figure II. shows the operations of the keys.

- A. Drum-shaft.
- B. The wooden arm on the drum-shaft.
- C. The dotted line represents the following point of the arm.
- D. The continuous line represents the leading point.
- E. The dotted line indicates the position of the arm when the key is pressed down.
- F. The key bearing the platinum points, which project below and are connected by the wire indicated by K. The dotted line below shows the position of the key when the platinum points dip in the mercury.
- H. The rubber elastic which caused the key to react.
- I. The rod upon which the key turned.
- L. The mercury cup.
- M. The wire connections.

each arm were two keys placed in such a position on the top of the chronograph that as the shaft revolved the ends of the arms came in contact with the ends of the keys and pressed them downward about half an inch to allow the arm to pass by in its revolution. The keys, which were ten in number, two to each arm, were made of strips of wood, six inches long and a half inch wide, and hinged horizontally upon a steel rod two inches from one end in such a way that the ends might move up and down. To the short ends were attached elastics, which caused the long ends with which the arms came in contact to rise up after they had been released by the arms on the drum-shaft. They were prevented from rising up too far by a piece of wood placed above them. Each arm

bore two points, the one about an inch to one side and ten degrees in advance of the other. The leading point came in contact with one key and pressed it down in advance of the other. As each point was broad, covering about twenty degrees of the circle described by the end of the arm, the first key would remain down until after the other had been pressed down. As both points upon each arm were of the same width, the key first pressed down would be released before the other. Near the long end, each key carried two platinum points which projected downward below the key, and which were connected at the upper ends by a wire. When the keys came down, the platinum points dipped into cups of mercury, which rested upon the top of the chronograph. (See Figure II.) These mercury cups were made by boring holes into the side

FIGURE III.

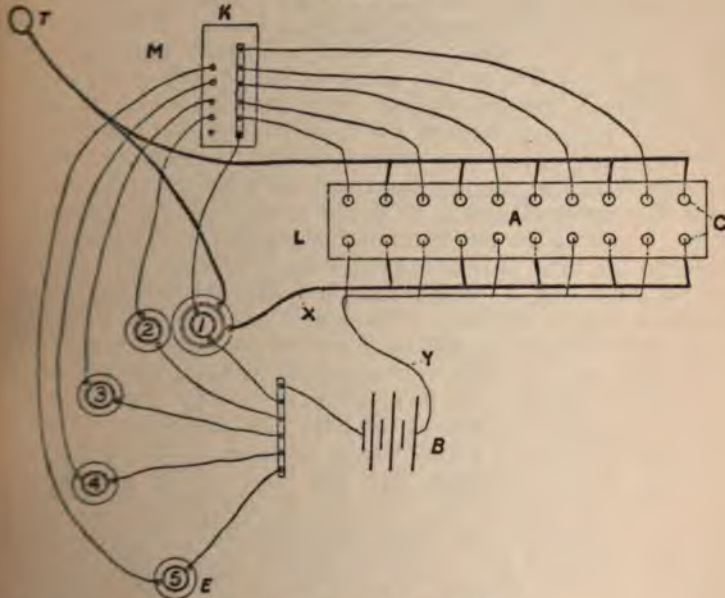


Figure III. shows the electrical connections.

A. The strip of hard rubber.

B. The battery.

C. The mercury cups.

E. 1, 2, 3, 4, 5. The primary coils. The double rings about 1 represent the induction coil.

K. The key-board.

The primary circuit is represented by light lines indicated by Y, the secondary circuit by a heavy line, X.

T. The telephone.

of a strip of hard rubber, five-eighths of an inch thick. Holes were drilled into the edge of the rubber opposite the mercury cups and copper wires inserted, which were connected with the battery and induction coils in the manner which is schematized in Figure III. Beginning at the left hand end (marked "L") of the hard rubber strip, the first pair of opposite cups and each alternate pair along the strip were connected with a coil of wire on one side, and with the battery on the other. For purposes to be described later, were five coils of wire which might be connected with these mercury cups. The coil and the battery were connected, thus completing the primary circuit. The other pairs of opposite cups which alternated with these were all connected together on the one side with an induction coil, and on the other with the telephone. The induction coil and the telephone were joined, thus completing the secondary circuit. The ten keys corresponded to the ten pairs of mercury cups. When the first key at the left hand, and each alternate key thereafter, was pressed down by the arms on the drum-shaft so that the platinum points dipped into the mercury, it would close the primary circuit, for these keys joined the opposite mercury cups which were connected with the battery. When the second key at the left hand, and each alternate key thereafter was pressed down, it would close the induction circuit. Key 1 at the left hand end of the strip of hard rubber matched the first pair of opposite cups of mercury and was paired with key 2, which matched the second pair of the opposite cups of mercury. These first two keys were operated by the first arm at the left hand end of the drum-shaft. The other four pairs of keys were operated by the other four arms on the drum-shaft. Let us consider now only the first pair of keys and the first arm at the left. As the shaft revolves, the point of the arm which was in advance of the other was made to come in contact with the long end of key 1, and pressed it down. After coming in contact with key 1, the point of the arm could move through an arc of ten degrees, keeping the platinum points in the mercury, and thus closing the primary circuit, before the second point of the same arm would come in contact with key 2. When the keys were pressed down sufficiently to make the circuit, the points of the arm were made to slide by the ends of the keys in such a way that the key was not released until the arm had moved through an arc of twenty degrees. A further revolution of ten degrees by the arm would press key 2 down sufficiently to close the secondary circuit. If, now, the arm continues to revolve, key 1 would be released and rise up, breaking the primary circuit, but key 2 would

remain down while the arm moved through an arc of ten degrees, keeping the secondary circuit closed for a time after the primary circuit was broken. This would give a sound in the telephone. The same process would be repeated with each of the five pairs of keys and their corresponding arms. If, now, the arms were set at an equal number of degrees apart and the drum-shaft were made to revolve at a uniform rate, the clicks in the telephone would be separated by equal intervals of time, and not varying in intensity, pitch or quality, these clicks would form the required series of auditory impressions. If a change in intensity is desired, as it was, the five wires connecting the different pairs of mercury cups might each be connected with the five different coils which were referred to above. These were set at different distances from the induction coil (see Figure III.). As the different primary coils were of the same size, the strength of the induced current, and therefore the intensity of the sound, would depend upon the distance at which the primary coils were placed from the induction coil. They were placed at just sufficient distance apart to make the sounds easily distinguishable from one another in a graded series of intensities. By means of the key-board (marked "K") it was possible to connect all the five wires in any way that was desired with the five primary coils. The clicks might all be of the same intensity, all different, or of two, three or four different intensities. Whatever the variation, according to this arrangement it would recur every fifth click. When variations every fourth or third were desired, three or four arms were set upon the drum-shaft and only three or four pairs of keys operated. If the arms were separated by an equal number of degrees, the series of clicks would still be regular. Two kinds of arms were employed, those with a single end and those with a double end. (Figure II. represents the double ended arm.) By using both single and double ended arms on the shaft, and operating the five pairs of keys, it was possible to get an arrangement by which variations in intensity might occur every sixth or eighth click. Taking all the possible arrangements together, the operator might introduce a more intense click every two, three, four, five, six or eight clicks. Again, he might make a series of clicks which were composed of two, three, four or five different intensities of sound.

By making the number of degrees between the arms on the drum-shaft different, a difference in time-interval between the clicks was produced. In the same way as with the different intensities, a longer interval of time might be made to recur every two, three, four, five, six or eight clicks.

The rate at which the drum-shaft revolved determined the rate of the clicks in the telephone. This was controlled by the fan regulator upon the chronograph. Faster or slower rates were obtained by using smaller or larger fans. The rate was determined by counting the clicks in the telephone by a stop-watch. Rates between one click in two seconds and ten in one second were possible. As the rate was a very important factor, it will be given in all cases in the presentation of results. The "time" will indicate the interval between two clicks. The battery used consisted of 36 cells of the Watson's patent.

A further method of testing the accuracy of the setting of the arms upon the drum-shaft, which was done with a protractor, was to connect a time-marker in the primary circuit and take the record upon a drum along with a tuning-fork. It was found that setting might be accurate, but the drum-shaft might vary between one and two hundredths seconds in six seconds.

There is one particular in which an improvement might have been made in the apparatus. It was this: When the primary circuit was made, though the secondary circuit was open, a faint sound was heard in the telephone with close attention. The induction coil acts as an electric condenser, and the telephone being extremely sensitive, betrayed the presence of a weak current. This might have been avoided by making a break in both wires leading to the telephone, in such a way that the telephone would be wholly disconnected from the induction coil, when the primary circuit was made. During the entire experiment, only a single subject detected the presence of this sound, and for that reason it may be disregarded. The telephone was placed in a different room from the chronograph, where there was as little disturbance from other noises as possible, especially from any noises that were in the least suggestive of a rhythm.

When the experiment first began, the apparatus was set so that about three or four clicks to the second were heard in the telephone. The subjects were not informed in any particular in regard to the experiment. They were invited to be seated and listen to the telephone. This they did, taking very generally a rather critical attitude. They were then invited to say anything that suggested itself to them, whatever the character. These statements were all carefully recorded, and will be given in substance. The sounds suggested most generally and immediately the clock. Other suggestions were: slowly dripping water, galloping horse, pile-driver, etc. After the subjects had been seated for a time, during which it was apparent they were making a critical

study of the nature of the sounds, the statement most generally given, and voluntarily, was that the sounds were all alike, and seemed to be separated by the same interval of time. After this statement the subject paused, as if most that could be said had been said. In some cases they asked for particulars in regard to what they should look for. Sometimes, however, they went on to say that there was an apparent change of intensity in the sounds; the clicks seem to group themselves by twos or fours, as the case might be; generally, however, it required some kind of a suggestion to direct the attention of the subject to the grouping of the sounds. An indirect method was preferred to a direct one. In cases where the subject had spoken of the clicks seeming like the clock ticks, they were asked if there was the same difference of intensity or quality in the sounds as was apparent in the clock ticks. This suggestion was sufficient in many cases. The subject directed his attention then to the matter, and if there was any tendency to make groups of the clicks, it was apparent in a few moments. Sometimes it was remarked that they had noticed such a grouping, but had regarded it as a freak of their imagination, and did not think it worth mentioning. Another method of directing the attention of the subject to the grouping was to make a reference to the fact that they had said the sounds were all alike, and then to ask why they had said sounds and not sound; did they suppose there was more than one sound? In this case also, they replied frequently that they imagined that there was more than one sound, but did not think it worth while to mention the fact. In some cases it was sufficient to ask the subjects to count the clicks as they heard them, and then to ask how they counted. The reply was that they counted four or two, as the case might be, and then began again. Again it was noticed that the subject was unconsciously keeping time, with the foot tapping to every fourth or every second click. Such a subject was asked why he tapped every fourth or second click, and so his attention was directed to a grouping that was going on unconsciously. Such indirect methods were usually successful, but there were several cases in which indirect suggestions of this sort failed of their purpose. Direct methods of tapping a rhythm with the fingers or counting did not suggest anything beyond the clock tick to two subjects. These persons possessed no appreciation of music at all; they could not "carry a tune," and yet were able to recognize some of the common airs when they were sung or whistled. The general statement of the remarks and answers of each subject will be given as fully as it seems necessary. They will be abridged as far as possible,

but the special features in the answers of each subject will be mentioned. The treatment of special phases of these results will follow, and then will be taken up the result of special investigations that were suggested during the first part of the experiment.

Subject 1. Some musical talent and training.

Time, .23 sec. The first suggestion was a 4-group. Subject could suggest groups of two, three, five and six, but when he made no suggestion either by tapping or counting, he returned to a 4-group. The third in each 4-group was accented,¹ but it was possible in the later experiments to accent any member of the group. In general the first in all forms of grouping was accented. The 3-group was unpleasant and the 5-group was very difficult to maintain. Time, 1.14 sec. The most natural form of grouping was by two. It was possible to get a 4-group, but when the subject made no suggestion of any other group, he returned to the 2-group. Time, .167 sec. The 6-group was most easily suggested. It had the appearance of being composed of two 3-groups. The subject showed a tendency with this rate to group the 3 and 4-groups into higher groups. Eight-groups of threes and 4-groups of fours succeeded very well. It was not so easy or natural to make higher groups of fours. Time, 1. sec. This rate produced a drowsy feeling. The subject was inclined to make each click stand as the accented click in a 3-group, supplying the unaccented sounds between the accented in imagination. When the subject was tired he noticed a tendency to change the grouping frequently from two to three, and vice versa. The subject showed a strong tendency towards 4-grouping in preference to all other forms of grouping, and yet during one experiment, when the time was .208 sec., he found a 3-group more pleasant than either a 2-group or a 4-group. The rate was too fast for easy grouping by two. When he counted objects he counted them by fours. Time, .323 sec. The subject was disposed to make a 4-group, and, even when every third sound was made more intense than the others, he persisted in saying that he grouped them by fours, but that there was probably a longer interval in the series which disturbed the smoothness of his 4-groups. When his attention was called later to the accented clicks, he made no further mistakes of longer intervals for accented sounds.

Subject 2. Some musical talent and training. Accustomed to introspective work.

Time, .323 sec. The subject grouped by twos, visualizing the pendulum.² He could suggest groups of three and four easily, the four being more difficult than the three. Time, .263 sec. He grouped the clicks by four, but the 4-groups were divided into two 2-groups. Time, .208 sec. This rate yielded easily and naturally to a double 3-grouping. When he first listened to the telephone after

¹This accent consisted of an apparent increase in intensity with a change in pitch and quality.

²Almost every subject either visualized the pendulum or spoke of the pendulum-swing movement sometime during the experiments. In either case it was a form of grouping. When the rates were slow, the subject visualized the clock pendulum and made one click come near the completion of each half swing. The clicks were then grouped by two and were called the clock tick. In my own case and in some others there was a strong tendency to sway the body with the pendulum. This was called the pendulum-swing movement by the different subjects. It was quite visible at times. By this pendulum-swing movement groups of two, three, four, six or eight were frequently grouped into 2-groups. The first group, then, in the 2-group was accented or more emphatic than the other, and a distinct pause seemed to follow the second group.

either a change of rate or at the beginning of a new experiment, the clicks did not group themselves, but in a short time the tendency to group increased until it required the greatest efforts to hear the clicks as a uniform series. The subject was able to hear the clicks as a uniform series, only by imagining some one pounding in the distance. It required a mental picture of some objective thing that was perfectly uniform. When he gave himself up and listened to the series as a whole, he fell into some kind of grouping, which might or might not continue for any length of time. He had a strong tendency to shift from one grouping to another. He compared it to the optical illusion of the "stairs." The double 2-group is confounded sometimes with the double 3-group. Time, .187 sec. The subject said he got a compound 2 and 3-group, which by actual count of the accents to which he tapped with the fingers, showed he was making a double 4-group.¹ This subject was strongly disposed to double groups of all sorts. Time, .323 sec. At this rate the 2-group was most naturally accompanied by the mental image of the clock. Time, .263 sec. This yielded most easily to a 4-group, which took the form of two groups of twos. Time, .167 sec. This rate yielded at first to a 6-grouping, which was divided into two groups of threes, but it did not persist there; he returned to a double 2-group.

The pulse seemed at times to impose a grouping in which the clicks coming nearest in time to the heart-beat was accented. When the subject gave his attention to breathing, it more generally conformed itself to some grouping that was already going on. Inhalation lasted during a 4-group and exhalation during a 3-group.

Subject 3. Considerable musical talent and training.

Time, .5 sec. The subject's first suggestion was of a 2-group, but he immediately decided that a 4-group was more natural. He was able to count almost any rhythm at this rate as far as twelve, and the clicks seem to group themselves with the count. At first the groups were apparently separated by a longer interval, which the subject believed in the first place to be real. He was disposed to regard the 4-group as the most satisfactory. Any grouping was plainer when he counted. Diaphragmatic movements also accompany the grouping. With indifferent attention there was no grouping. The 4-group usually contained two accented clicks, either the first and the third or the second and the fourth. The former were preferred. This rate was found to be most pleasing. It was animating. The 5-group was difficult to get. A slight pause occurred between the groups in every form of grouping. In the presence of the chronograph, which gave a 6-rhythm which was composed of two 3-groups, the subject still grouped by four for a time, but this tendency was finally overcome and the series yielded to the suggestion of the chronograph.

When every fourth was accented, the subject being unaware of this accent, said that the 4-group only was possible, for there appeared to be a longer interval between every four clicks which made any other grouping impossible. When the accent was strengthened, he said the interval had been lengthened. This long interval might come anywhere within the group of four, but it more generally came between the groups. When two stronger clicks followed by two weaker ones formed the series, the subject said the

¹It is not unfrequent for a subject to mistake the actual grouping which he is making. Sometimes a subject is so disposed to a particular number that he persists in saying that he gets groups of that number, when it is perfectly evident a greater or less number of clicks according to the circumstances is grouped with the accented clicks to which he taps.

rate was slower. He grouped the series by fours, but it appeared as though two long sounds followed by two short ones formed the group. When three strong sounds and one weak one formed the series, he still grouped by four. The first two in each group seemed to be of the same length, the third was longer and the fourth very short. During all the experiments the subject confounded stronger clicks with long intervals, and was never able to tell the difference between a strong sound and a long interval. He was surprised when told afterwards that the longer interval had been caused by accenting one sound. Time, 2.304 sec. The subject visualized the pendulum, but said the pendulum seemed to reach its full swing before the click corresponding to the swing was heard. The clicks seemed to delay too long. Time, .323 sec. Every third sound was accented. The subject had a strong mental habit for grouping by fours and was greatly puzzled by this accent on every third, which he said was a longer interval and broke up his tendency to form groups of fours. Time, .208 sec. Every third was accented. The subject forms 6-groups, which were accented upon the first and fourth and a long interval appeared between the groups of six. Time, .137 sec. When the series was composed of clicks of three different intensities repeating themselves in the same order, the 3-groups were again grouped by four generally, though the subject could suggest groups of three 3-groups.

Subject 4. Some musical talent.

Time, .288 sec. The clicks suggested the clock-tick. The subject could group them by twos, but he found it more natural to group by fours. It has long been a mental habit with him to make groups of four of any objects or impressions that would admit of any kind of grouping. He counts by four and groups the puffs of a locomotive by four. Four objects or impressions of any sort standing together have always arrested his attention. He found it possible to group these clicks by two, three or five when he made a suggestion either by counting or tapping with the fingers, but when the suggestion was stopped he returned to a 4-group. In every kind of grouping the first sound was always accented. Time, .115 sec. The subject said the grouping was by four and was requested to tap the accented click in every group. In six trials for five seconds each, he tapped just five times during each trial, showing he made a group of four in one second. The actual number of clicks to the second being 8.6, it was apparent that he was making a much larger group than four, probably an 8-group. When asked to make a 3-group and tap the accented click in each group, the results were nineteen taps in fifteen seconds, showing that his groups were not far from six instead of three clicks to the group.¹

The 3-group was really a 4-group in many cases. Between each group of three occurred one click, of which no account was taken. It seemed to him something like this when he counted : 1, 2, 3, 1—1, 2, 3, 1—1, 2, 3, 1.

When the subject gave attention to the pulse, the number of clicks coming between the beats of the heart formed a group. The click which came nearest in time to the heart-beat seemed always to correspond to it. The breathing adjusts itself to the 4-rhythm. Inhalation lasts during one group of four, and exhalation during another. In this way the 4-groups were grouped by two. Time, .156 sec. By forming a mental image of the pendulum, or of some object moving up and down, he was able to make double 4-groups,

¹It is probable that the primary grouping was two, and these groups of two were then united into larger groups of three and four.

which corresponded to the full swing of the pendulum. These three rates were given in rapid succession. Time, .536 sec. The 4-group was very clear. Time, .268 sec. The 4-group was unpleasant at first, but he gradually became accustomed to it. At first the rates seemed too fast. Time, .536 sec. With this latter rate the 4-group seemed to divide into two groups of two during the second trial. Time 1.072 sec. The 2-group seemed most natural and the subject felt a strong tendency to form higher groups of twos. After the subject became accustomed to this rate, he was more inclined to form 4-groups than 2-groups, but still the third and fourth clicks seemed at times more like a 2-group than a part of a 4-group. The general effect of this last rate was soothing. Time, 1.66 sec. This rate was soporific; it was possible to form a 2-group, but he did not do so spontaneously. Time, .323 sec. The subject grouped the clicks by four and found difficulty in suggesting a 3-group. When every third was accented, he persisted in grouping by four. Again he was asked to suggest a 3-grouping, and he succeeded in doing so. The subject was unaware of the accent, and expressed surprise that he could group by three, and found it easier than grouping by four. The following rates were given in rapid succession. Time, .268 sec. The 4-group was very clear and pleasant. Time, .17 sec. The subject grouped by fours, but felt a confused irritating feeling. There was something added onto each group of four. Time, .134 sec. This rate recalled the sound of a locomotive. He visualized a revolving wheel, during each revolution of which he counted four. Time, .116 sec. He still groups by four, but the 4-groups are grouped by two, a strong and weak group together. When every eighth was accented, he grouped by eight. There was a distinct pause along with the accented sound. Time, .134 sec. Every eighth was accented. The 8-group divided into two 4-groups, with a pause after the second group. The 4-groups were grouped by two with the pendulum swing. Time, .268 sec. Every eighth was accented. The subject grouped by four and associated the pendulum swing with the groups of four.

Subject 5. Some musical talent.

Time, .288 sec. The subject was most naturally disposed to a 4-group, and found it difficult to get any other. Time, .78 sec. The 2-group was most natural with this rate. The 3-group was pleasant and easy when it was suggested. Time, 1.44 sec. It was easy to form 2-groups, but other groups were impossible. These three rates were given in rapid succession. Time, .353 sec. The 4-group was most natural. Time, .183 sec. The 8-group was most agreeable with this rate. Time, .156 sec. The subject found the 8-group most natural.

Time, .3 sec. When every fifth was accented, the subject made groups of four, accenting the four, and said that there was a rest between each group. He found it quite as easy also to accent the first. When his attention was called to this pause between the groups, he decided that the groups contained five clicks, in which the fourth was accented. When the first and third clicks were made more intense, the subject was greatly puzzled for a time, but decided that the series was compounded of a 2-group and a 3-group.

Time, .969 sec. He was able to form 3 and 4-groups. The series was associated strongly with the clock, and for that reason the 4-group tended strongly to divide into two 2-groups. Time, .323 sec. He found the 4-group most natural and pleasant, and when

he attempted to form groups of three, they would immediately run into fours. Time, .208 sec. The subject formed a long group of the clicks which he thought was an 8-group. The rate was too fast for easy grouping by four, and his attention seemed to waver between a 4-group and a longer one which he thought was an eight.

Subject 6. Some musical talent and training.

Time, .3 sec. When the subject first listened to the telephone he found no tendency to form groups of these clicks. Even after repeated suggestions the subject did not comprehend what was desired or to be looked for. He was asked to tap an accompaniment to the sounds with four fingers. After a time he found himself accenting the third, and grouping the sounds by four. When he tried the suggestion with three fingers it did not succeed very well. When two were tried, the subject decided that the 4-group was a combination of two 2-groups. In the same way he was able to group the sounds by six, but the groups divided easily into two 3-groups. On the whole the 2-group was the most natural with this rate. Time, .156 sec. The 4-group prevailed and easily combined into 8-groups. Time, .78 sec. The 2-group was most natural, but the subject was able to suggest the 3-group easily. Time, 1.44 sec. Even a 2-group was difficult to get. The time seemed to be too long. Time, .353 sec. Groups of two, three and four were all possible. The first click in all groups was accented, and the third also in the 4-groups. When he suggested a 6-group, it divided easily into two 3-groups or three 2-groups. The 8-group divided readily into two 4-groups. It was difficult to get a 5-group. The 5-group tended to run into a 6-group, which then divided easily into two 3-groups. Time, .288 sec. The 4-group was most natural, and readily combined into double 4-groups. Time, .156 sec. The 6-group was most natural. When the telephone was disconnected and the chronograph continued to run, the grouping always began with the first click in a new group. Time, .78 sec. The 2-group was the most natural. Time, 1.66 sec. There was no spontaneous grouping with this rate. It was too slow. The subject found it convenient to regard the click as an accented click in a 4-group, supplying the three intermediate sounds in imagination. Time, .323 sec. At this rate, the subject showed strong tendency to muscular movements, either to tap with the finger or toe upon the accented click, sometimes to nod the head or sway the body. He found this rate very favorable for voluntary changes of the grouping, which he did either by counting or tapping with the fingers. The general emotional effect was depressing.

The pendulum-swing movement or 2-rhythm was an important factor in all his groups. The 6-group was usually composed of two 3-groups, and the 8-group of two 4-groups. Time, .167 sec. The 4-group was very clear and pleasant, and the subject tended to group them by the motions of the pendulum. When every third click was strengthened, the subject grouped by threes, and made the 3-groups follow the motions of the pendulum. In this way the series produced an exciting effect. Time, .134 sec. The 4-group was plain and distinctly grouped by twos by the pendulum-swing. This rate was also exciting and animating. When every sixth click was accented, the grouping lost its exciting effect. When the series was composed of clicks of three intensities, the strongest first, the clicks were grouped primarily by threes and these 3-groups were again grouped into 4-groups. When the time was changed to .167 sec., and three grades of intensity retained, the higher grouping of 3-groups by four ceased. Time, .137 sec. When

every sixth click was accented, the series was grouped by six, and the six groups were again grouped by the motions of the pendulum. Time, .167 sec. Every sixth was accented. The grouping was still by six, but the 6-groups did not group by two. With the slower rate the 6-groups did not seem so compact as with the faster rate. Time, .208 sec. Every sixth was accented. The 6-group was difficult to grasp. The time was too long and the group tended to divide into two 3-groups. Time, .263 sec. Every sixth was accented. There was greater difficulty still in grasping the 6-group, on account of the tendency to divide the group into two 3-groups. Time, .323 sec. Every sixth was accented. The subject now grouped the series by four in spite of the accent upon every sixth. When the subject heard the sound of the chronograph, which was rhythmical, he grouped the clicks according to this rhythm, which in this case was a double 3-rhythmical. Time, .3 sec. Every fifth sound was accented. The subject grouped by fours, but the accent came in a different place in each group. It shifted one place further to the right. When a 6-group was suggested to the subject, the accent changed its position in the opposite way. It shifted its position one place to the left in each group.

Subject 7. Some musical talent and training.

Time, .3 sec. Almost immediately the series divided into groups of four, and soon after the 4-groups were grouped by two. With the suggestion of tapping, the subject was able to group by two, three or five. Time, .78 sec. The 2-group could be suggested only with difficulty. The 3 and 4-groups were not at all pleasant. Time, .156 sec. There was no distinct grouping. The series seemed to rise and fall in intensity at regular intervals. At times he had a "dreadful" feeling that the chronograph was slowing up and about to stop. The subject had observed this grouping of sounds in the puffing of a locomotive. He had not noticed a definite number in the group. The sounds simply rise and fall in intensity. Speaking of the 8-group, the subject said he had a feeling of not being able to "round up" until he came to eight. It seemed natural to stop at eight, and start over again. This group was accompanied by a feeling of completeness. During inhalation, the clicks seemed to come faster, and slower during exhalation. In all forms of grouping the subject felt a muscular sensation in the stomach and intestines. He also felt a strong tendency to beat time with the thumb. He had been taught to do so when quite young.

Four-grouping is a kind of mental habit with him. When the series was accented upon every fifth, he still grouped by four, making a pause between each group in which he pronounced the word "and:" 1, 2, 3, 4, and 1, 2, 3, 4, and 1, 2, 3, 4. In the same way the 3-group appeared as 1, 2, 3, 1,—1, 2, 3, 1,—1, 2, 3, 1. In the 4-group the first and third were always accented. In other groups the first was accented. When the rate was .72 sec. or .156 sec. the grouping did not come without suggestion by muscular movement or counting; with intermediate rates the grouping by four was wholly involuntary.

The subject could group 4-groups by two very easily, but it required an effort to group them by four, or 8-groups by two. The 8-group was generally composed of a more and a less emphatic group of four.

Time, .268 sec. A double 4-group was the most natural and easy. One group was more emphatic than the other. Time, .263 sec. Though the time was only slightly changed, the subject thought the previous double 4-group changed into a more perfect 8-group.

There was not such a strong division into two 4-groups. Time, .208 sec. This rate gave a "better 8-group" than the previous one. Time, .167 sec. The groups did not separate distinctly. There was a kind of confused feeling about the clicks. Time, .137 sec. The confused feeling with the previous rate was more apparent still. When every sixth was accented, he grouped by six and the 6-groups were grouped by two with the pendulum-swing movement.

Subject 8.

Time, .3 sec. The subject took a critical attitude. He had no preference for any grouping. He could count any number as far as ten, and the series seemed to group itself according to the count. In the longer groups, groups of two were frequent. Time, .156 sec. The 8-group was the most suitable. It was composed of two 4-groups, and each 4-group of two 2-groups. Time, 1.44 sec. It was not possible to form any grouping. The rate was too slow. The subject has noticed rhythms in the sounds of a mill-wheel, locomotives and fans. He was not aware of any definite grouping of the sounds.

Subject 9. Considerable musical talent. Long and careful training in music. Accustomed to introspective study.

Time, .3 sec. The subject adopted a critical attitude and gave his attention to the nature of the sounds. At first he was inclined to believe that they were all alike in intensity, but then he thought every third was stronger than the rest. For a time the interval between the clicks seemed to be irregular, but he soon discovered that this irregular interval might occur anywhere he chose to put it. In a short time his tendency to find groupings of the clicks grew so strong that it required an effort to hear the series uniform. Such an effort was akin to the feeling of "looking long into the future." The grouping tendency had to be restrained. Time, .3 sec. The 4-group was so plain that he did not discover the fact that it was imaginary and was completely surprised that the illusion was so complete. It was then more than ever an effort to hear a uniform series of single impressions. He said, "I find no rhythm as long as I hold my breath and stick to it." "I get hold of one click to compare it with the succeeding clicks, but I can't hold onto more than eight or nine." The simple suggestion of any grouping was sufficient to produce that grouping. Groups of two, three, five, six and eight follow immediately the suggestion of any of them. A group of seven was more difficult. Groups of all numbers were generally accented upon the first, but the accent could be voluntarily changed. In the 8-group the subject had a tendency to accent every other one. The grouping was generally accompanied by visible motions of the head and lips. A slight feeling of muscle tension in the ear and back of the scalp marked one group from another. There was a feeling of innervation of the muscles connected with attention.

When the attention was directed to respiration the grouping was not affected. Respiration was more inclined to follow the grouping. The heart-beat coming in about the same time as the accent in a 4-group, tended to coincide with it. Time, .2 sec. The tendency to group was still present in a small degree. The sound was quieting. It suggested slowly dripping water. Time, 1.5 sec. The suggestion was of a big clock. After listening to a fast rate for a time and then to the rate of .687 sec., he decided that he felt no tendency to group the sounds of the latter. Time, .116 sec. The most natural group was eight, with a slight tendency to divide into

two 4-groups. Time, .134 sec. The subject found a 4-group more natural than an 8 with this rate, but felt some tendency to make a double 4-group instead of a simple 4-group. Time, .116 sec. An 8-group composed of two 4-groups was most pleasant. Time, .268 sec. His most pleasant group was two, but these groups tended to combine to form double 2-groups. When two strong clicks followed by two weak ones formed the series, he grouped by fours, but accented the second and fourth. He described the phenomenon as a summation in the second: "The after-image of the first was left to the second to increase its strength." When the subject heard the rhythm of the chronograph, he grouped the sounds accordingly.

Subject 10. Some musical talent and training.

Time, .3 sec. The 4-group appeared immediately. The subject could suggest other groups of three, five, six and eight. The 3-group was accented upon the first, and the 4-group upon the first and third. During one experiment the subject said the accents in the 4-group were not distinguishable, but the groups were separated by a slight interval. The 5-group was accented upon the first and fourth, the 6-group upon every other one, and the 8-group was a repetition of two 4-groups. Higher groups of threes as far as four were easily obtained. The first and third groups of threes were accented. Higher groups of fours were not easy or distinct. During all the experiments unconscious movements in the tongue were present. A slight muscular contraction took place with the accented click. Other movements of the head, trunk, feet and hands were visible, and the subject found it difficult to restrain them. Time, .134 sec. These sounds were grouped by eight and the grouping was pleasant and animating. Time, .156 sec. The first suggestion was of a 6-group. The 8-group was difficult. Time, .268 sec. This yielded to a 4-group, which the subject said required about the same time as the previous 8-group. Time, .78 sec. The 2-group was the only one practicable. The suspense for others was too great. Time, .116 sec. During this experiment the rate yielded most easily to a double 4-group, and when the time was changed to .45 sec. he had a similar feeling with the group of two, but one click stood in the place of the 4-group with the previous rate.

Time, .116 sec. Every eighth was accented. The subject got a very pleasant and "harmonious" 8-group. Time, .134 sec. Every eighth was accented. The subject was less animated. He said, "The group was more staid and steady. It had lost its tones." Time, .17 sec. Every eighth was accented. It now required an effort of attention to get the 8-group. It grew more pleasant as the subject became accustomed to it. Time, .268 sec. Every eighth was accented. The grouping was by four. Sometimes the subject accented every other one and felt disposed to count thus: one and two and three and four, and repeating this between the accented clicks.

Subject 11.¹ Some musical talent and training.

Time, .5 sec. The 4-group suggested itself immediately. The first and third clicks were accented, the first stronger than the third. Sometimes the third might be stronger than the first. It was possible to accent the second and fourth. When the subject gave close and critical attention to the sound, there was no tendency to grouping. The grouping seemed most clear with an indifferent state of mind. He showed a decided preference for 2 and 4-groups. Time, .25 sec. The 4-group was most natural. Time, .115 sec. This

¹The subject knew beforehand that this was to be an experiment in the rhythmical grouping of sounds.

rate yielded to an 8-grouping, each group being composed of four strong and four weak sounds. At other times with this rate the sounds seemed to rise and fall at regular intervals, which the subject described as a waxing and waning of the attention. Time, .167 sec. The subject grouped by four, but felt a straining for a larger group. Time, 1.67 sec. He grouped by two and visualized the pendulum. One click came during each half swing. When the subject gave attention to his breathing, he made an inspiration last during the time of one click, and expiration during the time of another. The first click was louder than the last. Time, .115 sec. When the subject gave attention to his pulse the groups corresponded to the time of the heart-beats. The click which came near the beat was louder and became the first in the group. The pulse seemed to reinforce the sensation of the sound. When the attention was directed to respiration, the clicks increased in intensity during inspiration and were grouped by two and decreased in intensity during expiration. He visualized a curved line which rose during inspiration and fell during expiration. Smaller undulations in the larger curve corresponded to the 2-group. A melody always appears to him as a zigzag line, in which the rises correspond with every two notes. Time, .156 sec. He grouped the clicks by eight and visualized an ellipse with four points upon either side. The clicks seemed to locate themselves on these points.

The subject showed a strong tendency to muscular movements. He felt an impulse to dance, clap the hands and tap the toes and fingers upon the accented click. When the rate was .286 sec., this tendency to muscular movements was stronger than with the other rates. There was something animating about this rate.

Time, .3 sec. Every fifth was accented. The clicks were grouped by five. The accented click always appeared as the fourth in the 5-group and longer than the others. When this click was further increased in intensity, it seemed very much longer than the rest and appeared as an extraneous sound which did not enter into the group. The other four sounds then formed a group by themselves. When every sixth was accented, the accented sound again appeared as an extraneous sound. It simply disturbed his mental habits of forming some other groups. When two clicks in every five were made stronger with one weak click between the two strong ones, the grouping was still by five but it was a combination of a 2-group and a 3-group. When three strong and two weak clicks formed a group, it was composed then of a 3-group and a 2-group. The 3-group contained two strong sounds and one weak, and the 2-group one strong and one weak. A short pause came after the fourth sound, which made it impossible to make the 5-group appear as composed of a 2-group and a 3-group. Time, .268 sec. Every third sound was accented. This accent simply broke up the tendency of the subject to group by four and did not compel him to group by three. When every sixth was accented, he grouped by six, and accented the first and fifth, but there was a strain towards a 4-group. Time, .167 sec. Every sixth was accented. With this rate the 6-group was pleasant and did not tend so strongly towards a 4-group. Time, .137 sec. Every sixth was accented. The 6-group was pleasant, and it tended to unite into higher groups of two with the pendulum-swing movement.

Time, .208 sec. When the subject listened to the sound of the chronograph, which made a distinct and strong 8-rhythm, he was unable to form any other group than eight. The 8-group was composed of two 4-groups, the first of which was much stronger than

the second. When he listened to the chronograph, which gave a 4-rhythm, which was composed of two 3-rhythms, he was unable for a time to get anything but a 6-group, but this faded out with continued effort and gave place to his previous 4-groups. The 4-groups were then grouped by two with the swing of the pendulum. Time, .134 sec. Every eighth was accented. The grouping was by eight, and the 8-groups were then grouped by two.

Time, .116 sec. Every eighth was accented, and the grouping was by eight, and pleasant. Time, .134 sec. Every eighth was accented. The subject took no spontaneous interest in the 8-group at this rate. The period seemed to be too long. "It breaks off with a dead end," he said. Time, .17 sec. Every eighth was accented, but the grouping was by fours. The accented click was simply a disturbing element. The series did not group easily by either four or eight. Time, .208 sec. Every eighth was accented. The grouping at this rate was distinctly by four. The accented click acted somewhat as a disturbing element. When every fourth was accented at this rate, the 4-grouping became pleasant, and the accented sound was the first in each group. The 4-groups were grouped by two with the swing of the pendulum. Time, .17 sec. Every fourth was accented, but the time seemed to be too fast for a pleasant 4-group.

When every sixth was accented, and the time .323 sec., the grouping was by three, but the tendency to a 4-grouping was so strong that it was possible to get a 4-group in which every sixth sound was accented, the accented sound shifting its position in the group. The accented click seemed longer, and a longer interval followed it. When a very weak sound was followed by a very intense one, the sound of the loud click spread itself over the weaker one.

Subject 12. Considerable musical talent and great interest in music. Accustomed to introspective study.

Time, .3 sec. The subject began immediately to count the clicks, accenting every third. He unconsciously rocked himself in the chair to keep time. He thought the rate slowed up at times and then quickened again. The grouping was changed from three to four by simply thinking of the number. He believed there was some unconscious muscular movement about the change from one rate to another. He could suggest a change by simply tapping with his fingers. When he changed from a 3-group to a 4-group, the 4-group seemed too long at first, though he became accustomed to it. In a short time the grouping seemed to change of itself into three and then again into four. The 4-group was inclined to fall into two 2-groups, the subject unconsciously nodding his head to every other sound. He was able to suggest a 5-group, in which the first and third were accented, the third more strongly. He could accent any click in the group, but the first and the third seemed easiest. Time, .156 sec. The 6-group appeared immediately and spontaneously, and then broke up into two 3-groups. He suggested a double 4-group, which gave rise to a feeling of a slower pace. It was not so distinct as the double 3-group. This had a kind of impelling force. The subject attempted to step in time with the double 3-group, and then with the 4-group. The double 3-group required a sprightly step. It was exciting. The 4-group at this rate did not appeal to him; it didn't take hold. This rate was more stimulating than the previous one. Time, .115 sec.

The subject dropped into a 4-group, but the three was found more stimulating. It was difficult, however, for him to put aside the

previous rate, and adapt himself to the new one. One click in each group, however, seemed distinctly louder than the rest. When he grouped by four, it easily passed into an 8-group, but the 8-group was not so clear as the 6-group. He imagined a wheel going around, making six clicks to a revolution. When he changed the telephone from one ear to the other, the grouping changed from six to a double 4-group, and persisted for a time. The 5-group came only with difficulty. Time, .76 sec. The grouping was by twos. The subject imagined the clock at home. The 3-group was suggested by an image of a musical conductor beating time. Time, 1.44 sec. The subject gets the rhythm of the pendulum swing without suggestion. He suggests also a 3-group, which recalls the time of church music. Time, 1.66 sec. He finds it easy to imagine intermediate sounds between the actual clicks, and these he groups by three, the real click being the accented click in the 3-group. Time, .286 sec. In order to obtain a notion of a rhythmic series—one of uniform intensity—the subject turned his attention backward, and saw a series of images to which he was adding one all the time. He throws his attention upon what comes, and studies the nature of the noise to see if the timbre is the same. It is a comparative effort. But in spite of all efforts the series groups into a 2-group at times. When a relay sounder was connected in the circuit of a vibrator, which made 20 vibrations to the second, the subject was still able to effect a grouping of the sounds into either 3 or 4-groups by tapping with the fingers upon the table. When he dispensed with the suggestion, the clicks of the relay signal were perfectly uniform, except perhaps a slight waxing and waning in intensity, due probably to the waxing and waning of the attention towards the sound. There was no real grouping.

When a longer interval was introduced every fourth, the clicks came in a group of four, but there was nothing satisfactory about the group. The clicks did not form an organic group. Each group of four stood rather as a single compound impression. There was no organic relation between the separate clicks in the group. When the rate was rapid, the groups of four were grouped into higher groups, the groups of four standing as single impressions. When the rate was slow the long interval might come between the groups or anywhere within it. There was something wanting, something to be looked for in the interval.

As the nature of the group, the subject described his feeling as a tendency to go back when he had heard three or four clicks, as the case might be. He says he has a "mouthful"—a unity—and when he has one, he seeks to get another. The same process continues to repeat itself. When he directed his attention to the timbre of the click, he got no grouping, but when he looked at the series as a whole, the grouping was clear and spontaneous. There was not, however, necessarily an accent in the group.

Subject 13. Considerable musical talent and training. A lover of 2-4 music.

Time, .285 sec. It suggests the gallop of a horse—a short gallop—and the clock. There is a breathless feeling about it. It is the sound of car wheels—the whole train. It has a double vibration. The clicks are grouped by two or by four. The group seems to close with a rising inflection; the last is apparently accented lightly, as the first is strongly. The 2-group prevailed over the four. Parts of "Erl King" are suggested by this grouping. An objective suggestion was displeasing to the subject. The subject preferred a mental suggestion in order to change the grouping from two to anything that was desired. By such a suggestion the sub-

ject was able to get most any group up to eight. The eight group was not clear; the accents were not sufficiently prominent. The shorter measures are more strongly accented. Time, .115 sec. This rate had a bad effect; it was tormenting. The grouping was by four of a particular pitch, followed by four of a lower pitch. The subject might group the clicks by two in the same way, but with less clearness. Time, .352 sec. This rate suggested something going around, and every other sound was accented. When the 3-group was suggested, the first click was accented, and the group closed with a rising inflection. Higher groups of 3-groups could be obtained as far as four. The groups seemed to rise and fall in intensity. At this rate also the short groups were more strongly accented than the long. When the subject suggested a 4-group, the first and the third were accented, the first probably stronger. The 4-groups may be grouped again by four. Twenty was the greatest number of clicks that the subject could grasp easily in this way. The grouping becomes lost and disconnected with larger numbers. The first groups in the larger groups were of greater intensity, and the last of a lesser intensity. The intensity of each succeeding group seemed to be less. This rate was said to have the most "aesthetic effect." Time, .268 sec. The 2-group was most easy; a double 2-group was pleasant. The general effect of this rate was a hurried feeling. The previous rate had been restful. Time, .156 sec. The 4-group was most natural, and was accented upon the first and the third. The 6-group appeared without voluntary effort. There may have been a mental suggestion of the six. Time, .78 sec. There was no real grouping. It seemed painfully slow. Time, 1.44 sec. The subject supplied two intermediate sounds between the clicks, and grouped by three. The actual click was the accented sound in each group, and came first. Time, 1.66 sec. The subject supplied three intermediate sounds between the clicks, and grouped by fours. The real sound came first. Time, .134 sec. The double 4-group was most natural, and the subject breathed with it. When every eighth was accented, the subject did not become aware of the accent. The grouping was spoken of as being so strong that it could not be gotten rid of. The groups of eight were grouped by two with the swing of the pendulum. The clicks in the 5-group seem to decrease in intensity from the beginning to the end. Time, .116 sec. Every eighth was accented. The movement was the same as with the previous rate, or perhaps in place of the pendulum movement the subject visualized an object moving up and down, the upward motion lasting during the time of an 8-group, and the downward motion during another 8-group. There was apparently a longer pause after the second group. The subject felt a strong tendency to nod the head, and keep the time by tapping the toe. Time, .17 sec. Every eighth was accented. The 8-group lacked completeness. It was not so smooth as the 5-group before; it was distinctly divided into two 4-groups. The accented sounds were generally unpleasant. The subject "has not the restful impression of evenness" which had characterized the uniform series.

Time, .323 sec. When the clicks were all of the same intensity, the slightest suggestion of any sort was sufficient to cause the clicks to fall into the group suggested. Even when the attention of the subject was not called to a suggestion, and the subject apparently did not attend, it would change the grouping to that suggested.

At times the subject had a feeling which was described as "awful," that the chronograph was slowing up and about to stop. When stronger clicks were introduced, the effect was unpleasant.

The following rates were given in rapid succession: Time, .268 sec. The clicks were grouped by two, and the 2-groups seemed to rise and fall in intensity at regular intervals. Subject could suggest other groupings, but it drifted back to this, unless the subject kept up the suggestion of some other. Time, .208 sec. The grouping was by four. The rate was unpleasantly fast for a time. Time, .134 sec. The grouping was by four; the 4-groups seemed to rise and fall in intensity, every other one being more intense. The subject unconsciously breathed with this secondary grouping. Every eighth was made more intense. The subject did not detect the accent, but said the grouping by eight was so clear that it could not be avoided. The 8-groups tended to group into 2-groups. Time, .116 sec. Every eighth was accented. The clicks were grouped by eight, and the 8-groups were grouped by a wave-like motion. There appeared to be a longer interval between every two groups. Time, .17 sec. Every eighth was accented. The grouping was primarily by two, and the 2-groups were grouped by four. The intensity of the clicks seemed to decrease from the beginning to the end. The grouping was rough in comparison with that for the previous rate. This form of grouping gave place finally to a double 4-grouping, and the subject was strongly inclined to keep the time by nodding or tapping with the toe. Especially strong was this impulse when strength of the accent was increased. Time, .208 sec. Every eighth was accented. The 8-group was now more distinctly divided into two 4-groups. This grouping had more "dignity and force, but was not so tripping as the fast rate was." The 8-group was not so complete as it was with the faster rates.

Subject 14. Some musical talent and training.

The first suggestion of a grouping was by eight, and the 8-group was divided into two 4-groups. When a 2-group was suggested the subject agreed that he could get it, but the 2-groups were again grouped by two into 4-groups, and the 4-groups by two into 8-groups. A 6-group was suggested by counting six, but there seemed to be a division corresponding to 4-groups. The subject was under the impression for a time that there was a longer interval or four different intensities of sounds which made this 4-grouping. The 4-group was accented upon the first and the third. The 3-group did not succeed very well. The subject seemed to have a habit of forming groups of two, and the strongest kind of a suggestion was not sufficient to put it aside for a 3-group. Time, .156 sec. The 8-group, which was divided into two 4-groups, was the most natural, and seemed to prevail over all others. Time, .78 sec. The 2-group was most easily obtained, but it was possible to suggest either a 3-group or a 4-group. The subject was not sure whether he preferred a 2-group to a 4-group. He also found the 3-group quite pleasant. Time, 1.44 sec. The 2-group was most natural, and the subject could still suggest either a 3 or a 4-group, but when he dispensed with suggestion, he returned to the 2-group.

The subject has noticed rhythms in the sound of mill wheels. When he gave his attention to these sounds he visualized a series of points on a line which he counted by four or two. When he was asked to count a series of dots, he said they were divided off into twos by a bracket above them. It has always been a habit with him to count objects by two.

When every fifth was accented, he grouped by five; the accented click came fourth in the group, and it seemed longer than the rest. When the accented click was made more intense still, its time seemed longer than the rest. When one of the five was made

weaker than the rest, they formed a somewhat irregular group that was unpleasant. The weak sound caused a disturbance in the group which was not present when a louder sound was introduced. When all the clicks were made more intense, the rate seemed to be slower than at other times.

Subject 15. Some musical talent and training.

Time, .3 sec. The sound suggested the clock. It was more easy and natural to regard every other one stronger. Groups of three, four and five were suggested. The 4-group was the most natural; the first and third clicks were accented. At times the 4-group seemed to divide into two 2-groups. When the subject attempted to compare the 3-group with the 4-group in point of their agreeableness, the three group appeared as three, with one sound coming between the groups, thus: 1, 2, 3, 1,—1, 2, 3, 1,—1, 2, 3, 1. This extra sound seemed to occupy a blank space between the three groups. During other experiments afterwards, the 3-group appeared in this form. The 4-groups were easily grouped by two. They would combine into no higher groups as simple 4-groups. The subject was able with great effort to combine two double 4-groups. When the subject counted objects, he usually grouped them by twos. The objects seemed to be joined together by bars. Time, .57 sec. This rate was very quieting. The 4-group was most natural. The first and third or the second and fourth might be accented. A longer interval appeared between the separate groups. A long interval generally follows the accented click whether it is imaginary or real. The subject regards real accents as extraneous intruders. They introduce a long interval, and for that reason the series seems irregular. By irregularity he understood a difference in time interval of the clicks. The accented click seemed nearer to the preceding click than the others. When two real accents of unequal intensity were put into a group of eight, the interval following the more intense click was the longer, and gave to a series a very irregular appearance. When the accented clicks were dropped out, the series became regular again.

Time, .268 sec. This rate was very favorable for voluntary changes of grouping. He could suggest any grouping that he might desire within limits. During every experiment the subject manifested a strong tendency to some kind of muscular movements. Any kind of muscular contractions would suffice as a suggestion of a grouping. He said he either counted the clicks or made the proper muscular adjustments for counting. There was mental counting always at the start. He made unconscious movements with the eyelids. Motions of the head were clearly visible the whole time. When the subject was asked to restrain all movements of which he was conscious, he said there was great difficulty in keeping the grouping. The telephone was disconnected, and the subject was requested to restrain his muscular movements or attempts to count. When the telephone was connected again, he said that the grouping had kept up during the interval. Although he had restrained all visible motions, slight muscular contractions were observed in the eyelids at the proper intervals of the accented clicks. He said it was possible to keep the grouping by imagining a series of colors passing before the eyes. He spoke of a feeling in the eyes as "muscular color sensation." He seems to have felt an adjustment of the muscles ordinarily used in visual attention. At no time was he conscious of the muscular contractions of the muscles in the eyelids.

Time, .57 sec. Every fifth was accented. The series was grouped by five and the accented click came anywhere in the group. It was

more generally and naturally near the first place. Time, .268 sec. The 3-group could be suggested, and was more naturally accented upon the first, sometimes upon the third. The 6-group was strongly accented upon the third and slightly upon the first and fifth. With a uniform series, the 5-group required a distinct effort and was then accented upon the last. In general the long and complicated groups were less differentiated; they ran together. The 6-group broke up into two 3-groups and the 8 group into two 4-groups. A 7-group was very difficult to get. It would run readily into an 8-group. Time, .134 sec. Every eighth was accented. The 8-group was pleasant at this rate. When the time was .116 sec. and every eighth accented, there was a tendency to group the 8-groups by two. During a subsequent experiment when the time was .116 sec., the series seemed to rise and fall in intensity with no definite grouping. Whenever an accent was put in, it made the series irregular and unpleasant. The series became pleasant in proportion as it was uniform, and with this rate the subject perceived only a rhythmic rise and fall in intensity.

Subject 16. Considerable musical talent and training.

Time, .3 sec. His first suggestion was that every other one was stronger in intensity, the stronger one coming first in the group of two. For a time, the subject did not discover that the sounds were uniform. He could suggest a 4-group, in which the first and third were accented, the first stronger than the third. It was difficult to get a 5-group, but when the subject did, the accents were upon the first and third. The 2-groups might be grouped by fives, in which case the first and third 2-groups were more intense than the others; 4-groups of twos were accented upon the first and third 2-groups; 2 and 3-groups of twos were accented upon the first. Higher groups of 3-groups as far as five were possible. The accents were the same as for higher groups of twos. Three-groups of three were the most pleasing. Higher groups of four were more difficult. The accents could not be kept clear. From early childhood, the subject has observed and taken pleasure in the rhythms in the sounds of the fanning mill, feed cutter and other machinery. The 4-rhythm was the prevailing rhythm with him. The puffs of the locomotive are grouped by fours, the first and the third being accented, the first stronger than the third. He associates the pendulum with the 2-group. With the 4-group, he associates the locomotive or a wheel turning around, making four sounds to each revolution. The 3-group generally requires attention to keep it and a suggestion to begin. The 5-group breaks up into a 2-group and a 3-group. The 6-group generally divides into two 3-groups. Time, .176 sec. This rate seemed most favorable for a 6-group. It was composed of two 3-groups, the subject visualizing the pendulum which grouped the 3-groups by two. In general, the subject preferred short groups to long ones. The shorter groups were simpler. He preferred also his own accents to real accents. When he listened to the sound of the chronograph, which was distinctly rhythmical, he grouped the sounds accordingly. When he was dull and tired, faster rates were generally more satisfactory.

The following rates were given in rapid succession: Time, .323 sec. The clicks were grouped most easily by the pendulum-swing movement. The subject could visualize a revolving wheel which made four strokes during each revolution and thus group by four. Time, .263 sec. The 4-grouping was decidedly pleasant and compelling. It required an effort of attention to group by three. He visualized the locomotive wheel with the 4-group. He could group the clicks by two with a pendulum-swing movement, but "it was

too fast to be real natural." Time, .137 sec. This was "a train at full speed." The rate was more pleasant and enlivening than any previous rate. It required very little effort of attention to get either a 3 or 4-group. A 6-group was easily suggested, but the group divided easily into two 3-groups. Time, .208 sec. The 6-group was less easy than it was with the previous rate. Time, .137 sec. The clicks grouped readily by three or four. Higher grouping of 3-groups by two or three required a suggestion to start, and it seemed to continue of itself; 4-groups might be grouped by the pendulum-swing movement. Every sixth was accented. The 6-grouping was necessary and pleasant. The accented sound took away the effort that had been required before for a 6-grouping. The 6-group might be divided into three 2-groups or two 3-groups. The accented sound always came at the beginning of the 6-group. Time, .167 sec. Every sixth was accented. The 6-group divided easily into three 2-groups or two 3-groups. Time, .323 sec. Every sixth was accented. The grouping was by two. The accented sound grouped the 2-groups by three. The span for a 6-group was disagreeable. It was too long. The accented sound might be overlooked and the series grouped by four. Time, .167 sec. Every sixth was accented. It was less easy to overlook the accented click than before. The accent forced a grouping by three.

Time, .263 sec. It was most natural to group by two with the pendulum-swing. Time, .208 sec. A 4-group was most easy. When the subject heard the chronograph, which gave a 6-rhythm compounded of two 3-rhythms, he grouped the sounds accordingly. Time, .134 sec. A 3 or a 4-group was equally pleasant and easy. The sound of the chronograph, which now gave an 8-rhythm compounded of two 4-rhythms, compelled a grouping of the sounds accordingly. The following rates were given in rapid succession during a single experiment: Time, .268 sec. A 2 or a 4-group was easy. A 3-group could be suggested. Time, .208 sec. A 3-group was suggested, but a 2 or a 4-group was easier. Time, .17 sec. A 3 or a 4-group was equally pleasant and easy. There was no preference. Time, .116 sec. The series could be grouped by three or four. When every eighth was accented, the grouping was by eight. At first, the 8-group divided into two 4-groups. This disappeared, and the 8-group became pleasant and agreeable. Time, .134 sec. Every eighth was accented. The 8-group divided easily into two 4-groups. The span was too long. There was no satisfaction in the 8-group, for the accent did not come soon enough. Time, .208 sec. Every eighth was accented. This was distasteful. The feeling of suspense present before was greater still. Time, .268 sec. Every eighth was accented. The suspense was still greater, and the 8-group broke up into two 4-groups. Time, .116 sec. There was no accent. This rate, which had given before an agreeable 8-group, when every eighth was accented, yielded to an 8-grouping. There was a slight tendency for the 8-group to divide into two 4-groups, the first of which was more emphatic.

Subject 17. Some musical talent and training. Accustomed to introspective study.

Time, .3 sec. In the first place, the grouping was by two, and almost immediately and without effort it changed to a 4-group. When each click was attended to separately, they all appeared to be of the same intensity. Suddenly the subject began to group by four. He felt a tendency to count it off to himself. Sometimes the 4-group appeared as two 2-groups. Then he thought there was an irregular interval—a difference in the time of the clicks. He then imagined that a fainter sound was heard between the actual clicks.

Each click was grouped with the fainter sound following it, and these groups grouped by two. Breathing seemed to accommodate itself to the 4-group; inhalation lasted during one group of four and exhalation during another. When every third was accented and time .208 sec., the subject felt a strong tendency to inhale during one group and exhale during another.

Each group is attended with the feeling of having completed a member of the rhythm. The groups stand out as unities—as wholes—and as each group becomes complete, there is a striving for the next. The subject has a tendency to count the clicks by fours or other numbers. When he attempts to suggest a 3-group, the third click seems to repeat itself thus: 1, 2, 3, 3,—1, 2, 3, 3,—1, 2, 3, 3. He succeeded, however, in getting a real 3-group by counting and nodding the head with the accented click. When he attempted to group by five, the accents seemed to crowd along until it brought six into the group. The first three clicks seemed to come in the time of two and the rest were irregular. When he succeeded in getting a 5-group, it was accented upon the second.

Time, .965 sec. The 2-group was the most natural, but it was imperfect. Time, 1.615 sec. The subject was able by strong effort to group by two, but the sounds seemed more naturally to appear uniform.

Subject 18. No musical talent and no interest in music.

Time, .352 sec. This was a very pleasant rate. Other rates seemed either too slow or too fast. By no suggestion could any kind of grouping of the sounds be effected. The subject declared that they were all uniform in intensity.

Subject 19. Some musical talent and in training at the public school.

Time, .268 sec. The subject likened the series to dropping water. It was suggested to him that perhaps some sounds were louder than others, when he said that every fourth seemed louder. Again it was suggested that possibly every third was louder, but the subject would not agree to it. When every third was strongly accented, the grouping was by three. When the accent was dropped out, the subject returned to a 4-group. When he listened to the sound of the chronograph, which was making a double 3-rhythm, he grouped the sounds accordingly.

Subject 20. Some musical talent and good training.

Time, .268 sec. The sound was likened to dropping water. It was suggested that the clicks grouped together in some way, and the subject replied that they were grouped by four. Again it was suggested that some other grouping was possible. This, the subject said, was by three. After reflecting and counting for a moment, the grouping was thus: 1, 2, 3, 1—1, 2, 3, 1—1, 2, 3, 1. The first and third were accented in the 4-group.

Subject 21. Physicist.

Time, .30 sec. The sound suggested the pendulum. A loud click corresponded to one swing and a soft to the other. He visualized a conical pendulum, which struck at several points in its swing and thus grouped the sounds by other numbers than two. He seemed to attend now to the series of clicks and then to relax and attend again. During the "strains of attention," he might grasp three or four clicks. A feeling of relief followed each strain of the attention. All the muscles of the body seemed to point toward the source of the sound. They alternately contract and relax with the successive strains of the attention. The first click in each group was accented.

Subject 22. Some musical talent.

Time, .3 sec. The clicks were grouped by four. Time, .78 sec. This rate was too slow for any grouping. It did not even suggest the clock tick. Time, .156 sec. This rate was too fast for easy grouping in any way.

Subject 23.

Time, .268 sec. The prevailing group was four. It was difficult to suggest any other. The sound of the chronograph, which gave a 6-rhythm compounded of two 3-rhythms, was scarcely sufficient to break down the tendency to group by four. The subject had worked in the same room with the chronograph, and had become more accustomed to the 8-rhythm than to any other which the chronograph made.

Subject 24.

Time, .268 sec. The clicks grouped immediately by two. There seemed to be a difference in quality. When every fourth was accented, they were grouped by four. A longer interval preceded the accented click. When every eighth was accented, the clicks were grouped by eight and a longer interval preceded the louder sound.

Subject 25. Some musical talent and training.

Subject has noticed his tendency to group objects and sounds before the experiment. Objects passing rapidly before the eyes are grouped by eight, those passing slower, by four, and those passing very slowly, by two. Time, .78 sec. Every other sound appeared to be of sharper tone than the rest. The sharper toned click grouped with a weaker and came first. Time, .115 sec. He grouped by eight. When he gave attention to the pulse, he seemed not to hear the clicks coming near or just after the heart-beat. The clicks between the heart-beats were more distinct. No grouping of the sounds would persist long. The accented sound in the group generally came first, but it might come anywhere in the group.

Subject 26. Some musical talent and training. Laboratory boy.

Time, .323 sec. The most natural form of grouping was by two. The first was accented. When he suggested a 3-group, the rate seemed to be slower, and then the clicks seemed to be of the same intensity. When every third was accented, the accented click came first in the group, and was preceded by a longer interval. In whatever position an accented click stood, it was preceded by a longer interval. With uniform sounds the 4-group was accented upon the first and third; the first was stronger than the third.

Subject 27. Some musical talent and training.

Time, .3 sec. The most natural form of grouping was by two. The first was accented. He was able to suggest groupings by three or four. The first sound in either group was accented. By tapping with five fingers, and striking much harder with the fifth he was able to suggest a 5-group. It seemed to be a matter of the imagination largely whether there was a rhythm. When he thought of a clock or some other rhythmical machine, the series tended to group according to the suggestion. The sound was most naturally associated with dropping water.

Subject 28. No musical talent.

Time, .536 sec. It was possible to group the series by three, four or five. The 4-group was most natural. From early childhood the subject has observed the 4-rhythm in the puffing of the locomotive especially, and in later years the same rhythm has been observed in clocks, metronome, hammering, walking, and in all auditory impressions that approach a regularity in sequence. The rhythm is clearest in the sound of the locomotive. The first and third

sounds in the group are accented; the first is generally more strongly accented than the third. When the sounds of the locomotive become very rapid there is no definite grouping, simply a periodic rise and fall in intensity. Time, .268 sec. This was especially favorable for the 4-group, and the 3-group could not be easily suggested. Time, .208 sec. The 4-group was most natural. It was possible to suggest a 2-group by striking heavily on every other sound. The grouping, however, was very monotonous. Both the 3 and the 5-group were very difficult. Time, .268 sec. Although the 3-group was difficult at this rate before, it could be easily suggested this time. Time, .17 sec. The clicks were grouped by four and the 4-groups tended to group by two with the pendulum-swing movement. If the grouping was held down to a plain four, it became unpleasantly monotonous. Time, .134 sec. The series tended to appear in the form of a periodic rise and fall in intensity. The periods were about equal to the time of an 8-group, and with a slight voluntary effort the series grouped by eight. The 8-groups tended to group by two with the pendulum-swing movement. During a subsequent experiment with the same rate, the subject felt a tension in the eye muscles which grouped the series by eight; four sounds occurred during the upward movement and four during the downward.

Subject 29. Some musical talent and training.

Time, .268 sec. When the subject thinks of a clock the series groups by two, but when he thinks of hammering, the clicks appear to be of the same intensity. He could suggest a 3 or 4-group, but the 2-group was most natural. Time, .208 sec. He finds it easy to count almost any rhythm as far as nine. The longer rhythms tend to divide into shorter ones. The subject found it difficult to keep from thinking of a clock tick, which suggested the 2-group. Time, .17 sec. The subject still grouped by two and thought the rate seemed to be faster when he grouped by two than when he suggested other groups.

Subject 30. No musical talent.

Time, .268 sec. By no suggestion was it possible for the subject to effect any kind of grouping of the sounds. It appeared as a dead monotonous series, with which he could not avoid the association of a pile-driver.

Many other persons who simply came in as visitors, were experimented upon with results which confirmed the foregoing records. No especial account was taken of them. More than fifty persons in all were experimented upon, and only two failed to effect some kind of grouping in the clicks which they heard. In general it may be said that the younger and less educated yielded more easily and quickly to the suggestion of a rhythmical grouping.

The first point in the preceding records to which attention is called is the rhythmical grouping of the sounds. The grouping was the same in every case. It was accomplished by accenting regularly certain sounds more than others. The weaker or less accented sounds seem to run together with the stronger, and to form organic groups which are separated from one another by intervals which are apparently longer than the interval which separates the individual clicks. Such rhythmical grouping has been observed frequently at other times by many persons. Several of the subjects testify

to have known of their tendency to group the puffs of the locomotive, even in early childhood, and they have taken great delight in it. With us this habit of grouping the puffs of the locomotive when it was starting slowly or pulling up a grade became so strong, even in early childhood, that it led to all kinds of speculation as to the cause. The puffs are grouped by four. The first and third are accented, the first generally stronger than the third. No other grouping ever seemed possible until it was found in the experimental work that the tendency to group by four was only a habit or association. The puffs of a locomotive may now be grouped by two or three, but the association of the drive-wheel making one revolution to four sounds renders any other form of grouping than by four difficult. When the engine runs very fast, the sounds seem to rise and fall in intensity at regular intervals.

A kind of rhythm is also observed in the noise of mill-wheels. The winnowing machine and feed cutter, such as are found upon many farms, produce a rhythmical sound which few persons fail to observe. Long association in early childhood with such rhythms stamps them upon the mind so firmly that they become a mental habit. Children either fancy or perceive rhythms in many sounds; they indicate this by their attempts to reproduce the sound of machinery or of locomotives. Some railroad engineers believe their engines sing tunes. The same engine under like circumstances always sings the same tune.

Several experimenters have also observed this same grouping of rhythmic sounds. In the work undertaken by Dietze¹ in Wundt's laboratory upon the *Umfang* of consciousness, this rhythmical grouping of the sounds of the metronome was observed and employed to determine the length of the mental span. The grouping was accomplished by intensifying voluntarily certain sounds and subordinating others to it. By grouping the sounds first by eight and then the groups of eight by five, it was possible to grasp forty sounds. Wundt says it is impossible to restrain this grouping absolutely. It may be confined to a 2-group, beyond which it cannot go within certain limits. Four sec. is the lower limit, and .11 sec. is the upper limit. The most favorable rate is .2 to .3 sec. Wundt refers this grouping to the ripening of the concept on the wave of apperception. As we shall see later, it is possible to restrain this tendency to group sounds. The difficulty was with Wundt's apparatus. The two sounds heard during a complete swing of the pendulum

¹Wundt, *Physiologische Psychologie*, Vol. II. p. 73.

of the metronome are not of the same intensity or quality, and hence the impossibility of restraining the grouping by two.

Angell and Pierce,¹ in their experiments upon attention, state that one subject noticed a rhythm in the sounds with which he felt a tendency to muscular contraction—nodding of the head and beating time with the fingers.

In neither of these experiments could the experimenters be sure that there was not some difference in the sound which would suggest a rhythm. The importance of an absolutely uniform series of sounds cannot be too strongly insisted upon. A difference in sounds which would ordinarily remain unnoticed, is sufficient to suggest a rhythm. This will be seen when we come to discuss the voluntary changes of the grouping and the ease of suggesting such a change. In the present experiments the greatest precaution was used against any variation in the sounds that would suggest or impose a grouping. The only possible source of such a variation would come from a difference in the resistance between the mercury and platinum. If the mercury were dirty or the platinum points were not sufficiently immersed to form a good contact, or the mercury were to adhere to the points as they were withdrawn, a difference in the intensity of the sound might be heard. The mercury was carefully cleaned every few days, or fresh mercury put in. The platinum points were filed smooth and kept brushed. Strong elastics were attached to each key, so that when the keys were released there was no delay about reacting. If then there were any variations, since there were five sets of keys, it ought to recur every fifth sound; but as a 5-rhythm was always found very difficult, and a 2, 3 and 4-rhythm easy, we have strong ground for believing that any variations except those which were intended were so small as to have no influence upon the rhythmical grouping. We have, then, the testimony of all the subjects that the clicks seemed uniform in intensity.

Subject 2 always heard a uniform series for a time after a change of rate, or at the beginning of a new experiment. His tendency to group was so strong that he could avoid it only by imagining some one pounding in the distance, or some objective thing that was perfectly uniform. Subject 3 did not feel any tendency to group the sounds until after he had tried several suggestions. Subject 9, taking a critical attitude, was inclined to believe for a time that the clicks were all of the same intensity. After a few moments it required an effort, which was like "looking long into the

¹AMERICAN JOURNAL OF PSYCHOLOGY, Vol. IV. pp. 534 and 539.

future," to avoid a grouping. "I find no rhythm," he says, "as long as I hold my breath and stick to it." When subject 11 gave close and critical attention to the sound, there was no grouping. In order to get a notion of a *rhythmic series*—one of uniform intensity—subject 12 turned his attention "backward" and saw a series of images to which he was adding one all the time. He throws his attention upon what comes, and studies the nature of the sounds to see if the timbre remains the same. Subject 17 says that when each click was attended to separately, they all appeared to be of the same intensity. He said he experienced no such difficulty in avoiding a rhythm as the statement of Wundt had led him to suppose. Subject 25 could group the sounds, but he was more inclined not to do so. If he suggested a grouping, it did not persist. Subject 27 found it more natural to associate the sound with dripping water. Subject 29 made the series appear uniform when he thought of hammering. Subjects 18 and 30 could not effect any grouping at all. Upon this evidence we may safely rely upon having secured a series of impressions that was uniform for sensation. It is also true that though the rhythmical grouping of a series of uniform sounds is difficult to avoid, this tendency may be restrained within the limits spoken of by Wundt. Our own experience tallies with those above. When the attention is directed to each single impression, and an attempt made to study the timbre, it is possible to restrain the rhythmical grouping of the sounds. But when the series is attended to as a whole, this grouping takes place involuntarily.

The character of the sound employed in the experiments of Dietze differed greatly from that used in these experiments. The click of the telephone is about as simple and instantaneous a sound as it is possible to produce. The plate in the telephone vibrates a very short time. For that reason its chief characteristic is intensity; it does not persist long enough to establish its pitch and timbre. The mind has very little to work upon. It can construct variations only in intensity, for which reason the carrying power is greatly reduced. The sounds can be subordinated with respect to intensity only, and unless great intensive variations can be made, the mind will lose its grasp, and the grouping break up into single impressions. This phenomenon was observed several times, and in particular by subject 15. The sound of the metronome which Dietze employed is full and rich and has greater carrying power. Any experiments upon the carrying power of the mind must take into consideration the character of the sound. Dietze was able, by strong voluntary effort, to carry the grouping much farther than any subject in

this experiment was able to do with the clicks of the telephone. The explanation is to be found partly in a difference between the two sounds and partly in a different method. The subject in these experiments was requested to group the sounds, not by voluntary effort, but only so far as it was found easy and spontaneous. There was no attempt to force the grouping as far as possible, or even to force the grouping at all. It was the spontaneous and involuntary grouping that was studied.

In a study like this, which is purely introspective, the experimenter must rely upon the integrity of his subjects. There is and can be no test of the accuracy or truth of the results, except the uniformity which they show. If, however, each subject is unaware of the object to be obtained by the experiment, and of the opinions of every other subject, and renders his judgment without any interest in the results or without any preconceived notions of the experiment, the judgments are no more subject to error, and have about the same value as judgments in psycho-physical experiments. Certain attitudes, habits, and characteristics of mind do, however, affect results in certain ways which are injurious to the experiment. Some attention was paid to the attitude and method of the subjects in making judgments. A few words in regard to this may not be out of place. There are three classes of psychological subjects. The first includes those persons who yield immediately to any suggestion that is offered. This attitude results, then, from a social practice. In society, people do not wish to antagonize others. They instinctively give assent to any opinion. In an experimental investigation, if the operator will just give the slightest hint of his theory or preference they will add the weight of their opinions. If the operator leads them into giving an opinion which is opposed to his theory, "consistency becomes a jewel;" they stick to their opinion stoutly. If the experiment shows plainly that they are wrong and it is preposterous to hold such a view, they make a compromise with their former position and try to excuse themselves for having been led astray. They remain respectfully silent afterward and avoid, if possible, giving an opinion. If they are forced to make a judgment, they do it tentatively; they are not sure. Of a number of possible views they cannot make up their minds which is the correct one. They generally hair-split until they find out someone's opinion and then agree with that.

The second class of subjects includes those who take a moderately critical attitude. They are concerned in others' opinions in so far only as other opinions suggest different points of view. They give their own opinions when they

have considered all the phases of the experiment that are suggested to them. They are unconcerned about the outcome of the experiment. They are not dogmatic; they might have a different opinion under different circumstances or with further consideration. In the light of the evidence before them, they hold to a certain view.

The third class includes those persons who are excessively critical. They incline always to an opposite view. The experiment is not conducted properly to suit them; they are not in their best mood for judgment. They are sure to take ground against some one's opinion. If they cannot get any clue to others' opinions, they are doggedly silent or quibble, and refuse to answer except they qualify their answers to such an extent that the answer means nothing. This class of subjects is intellectually dishonest. If they are compelled to answer, they indulge in hair-splitting differences between their opinions and those of some others.

When the experimenter is compelled to rely entirely upon the judgment of his subjects, he must study them carefully and use the opinions of certain subjects in so far only as he finds that they harmonize with the general results. It is a fact which every psychologist must understand that certain classes of persons are incapable of introspection. The first class to which we referred are unfitted, because of habits of too free judgment and of always agreeing with others. The third class are rendered unfit for introspection from habits of too free judgment in regard to matters that concern themselves, and from an unnatural bias toward the negative. They are inclined to make too much of their individual opinions. In making out the results, the investigator cannot rely much upon individual opinion. Where there is almost perfect uniformity, the results may be given in tabulated form; but a large space must be given to merely individual opinion.

We have then to inquire first in regard to the certainty of a rhythmical grouping of a series of absolutely uniform sounds. The point does not need argument; the preceding records show how strong is this tendency. Only three out of fifty or more persons tested would agree that it was easier to hear each dick separately. In addition to the records given above, several subjects were asked to give a written statement of their impressions of the experiment. In one case definite questions were asked in writing.

(A) "As far as I can recall my impressions at the different occasions on which I listened to the series of sounds from your apparatus, they appeared to me always as a sequence of groups containing the same number of elements. The exceptional cases where the impression was that of a sequence of

single sounds, were those in which the period of the sequence was at its longest. For any given rate there was in general one certain number of elements of which the groups more naturally consisted than any other: but I found, too, that the sequence took on instantly the character of almost any other grouping that was suggested, whether by word or sound. As to the psychological nature of this phenomenon of grouping, it is a difficult matter to give an opinion. I found the effort to determine whether or no there were any recurrent differences of sensation in the sequence a great strain upon the powers of attention. The grouping had in general the appearance of being forced on the mind by the sounds rather than that of being imposed on them by it."

(B) "A series of clicks may be given in such manner that by giving the closest possible attention they seem to be uniform both as to intensity and interval. This degree of tension (of attention) can, however, be maintained for only a few seconds. When the attention is moderate, the clicks tend to fall into rhythmic groups, the number of clicks falling into a group varying with the rate of the clicks. Slower than a certain rate no rhythm is felt. With more rapid rates two clicks form a group, the accent falling on the first and an interval occurring after the second. Faster still, four clicks form a group with accent, primary on the first and secondary on the third, and an interval after the fourth. This seems a very pleasing rhythm to me, more so than any other. A still more rapid rate gives eight in a group. This becomes visualized quite strongly in my case. It is exceedingly difficult for me to hold the series of clicks out of some of these rhythms. They fall into one or the other types (according to rate) almost irresistibly. At some rates I was able to get a 3-rhythm, accented strongly on the first."

(C) "With regular ticks within certain limits, I do not perceive them as distinct separate ticks, but from the first I group. With slower rates, the grouping is two by two, which passes very easily into four, subdivided into two. With faster rates, the tendency is to perceive the grouping into fours, divided into two, or to perceive the grouping into threes. The quicker the rate, the larger the number of ticks entering into the groups up to about six. Below the lower limit, the ticks are first perceived separately with a tendency to fall into twos, this tendency decreasing as the rate decreases. Above the upper limit, the grouping becomes vague and the tendency is to perceive the ticks as separate and individual. In general the grouping can be changed within certain limits."

"The groupings influence one another. There is a tendency to become habituated to a grouping. A grouping heard in one rate is likely to repeat itself in a subsequent rate. It is difficult to be perfectly passive when one knows he is to find a rhythm."

(D) "It seems to me easier to group the clicks unless they are very slow; but I do not find it so difficult to perceive them singly as I should have inferred from Wundt's remarks on Dietze's experiments. Having now tried many times when the grouping was strongly present, subjectively (voluntarily) or objectively, I think I am a little more inclined to discover groupings. It seems to me that I do not lengthen, but rather intensify one or more of the sounds. Perhaps, however, the change is more in quality than in intensity, or perhaps an accompanying impulse of the diaphragm, stress in the mental counting, etc., etc. Possibly, however, I do also lengthen the stressed sound at the same time; but the lengthening is not so clear as the stress. I infer from my experience as a subject that the rates from 1 or 2 per sec., up to 6 or 8 per sec., are best; probably about 4 per sec. being the best. The fast rates are better for groupings by four, the slower for groupings by two. Three-groups, 5-groups and higher groups do not occur spontaneously with me, though 3-groups are not hard to start by counting. Perhaps 2-groups go easiest of all with me. There is a sense of expectation of 'hope deferred' when the rate is too slow — or, at least, a feeling of 'too slow,' like traveling in a slow train, although you have plenty of time.

"This probably increases with the length of the group. The span of the respiratory rhythm is exceeded, and instead of being able to tell off a whole foot of the rhythm with one breath, several breaths intervene between those that mark the accented sounds. With small groups and rapid rates there is a feeling of hurry. The motion is too quick and short. There is none of the repose—the swaying, the grace, the easy fulfillment of expectation that a slower rhythm possesses."

This rhythmical grouping was a series of efforts to attend to the sound. The grouping results from a sequence of acts of attention. When the attention is directed to the sensation, it lays hold upon the first impression with great force and makes it the sole object of consciousness. If this were the only sound, the attention would turn to something else, but as succeeding impressions follow before the first wave of attention has subsided, they are seized upon with less force than the first impression, and are subordinated to it in different degrees according to the strength of the apperceptive act. Subsequent waves of attention follow the same process as long

as the will directs the attention to the phenomenon. The attention accommodates itself to a certain number of impressions, which fall easily within the period of a wave, providing there is no objective difference in the impressions. If there is a regularly recurrent difference, this becomes the signal for a new act of attention, providing only that the span does not exceed or fall much under the normal period of a wave. If this recurrent difference follows at too great intervals, the attention breaks up the span in two portions, the one more emphatic than the other. If it follows at too small intervals, these periods fall together into group, first of two and then of larger numbers. The too great interval is marked by a feeling of suspense, and the too short interval by a straining after something more.

The number of uniform elements which may enter into a member of the sequence is not determined wholly by the time interval which separates them. Previous mental habits and associations influence the number of elements in the members of the sequence. All individuals are more habituated to two and its multiples than they are to three. There are also many associations which will suggest groupings by two and four. All ordinary muscular movements follow a rhythm of two. The associations of four are far more frequent than those of three. For this reason to a large extent, groups of two and four prevail. Several subjects have described this effort of attention in a manner which deserves notice and which shows very well the nature of the act.

Subject 7, speaking of his grouping by eight, says he is not able to "round up" until he comes to eight. There was feeling of completeness about the 8-group with a certain rate. Subject 9 says there is a slight feeling of muscle tension in the ear, sometimes in the back of the scalp. He attends, relaxes, and attends again. There is an innervation of the muscles connected with attention. Subject 12 describes his feeling about the grouping as a tendency "to go back" when he has heard three or four clicks, as the case may be. This is a "mouthful"—a unity, and, when he has one, he seeks to get another. Subject 11 describes his feeling as a series of efforts of attention. He grasps and grasps again. Subject 17 says each group is attended with a feeling of having completed a member of the rhythm. The groups stand out as unities—as wholes—and as each group becomes complete, there is a striving for the next. Subject 21: "I attend now to the series of clicks, then relax and attend again. During the strains of attention, I may grasp three or four clicks. A feeling of relief follows each strain of attention. All the mus-

cles of the body seem to point toward the source of the sound. They alternately contract and relax."

This is the rhythm in the attention to which a reference was made above. The view taken, then, was that only one undivided state of consciousness might arise during each pulse or wave of attention, and that the number of objects which can be grasped in that state must form an organic unity or be presented as a single object—have the appearance of a unit.

A given number of auditory impressions within certain time limits, when presented in such a way that there is a kind of subordination among them with respect either to time, intensity, pitch or quality, or with respect to any two or more of these properties, always stand as a unit in consciousness. They form an organic unity which is the essential condition of a number of impressions entering into a state of consciousness. If such organic unity does not exist and it is possible to make it, the mind imposes such an arrangement upon a given number of the elements that they may enter into a state of consciousness. *The essential conditions of forming such a unity among sounds is a regular temporal sequence within limits which shall be named hereafter, and perfect uniformity in intensity, pitch and quality.* Regular variations within limits with respect to intensity, pitch or quality, or to any two, or to all of these together, will effect a subordination among them sufficient to constitute an organic unity. There is a temporal limit within which these variations must occur in order to form such a unity.

The test of how many auditory impressions might be grouped together was the ease and pleasure which the subject found in doing so. If he were compelled to keep up a constant suggestion of a particular number in order to group the clicks so, no account was taken of it. If, after suggesting a grouping, it should persist until some other suggestion was made, the rate was considered favorable for that form of grouping. The subjects have described some groupings as most natural, easy or pleasurable, and others difficult or displeasing. The groupings which were spoken of as natural, easy or pleasurable, are gathered together in the following table, with the time, to determine what rates have been found best adapted to the different forms of grouping.

In the following table are brought together the judgments of all those subjects with whom extensive observations were made. The number of the subject is given in the first column at the left hand, and in the columns to the right are given the rates in thousandths of a second, at which a certain form of grouping was found pleasant and easy. The designations at the top of columns 1, 2, 5, 8, 10 and 13 are sufficiently clear.

The others require further explanation. In column 3 are given those rates at which the subjects found a 2-group more easy, but there was a straining for a larger group, or the 2-groups seemed to group by two. The rate was a little fast for a 2-group, and yet it was not more pleasant to group by four. In the same way certain rates were found at which a 3, 4, 6 or 8-group was easier than any other, but it was a little too fast for simply grouping by these numbers, and hence the groups tended to group by two. This was generally spoken of as the "pendulum-swing movement." Still other rates were found at which a 4, 6 or 8-group was more pleasant, and yet the rate was too slow, and the group tended to divide into two smaller groups. In column 15 are given those rates at which there was no distinct grouping—simply a periodic intensive change in the series. Rates at which there was no appearance of a group are given in column 16.

Multiplying the average rate for each form of grouping by the number of clicks in a group, we get as the length of groups :

Lower limit for no group,	1.581 sec.	Average variation,	.29 sec.
Average length of 2-groups,	1.590 "	"	" .328 "
" " " 3 "	1.380 "	"	" .204 "
" " " 4 "	1.228 "	"	" .068 "
" " " 6 "	1.014 "	"	" .028 "
" " " 8 "	1.160 "	"	" .025 "

The foregoing table shows that the lower limit for the rhythmical grouping of sounds is near 1.58 sec. Some subjects are able at times to group sounds that are separated by this interval, but as a general rule spontaneous grouping has ceased. The records give several instances where the subject has visualized the pendulum with this rate, but he had a feeling that the pendulum reached its full swing before he heard the click. The upper limit at which spontaneous rhythmical grouping ceases cannot be far from .115 sec. Several subjects declared their inability to make definite groups at a rate less than this. Others perceived only a periodic rise and fall in the intensity of the sound ; there was no definite grouping.

Between these limits there was some form of rhythmical grouping which depended in a large measure upon the rate. The average of all the rates at which a grouping by two was found easy and natural has been taken and multiplied by two to find the average length in time for the 2-groups. The same has been done for groups of three, four, six and eight. The averages for groups of all forms are found not to differ greatly, when we consider certain facts which influence the length of the group. The average length of 2 and 3-groups is somewhat greater than the average for groups of

[illegible]

six and eight. The explanation for this is to be sought in the fact that persons are more accustomed to rhythms of two and four than to the longer rhythms.

The average variation for 2 and 3-groups is greater than for groups of four, six and eight. The associations with the 2-rhythm are far greater than with any other, and these associations tend to suggest the 2-group where it would not otherwise occur spontaneously. Long experience with clocks that vary greatly in their rates of ticking has much to do with the wide limits within which the 2-rhythm is possible. The 3-group is a more rare form of grouping, and only a few subjects succeeded in suggesting it easily. For that reason it is not surprising that the average time of the 3-group should differ greatly from the others. Then the power to carry one or two impressions in the mind is greater, and they can be held longer. The actual span for two clicks in a given time is only a little more than half the span for eight clicks in the same time. In the first case almost half the interval is a pause between the groups, and in the second the pause takes up less than one-eighth of the interval.

There are several facts, as the records have shown, that tend to make the length of groups vary. Several subjects were predisposed to groupings by four. This number has had a peculiar charm for one from early boyhood. It was his number in school, and ever since, objects that were grouped by four, or that could be grouped by four, have had an especial attraction for him. Four impressions, of whatever sort, always arrest his attention. For this reason he attempted to group all rates by four, even though it required strong effort to do so. With two exceptions, all subjects had the prevailing tendency to group by four. A second fact, which influenced probably the grouping to some extent, was that when a subject found it easy to group a given rate by four he became somewhat habituated to a 4-grouping, and was inclined to group the succeeding rates by four, unless they differed greatly. If a very slow rate followed a faster one, which had been grouped by three or four, the subject tended to imagine intermediate clicks between the actual clicks, and still to group by three or four, as the case might be. (See the records of subjects 1, 6, 12, 13 and 17.) Taking all the forms of grouping together, the average time is taken to indicate the normal period of a wave of the attention which does not exceed greatly one second. A spontaneous effort of the attention, or with Wundt a wave of apperception, endures about a second or more. We do not, however, hold that there is an absolute psychical constant, even for the individual. No other fact is more certain than that the condition of the

subject, as regards fatigue and previous engagement, has much to do with the rate at which a certain group is found pleasant and agreeable. (See the records of subjects 1 and 3.)

Before leaving the subject, let us call attention to the averages for groupings that are intermediate between two and four, four and eight, and three and six. The averages for the rates at which these groups were observed lie between the average rates for the groups between which they stand.

A further method of testing the normal length of a spontaneous effort of attention was sought in this way. An accented sound every sixth or eighth was introduced into the series, and a number of different rates were tried, until one was found at which the group seemed most pleasing and natural. If the rate was too slow for easy grouping, the subject perceived a feeling of suspense. A slower rate still, caused the group to divide into two parts, or at least the subject felt a tendency to divide the group. A still slower rate generally caused the long group to disappear entirely, giving place to a number of small groups which were equal to the long one. If the rate were too fast for easy grouping by six or eight, the groups tended to group by two with a kind of pendulum-swing or wave-like movement. Before trying a subject upon an accented series of six or eight, he was given a number of rates with uniform clicks, beginning with a slow rate. The purpose was to determine to how great an extent the form of grouping changed with different rates, when they were given in close succession. The results of the experiment with a series of uniform rates are given in the first part of the following table. The results of the experiment when every sixth click was accented are given in the second part, and when every eighth click was accented, in the third part:

UNACCENTED SERIES, PART I.

Subject 7.	.323 (.268) 4-group. Tends slightly to 8-group.	.263 4-group. Strongly tends to 8.	.208 8-group. Pleasant.	.167 Groups not separated. Confused feeling.	.137 More confused feeling. Accented double 6-group.
Subject 16.	2-group.	4-group.		6-group. Not so easy as with the following rate	6-group. Groups by two.
Subject 3.	3-group.	Double 3-group.		3-groups. Groups by two.	
Subject 4.	.268 4-group. Pleasant.	.208	.17 4-group. Confused.	.134 4-group.	.116 4-group. Groups by two.
Subject 10.	4-group. Pleasant.		.156 6-group. Pleasant.	8-group. Pleasant.	
Subject 13.	2-group. Groups rise and fall.	4-group.	4-group. Groups rise and fall by two.		8-group. Groups by two.
Subject 16.	2 or 4-group.	2 or 4-group.	3 or 4-group.		8-group. Divides into two 4-groups.
Subject 9.	4-group. Tends slightly to double 4-group.			4-group. Strongly towards 8.	8-group. Divides into two 4-groups.
Subject 17.				8-group. Illusive.	8-group.
Subject 5.	.78 2-group.	.353 4-group.	Wavers between 4 and 8-group.	.183 8-group.	.156 8-group.

ACCENTED 6-GROUP, PART II.

	.323	.263	.208	.167	.137
Subject 6.	4-group. In spite of accent.	6-group. Suspense tends to 3-group.	6-group. Difficult.	6-group. Pleasant.	6-group. Groups by two.
Subject 11.		6-group. Tends to a 4-group.		6-group. Pleasant.	6-group. Groups by two.
Subject 16.	6-group. Two 3-groups. Span disagree- ably long.			6-group. Divides into two 3-groups.	6-group. Pleasant. Pendulum- swing movement.

ACCENTED 8-GROUPS, PART III.

Subject 4.	.268 4-group.	.208	.17 4-group.	.134 Two 4-groups.	.116 8-group.
Subject 10.	4-group.		8-group. Requires effort.	8-group. Not animating.	8-group. Harmonious.
Subject 11.		4-group. Accents disturbing.	8-group. Accents disturbing.	8-group. Not pleasant.	8-group. Pleasant.
Subject 13.		8-group. Not complete. Two 4-groups.	8-group. Divides into two 4-groups.	8-group. Pleasant.	8-group. Groups by two.
Subject 16.	8-group. Really 4-groups. Great suspense for 8-group.	8-group. Distasteful feeling of suspense.		8-group. Divides into two 4-groups. Span too long.	8-group. Pleasant.

With the unaccented series, the 6-group was found natural twice near the rate .167 sec. When every sixth sound was accented, the most pleasant rate for the 6-group was .167 sec. At the rate .137, the 6-groups group by two. At the rate .208 sec., they were difficult to grasp. At slower rates, there was a feeling of suspense, or the group tended to divide into two 3-groups, or the subject was more inclined to group by four in spite of the accent. According to this, the 6-group is found most natural and pleasant at the rate .167 sec. By multiplying this by six, we will get as the time limit for the 6-group 1.002 sec.

With uniform series, the 8-group was found most natural and pleasant, once at the rate .208 sec., once at the rate .134 sec. and twice at the rate .116. When every eighth was accented, the 8-group was found most pleasant at the rates .134 and .116 sec. The average rate for all is .130 sec., which, when multiplied by eight, gives 1.04 sec., the time limit for the 8-group. The difference between this and the time for the 6-group is very small, and at the same time they agree very well with the times for the same groups in the preceding table. The general fact of certain rates being better adapted to certain forms of grouping is pretty well established. This adaptation of a particular form of grouping to a certain rate depends upon the fact that the length of the group corresponds to the normal period of a wave of attention. The lack of adaptation results from cutting short the normal wave. For a fuller account of the different states of feeling arising

with different rates for a certain group, the reader is referred to the records of the experiments upon subjects 10, 11, 13 and 16.

The conscious state accompanying each wave of attention grasps together or unifies all the impressions that fall within the temporal period of a wave. As the result of a series of attentive efforts, a series of auditory impressions takes the form of a sequence of groups. This rhythmical grouping is due to the unifying activity of the mind; it is an attempt to conceive a series of sounds in a simpler form. When the mind acts upon a continuous series of auditory impressions, it groups all the impressions that fall within the period of a wave of attention, and conceives them as a single impression or a unity. Each succeeding wave groups a like number, so that the series is conceived in the form of groups. If the single impressions are separated by a greater time interval than the normal period of a wave of attention, each impression stands alone as the sole object of consciousness. But what becomes of the series when the rate is too fast for rhythmical grouping? A partial answer is to be found in the fact that the clicks show a regular periodic rise and fall in intensity. There was no separation among the groups; no definite number of impressions constituted the group. The view to which least objection can be offered, but which is unsupported at the same time by any positive evidence, is that when the sounds become too rapid to find expression in muscular contractions of any kind, they can be no longer separated from one another as simple impressions.

The most rapid rate¹ of voluntary control is about ten per second. This periodic rise and fall in the intensity of the clicks simply marks the waxing and waning of the attention. The changing intensity of the sounds indicates the changing degrees of clearness in the conscious state. If the clicks are separated by more than one-tenth of a second, the groups are separated by an interval; at least, there is the feeling of an interval. Below this limit of one-tenth of a second, the clicks preserve their individual character. They do not fall sufficiently near together to appear continuous. They preserve their temporal succession, and as before appear in different strengths according to the degree of clearness in the conscious activity. The conscious state, which seems to ride upon the crest of the wave—that is, appear when the attentive effort is at its strongest—fades gradually and conceives the last elements in the group with less clearness than the first, and with the coming of a new wave of attention, the first impression is laid

¹"Some Influences which Affect the Rapidity of Voluntary Movements." F. B. Dresslar. *AM. JOUR. OF PSYCHOL.* Vol. 4, p. 516.

hold upon with great force, and appears stronger in contrast with the last in the preceding group.

This rhythm in the attention, and hence in conscious activity, finds its counterpart in the activity of the nerve cell, which we have seen reason for believing was a series of explosions—an alternation of periods of activity and periods of repose.

The subject invites speculation, but we forbear except to offer the further supposition that with rates slower than ten per second, the interval or pause between the rhythmical groups marks a period of perfect quietude in the cell. When the rate reaches ten a second or more, there may still be a period of absolute inactivity, but no less interval than a tenth of a second can cause a real break in the conscious state or no less interval becomes an "object of consciousness." The change from one state of consciousness to another is represented by the reversal of a muscular movement. If between two impressions there is not sufficient time or time equal to the reversal of motion in a member, there is no consciousness of an interval between the impressions. The thought of the interval is a deduction and not a sensation or conscious fact—a fact revealed by the immediate conscious state itself. The conscious state disappears when the activity in the cell ceases; and when the will directs the attention to the series of impressions, the conscious state tends to disappear when it has effected all the subordinations that are possible among the impressions that fall easily within the normal period of a wave of attention.

Another phenomenon, which was observed by several subjects and by the experimenter at different times, was the apparent slowing up of the rate. The feeling was one of extreme suspense, and was described as "awful" and "dreadful." There was no apparent regularity with which the slowing up occurred and no definite time that the feeling lasted. The only fact which was observed concerning it was when the attention was diverted, the feeling disappeared. Several suppositions occur to us as explanations, but none of them seems to be completely satisfactory. It appears to be more in the nature of fatigue, but it is not clear why the impression should seem to be separated by longer intervals. The general fact, however, of time passing more slowly, when one is suffering from fatigue, has been observed frequently. A more probable supposition is that it indicates a kind of rhythm in the voluntary effort which directs the attention to the source of the sound.

We have now to ask what is the inherent nature of a rhythmical group, or what is meant by a unity among

a number of auditory impressions. What relation must the impressions bear to one another that they may be grouped together or grasped by a single act of apperception? How may a number of impressions become the object of a single state of consciousness? With Plato, we ask how the many become the one, or with Kant, how the mind makes a unity out of a manifold. Upon the basis of this study, we can hope to answer the question with regard to auditory impressions in a sequence. The question has already been answered for simultaneous sounds—musical tones—in the laws of harmony. The general principle as laid down in the treatment of poetry was that by coördinating and subordinating the elements to one another, unities were effected among them. The same principle holds good here.

From the nature of the apparatus, only changes in the intensity of the clicks could be effected. For this reason the subordinations and coördinations among the sounds must be accomplished through different intensities. Two methods for determining the relations of the sounds in a group with respect to their intensities were employed. By the first each subject was asked how he effected a grouping in a series of sounds which were of uniform intensity, and, if by accent, what sounds in the groups were accented. By the second method, the subject was given a series of sets of sounds of different intensities, which recurred always in the same order, and he was asked to point out where the series was grouped—the position of the strong and weak sounds in the group. In this way we were able to determine what was the most natural order in which the different intensities occurred in the group.

By the first method, it was determined that :

The first sound in the 2-group was accented. It was possible by objective suggestion of tapping, or counting, or by voluntary effort, to accent the last sound, but no subject would agree that this was the natural accent.

The first sound in the 3-group was strongly accented and the second slightly. Occasionally a subject found it easier to accent the second more strongly than the first, but this did not seem to be the natural way of accenting the group. It was possible by voluntary effort, or objective suggestion, to change the position of the accent. Very few subjects found it easy to group by three, and it usually required a strong suggestion to start the group.

The 4-group was very generally accented upon the first and third sounds; the first was stronger than the third. There was, however, some difference of opinion. Several subjects found it easy and natural to accent the second and fourth, and

subject 15 was more inclined to this form of accent than to the other. Sometimes there was only a single click accented, and this was very generally the first. The accents could be changed voluntarily. The reader is referred to the records of subjects 1, 2, 6, 7, 9, 10, 11, 13, 15, 16, 17, 20.

Most subjects preferred a grouping by four to one by three. When the attempt was made to suggest a 3-group by counting three, they felt an over-powering tendency to count one or three a second time. Thus: 1, 2, 3, 1—1, 2, 3, 1—1, 2, 3, 1—or 1, 2, 3, 3—1, 2, 3, 3—1, 2, 3, 3. The former was the more common. Subjects 1, 4, 7, 11, 12, 13, 15, 17 and 20 mentioned this phenomenon.

The 5-group was very difficult to suggest and maintain. Most subjects declared their inability to get such a grouping. Subject 11 said that an extra click would attach itself to the group and "pull it over" to a 6-group. Subject 17 mentioned a similar phenomenon. In counting a 5-group, it was found easy to emphasize the first and third or the first and the fourth. This gave to the 5-group the appearance of being compounded of a 2-group and a 3-group. Subjects 10, 11, 12, 15, 16 and 17 make observations on their attempts to suggest a 5-group.

The 6 and 8-groups were generally compounded of smaller groups of two, three or four. The 6-group was composed of two 3-groups or sometimes three 2-groups. The first group in the 6-group was more emphatic or was accented. The 8-group was composed of two 4-groups or sometimes four 2-groups. Subject 13 thought that the intensities of the sounds in the 8-group decreased from the beginning to the end.

Higher grouping of these groups was possible to some extent. The most common form was to group by two. This was spoken of as the pendulum-swing movement. In this case, the first group was always accented. Subjects 1, 10, 12, 13, 15 and 16 make observations upon their attempts to group 2 and 3-groups. The record of subject 16 is especially important. Several were unable to group 4-groups beyond two, on account of their inability to keep the accents clear. In general all subjects made a kind of interval between the groups. In 6 and 8-groups, which were compounded, a short interval followed each smaller group and a longer interval followed the whole group of six or eight.

Various methods of suggesting a grouping were employed. The most frequent method was by counting or beating time with the fingers. Subject 17 says: "Subjective counting is most effective, or this assisted by respiratory stresses and probably other muscular movements." The associations

which the sound brought up, very frequently suggested a form of grouping. The clock (various kinds), pendulum, locomotive, conical pendulum and revolving wheel, making a certain number of sounds during a revolution, are most frequently mentioned as influencing the form of grouping. The operator frequently directed the attention of the subjects to respiration, or asked them to feel the pulse. Most of the subjects incline to the view that respiration accommodated itself to the form of grouping that was found most natural with the rate to which they were listening. Inhalation and exhalation each lasted during the time of a 4-group. In this way a kind of higher grouping was accomplished, for the clicks heard during inspiration were more intense. When the rate was slow both inspiration and expiration were accommodated to the time of one click.

With fast rates, the pulse acted as a suggestion. All the clicks falling between two heart-beats were grouped together, the click coming nearest in time to the heart-beat being accented. Subjects 2, 4, 9, 10, 11, 13, 15, 17 and 25 make observations upon this subject.

When the subjects were allowed to hear the sound of the chronograph, which was distinctly rhythmical, no other grouping was possible. The reader is referred to the records of subjects 2, 3, 9, 11, 16, 19 and 23.

This general conclusion seems to be warranted: In the presence of any fixed rhythm within limits, or of objective suggestion, the series was grouped according to the suggestion, and it was found difficult, if not impossible, to suggest any other grouping. The grouping would follow the stronger suggestion.

Certain rates were more favorable than others for voluntary changes of the forms of grouping. Subjects 4, 6, 13 and 15 mention these rates respectively as especially favorable for voluntary changes: .323 sec., .353 sec. and .268 sec. Subjects 1, 2 and 16 thought that the grouping changed easily when they were fatigued. When a very weak accent was introduced every third in the series, subjects 4 and 13 did not detect the accent, but grouped the series by three, and were unable to suggest any other form of grouping; but they could not tell why the series grouped this way. Subjects 6, 9, 11, 12, 13, 15, 16 and 17 make observations upon easy changes of the form of grouping.

The second method of determining the nature of rhythmical groups was to give the subject a series which was composed of a regularly recurrent set of sounds of different intensities. Sets of two, three and four different intensities in groups of two, three, four and five were studied. Very few observa-

tions were made upon 5-groups. To make a graphic representation of such series of sounds, let A, B, C and D represent the four intensities of sound, A the strongest, and D the weakest. By using only two intensities (A B) it is possible to form the following series of sounds :

2-groups.	A B A B A B A B	(1)
3-groups.	{ A B B A B B A B B	(2)
	{ A A B A A B A A B	(3)
4-groups.	{ A B B B A B B B	(4)
	{ A A B B A A B B	(5)
	{ A A A B A A A B	(6)

Of 5-groups, these only were tried :

A B B B B A B B B B	(7)
A B A B B A B A B B	(8)
A A B A B A A B A B	(9)

The question was to determine where the mind most naturally made the division into rhythmical groups. The first series might divide in two ways, thus: A B—A B, or B A—B A. The second in three: A B B—A B B, or B A B—B A B, or B B A—B B A. Details regarding the others are unnecessary. Of series composed of three intensities, the following out of all the possible forms were thought to be characteristic, and were tried :

3-groups.	{ A B C A B C A B C
	{ A C B A C B A C B
4-groups.	A C B C A C B C A C B C

Of series of four intensities, the following out of the many possible forms were tried :

4-groups.	{ A B C D A B C D
	{ A D C B A D C B

In the following table are given the rhythmical groups which each subject made of the series upon which he was tried. At the top of the table, in each column, are letters which indicate the order in which the different intensities recurred in the various series. The number of each subject is given in the left-hand column. If a subject has given a stronger intensity to a click than it actually possessed (said that a B intensity was equal to an A, or a C to a B), it is printed in full-faced type. Where a subject has remarked upon a longer interval, either following or preceding the strongest sound, this is indicated by placing a dash either before or after the strong sound :

At the bottom of each column is given the form of group which is generally made when the series is formed in the way indicated by the letters at the top of the column. Two factors seem to operate in determining where the series shall be divided into groups. The group must begin either with a very intense sound or close with a very weak one. The subject strives either to put all the strong sounds as near the beginning as possible, or all the weak ones as near the close as possible. There are three cases where these principles are brought into strong conflict. The first is where the series is composed of three intensities, in the order of A C B or C B A. Either the strongest cannot come first, or the weakest last. The weakest generally comes last and the strongest second; the second being the position of the secondary accent in a musical rhythm. The other form which is common and sometimes preferred, places the strongest first and the weakest second. This does violence to both the weakest and middle intensities, by placing an unaccented sound in a position that requires an accent, and an accented sound in a place where an accent does not occur. A second case is where the series is composed of four different intensities in the order of A D C B or D C B A. The strife would be greater here than in the first case, if it were not for the fact that the third position may frequently receive the sound of the greatest intensity. The most common form is to place the weakest sound last, and the strongest in the third place. This of course leaves the first place occupied by an unaccented sound, though this sound is stronger than the last. Subject 9 makes the strength of the first equal to that of the second, and thus harmonizes the group somewhat with the 4-group, which is formed of uniform sounds. The third case is where the series is formed of a sequence of 4-groups of three different intensities, thus: A C B C. There is really no strife here, except that the strongest sound often appears in the third place. There are some irregularities, but none sufficient to require special notice. The general principle just laid down is well illustrated in the last two forms of the 5-group, when composed of two intensities of sound.

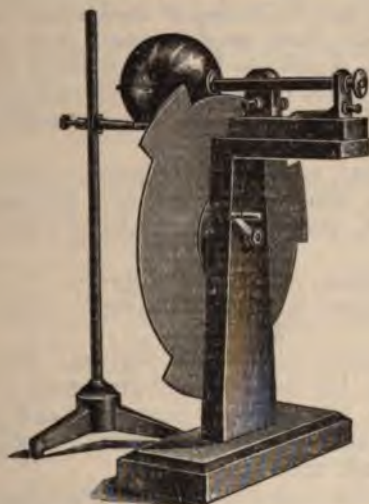
When two or more strong sounds, standing together, are followed by a weaker sound, the sound which is followed immediately by a weaker one appears stronger in contrast with the following weaker sound than the preceding, which is actually of equal strength. This will be observed in every case with 4-groups composed thus: A A A B. And in one case, with the 3-group composed thus: A A B. The third sound in the 4-group and the second sound in the 3-group appear to be stronger than the preceding sound. A

further fact to which attention is called, is the long interval which appears between the groups. The pause seemed to be due to the fact that a long interval generally preceded the accented sound. At the same time some subjects, especially 10 and 15, make a short interval after the strongest sound. To most subjects, the strongest sound seemed longer than the rest. With some this was more apparent than with others. Subjects 1, 3, 4, 10, 11, 13, 14, 15, 24, 27 and 28 either confound the accented click with a longer interval, or make the louder click seem longer than the others. Subject 11 speaks of the strongest sound spreading itself over the rest. It is possible by voluntary effort to avoid the illusion of a longer interval, either preceding or following the accented sound, but ordinarily it was very clear. When the strength of all the sounds in the series was increased, the rate seemed slower. Subject 27, especially, makes this observation. When these accented 3 and 4-groups were given at a fast rate (.134 sec.), the separate clicks seemed to fuse into a single impression, which grouped generally by four.

The different intensities of sound bore no special relation to one another; the strongest was clearly discernible from the second, and the second from the third, and so on. When in a series of impressions of the third (C) intensity, the strongest (A) was introduced every fifth, it seemed to appear as an extraneous sound which would not group with the others. (See records of subjects 9 and 11). It appeared from several other records also that sounds differing greatly in intensity would not easily group together. When a very weak accent was placed upon every third sound, subjects 4, 13 and others did not discover the accent; they expressed their inability to group the series in any other way, but could not understand the reason. Subjects 13, 16 and 17 expressed the opinion that strong accents were disagreeable; they preferred their own accents to real accents of any strength. Real accents did not seem to form so harmonious a group as did the accents which the subjects put in themselves.

As a further investigation into the nature of rhythmical groups, especially with reference to poetical rhythms, it was proposed to employ sounds of which the length or endurance might be varied. The click of the telephone is almost instantaneous. The disk probably makes a very few vibrations. We are indebted to Dr. E. C. Sanford for devising and constructing an apparatus, which served the purpose admirably in some ways. The principle involved in this apparatus was simply interrupting the sound of an electric tuning fork, which was placed before the opening to one of Helmholtz's resonators. When a card is placed over the

opening into the resonator, which is near a tuning fork of the same pitch, the sound of the fork is rendered almost inaudible. Regular interruptions result in a series of uniform sounds and silences.

FIGURE IV.¹

For this experiment were required an electric tuning fork and a set of disks with notches cut in the circumference. The resonator rested horizontally, supported by a stative near the edge of a small table, upon which the tuning fork was placed. The fork and the resonator were placed at the same height, with just enough space (about half an inch) between the end of the fork and the opening of the resonator to allow a pasteboard disk to pass without interference. This disk was about twenty inches in diameter, and placed at just the proper height to cover up the opening into the resonator. Notches were cut in the circumference of the disk in such a way that when it was revolved the opening into the resonator was now closed and now open. With a regular revolution of the disk, and with notches of an equal number of degrees, and equal spaces, a series of sounds, uniform in length, pitch and intensity, would be produced. To get sounds of different lengths, some notches were made to cover a greater number of degrees of the circumference than others.

¹This represents the apparatus as it has since been perfected by Dr. Sanford.

The number of degrees in some cases was twice that of others. By cutting the notches upon the arc of a circle, and at just a sufficient depth to cover a part of the opening of the resonator, it was possible to decrease the strength of the resonance, and thus get a sound of less intensity. This gave the effect of an accent upon certain sounds by weakening others.

The following are the forms of disks that were thought to be characteristic. Just enough space was left between the notches to cause a silence in the sound of the fork. The spaces were always of the same number of degrees in a given disk. Notches were cut in the circumferences of the disks as follows:

1. Two notches, each of 150 degrees. One accented.
2. One notch, 200 degrees, and one, 100 degrees.
3. One notch, 200 degrees—accented—and one, 100 degrees.
4. Three notches, each of 100 degrees.
5. Three notches, each of 66 degrees. The spaces of 40 degrees.
6. Three notches, each of 100 degrees. One accented.
7. One notch, 120 degrees, and two, each of 60 degrees.
8. One notch, 120 degrees—accented—and two, each of 60.
9. Four notches, each of 60 degrees. Strong accent upon the first, and weak upon the third.

The question was to determine where the rhythmical groups began, with the long or the short sounds. As the pause between the successive sounds was the same length, it is a matter of interest to determine what effect the rhythmical group had upon the pause coming after the sound with which the group closed.

The results of the experiments with the different series of sounds produced by the disks, as described above, are given in the following table. A long sound is indicated by a capital letter, and a short one by a small letter. In disk 9 the accented sounds, which were of different intensities, are represented, the stronger by a bold faced A, and the weaker by a bold-faced B. An accented sound is given in full-faced type. In turning the disk, the operator sought to keep a uniform speed at a moderate rate—about one turn to the second:

	2-Groups.			3-Groups.				4-Groups.	
	1	2	3	4	5	6	7	8	9
	A A	A a	A a	A A A	A A A	A A A	A a a	A a a	A A B A
1		a A	a A-	A A A A A A A	A A A	A A A-	a a A	a a A	
3	A A A A	A a A a		A A A A A A A		A A A A	A a a a a A	a a A	A A B A
4		a A	a A	A A A A A A A	A A A		a a A		B A A A
7			a A	A A A A	A A A		a a A	a a A	
10		a A		A A A A	A A A		a a A		B A A A
11	A A A A A A	A a	A a	A A A A A A A	A A A	A A A	A a a	A a a	A A B A
15	A A		a A	A A A	A A -A		a a A	a a A	A B A A
17	A A	a A a A a A	a A	A A A A A A A	A A A			a a A	
18		a A		No group			a a A		
27		a A	a A	A A A			a a A		
28	A A	a A	a A	A A A A	A A A	A A A	a a A	a a A	A A B A
groups after 4.	A A	a A or a A	a A	4-group or 3-group	A A A		a a A	a a A	A A B A

Several facts are to be observed in this table. First, a series of sounds of uniform length and intensity may be grouped by two, three or four. With disk No. 4, while the most common form of grouping was by three or four, by turning very slowly it was possible to group by two, or by turning faster to group by six or eight. With No. 1 it was easy to group by two or four by turning slower or faster. When disk No. 9 was turned at a slow rate, the sounds were grouped by two, at a faster rate by four, and at a still faster rate the 4-groups were grouped by two or by four.

Second, a more intense sound occurring regularly, imposes a grouping according to the number of sounds between the accents. The accented sound comes first in the 2 and 3-groups, and in the 4-group the first and third receive accents. The first is more strongly accented than the third.

Third, a longer sound occurring regularly in the series, imposes a grouping according to the number of sounds between the longer ones. The long sound, as a rule, is the last in the group, and is frequently accented. It was possible for most subjects to change the place of the long sound to the first of the group, but with the exception of subject 11, it was difficult to keep it at the beginning of the group. Most subjects remarked upon the long interval or pause which seemed to follow the long sound, and for this reason it was found difficult to make the close of the group come at any other place. When the attempt was made to begin the group with the long sound, the preceding group would not seem to separate from the following; the two would run together and become indistinguishable. In the telephone experiments, when a subject attempted to suggest a 3-group, which was accented upon the third by counting one, two, *three*, emphasizing three, it required the closest attention to make the group close with three, for the emphatic three would begin the group thus: *Three*, one, two, etc.

Although it was impossible to control the rate, faster rates than common caused these groups to group by two or four.

The accented long sound frequently appeared more prolonged than the unaccented sound of the same length; the accent had the effect both to increase the length of sound and of the interval which followed.

When the short sound in disk No. 2 and the last short sound in disk No. 7 were accented, the accented sound always came first and the long sound last. It was more difficult with this arrangement to place the long sound first and the accented last, than before.

The results of this experiment confirm in part the results of previous experiments concerning the nature of rhythmical groups. First, the accented sounds occupy the first place in the group. Second, the weaker accent comes upon the third sound in the 4-group. Fast rates with accented groups caused them to fall into higher groups, first of two, and then of three or four.

We come now to the consideration of the nature of the rhythmical group. The general principle is this: In a series of auditory impressions, any regularly recurrent impression which is different from the rest, subordinates the other impressions to it in such a way that they fall together in groups. If the recurrent difference is one of intensity, the strongest impression comes first in the group and the weaker ones after. If the recurrent difference is one of duration, the longest impression comes last. These rules of course hold good only within the limits spoken of above. When the impressions are uniform in length and intensity, the mind

enforces a grouping by giving fictitious values to the impressions, generally with respect to intensity, but sometimes with respect to duration. At the rate .795 sec., the mind intensifies every other sound, so that the series is grouped by two. The second sound in the group is subordinated to the first. At the rate of .460 sec., the mind finds it easy to group a series of auditory impressions by three, by intensifying the first greatly and the second slightly, so that the second is subordinated to the first and the third to the second. More than three degrees of intensity do not appear together in the order of their intensities in a series. In grouping by four, which takes place generally at the rate .307 sec., the mind accents the first strongly and the third slightly. The second and fourth impressions are generally of the same intensity. If there is any difference in intensity, the second is stronger than the fourth, but it is always less than the third or the first. It would appear from this that the 4-group is compounded of two 2-groups, or it may perhaps arise, as Hauptmann says in his "*Natur der Harmonik und Rythmik*," from a combination of two 3-groups. However this may be, the 4-group does appear as a harmonious and organic unity in itself. Given, then, a series of impressions which is made up of three or four intensities recurring as a sequence of fours, the mind divides the series into rhythmical groups, whatever may be the arrangement of the intensities in the sequence of four, so that the impressions are subordinated to one another as nearly as possible from the beginning to the end. The effort is always made to subordinate the last impressions to the first. The same holds good for series which are made up of sequences of two or three. In a sequence of twos, only two impressions can recur; the stronger is always first in the group. In a sequence of threes, the groups may contain two or three different intensities, but the mind always divides the series in such a way that either the strongest comes first or the weakest last.

When the series is composed of impressions different in duration (the longer impression twice the length of the shorter), recurring in a sequence of twos, the mind groups the series by two, placing the longer impression last, and at the same time gives to it frequently a greater intensity. When the series is composed of a sequence of threes, one long and two short, the mind groups the series by three, placing the longer sound last, and at the same time giving to it also frequently a greater intensity. The order of subordination is here reversed. The more important element in the group comes last. For this fact we can offer no explanation upon purely psychological grounds. The fact, however, is interesting for its connection with poetry. Although, as we have seen,

English poetry in its early history contained feet accented upon the first syllable, the most common foot in modern poetry is accented upon the last syllable. What formerly was the beginning of the foot is now the end. In the experimental study with long and short sounds—these correspond to syllables—all the subjects found great difficulty in not making a pause after the long sound, which compelled them to begin the group with the short sound. It was impossible to avoid this pause or to make another after the short sound equal to it, although the interval in every case was the same. Upon this basis and other facts mentioned above, we are able to base our answer to the question whether there is a foot-division in English poetry. Although the long and short syllables do not stand in the absolute relation of two to one, yet the syllables do differ in length and in intensity of accent, and for that reason they tend to fall together in groups. The accented syllables, like the accented sound, will seem to be longer than the unaccented, and in uttering them the speaker will prolong them still farther. Series of syllables, then, which are arranged with reference to the regular recurrence of the accented syllables will fall into groups, and since the accented syllables are longer than the unaccented, a pause will be felt after the long syllable. To use the Latin terminology, the most natural foot must be either iambic or anapaestic. This, however, seems to be due largely to modern ways of utterance. In order for a word to be intelligible, it must be distinctly and carefully enunciated. In the early history of poetry, it was always recited in highly emotional states; words were not articulated, they were shouted. The line of poetry was little more than a series of strong and weak sounds, which, we can argue upon the basis of our experiments, would be grouped with the strongest first and the weakest last. In the change from the merely emotional shout to articulated utterance, the character of the foot changed from one which was accented upon the first to one which was accented upon the last.

We come now to the subject of muscular movements and their relation to rhythms. Most subjects felt themselves impelled by an irresistible force to make muscular movements of some sort accompanying the rhythms. If they attempted to restrain these movements in one muscle, they were very likely to appear somewhere else. Wundt¹ says that the intensive clang change has its nearest pattern in the sensation of motion. A corresponding rhythmical series of motions associates itself in dancing, marching and beating

¹Physiologische Psychologie, Vol. II. p. 73.

time, with almost irresistible force to the changes of strength in the clang.

The most common forms of muscular movement were beating time with the foot, nodding the head, or swaying the body. Subjects 3, 10 and 17 accompanied the rhythmical grouping by muscular contraction of the diaphragm and chest, and it was exceedingly difficult to restrain them. Other subjects counted inaudibly or made the proper muscular adjustments for counting. Slight or nascent muscular contractions were felt in the root of the tongue or larynx. Most subjects were unconscious of their muscular movements until their attention was called to them, and subject 15 never became conscious of the rhythmical contractions in the eyelids. When he was asked to restrain all muscular movements, he found great difficulty in maintaining the rhythmical grouping. This fact was remarked upon by other subjects also. The reader is referred to the records of subjects 2, 3, 7, 9, 10, 11, 12, 13 and 15.

Of the same nature as muscular movements, are the associations of various objects. Most subjects visualized the pendulum and clocks, large and small. Several referred to the conical pendulum, striking three or four times in a swing, and others to revolving wheels. Subject 14 visualized a series of dots, and subject 11 at one time an undulating line, and at another an ellipse with four dots placed upon either side. Subject 15 made a color association.

The question we have to decide upon is, are these muscular movements and associations the result or the conditions of the rhythmical grouping? With Ribot we accept without hesitation the latter.

Ribot states this principle, "Every intellectual state is accompanied by physical manifestations.¹ Thought is not—as many, from tradition, still admit—an event taking place in a purely supersensual, ethereal, inaccessible world. We shall repeat with Setchenoff, 'No thought without expression,' that is, thought is a word or an act in a nascent state, that is to say, a commencement of muscular activity." Each impression as it enters into consciousness tends to find expression in a muscular movement, but the intensive changes in the series of impressions produce corresponding changes in the intensity of the sensations, which must find expression in different degrees of muscular activity. In order to express these different degrees of sensation, the muscular movements must rise above the merely nascent state in which they ordinarily occur, and manifest themselves in visible muscular

¹The Psychology of Attention.

movements. The tendency for sensation to find expression in visible muscular movements is stronger with children and primitive peoples than it is with highly civilized and especially well-trained persons. With the latter class, muscular movements accompanying attention do not so easily rise above the nascent state.

Exact coördinations of sounds with respect to intensity are difficult, for the reason that great degrees of difference must be allowed, that two sounds may be discriminated. This is proved by the fact that higher groupings of 4-groups are difficult, for the reason that the differences in the accents cannot be kept clear. Groups of six and eight are difficult because the different degrees of intensity required cannot be discriminated. Pitch changes are much more easily discriminated, and more exact coördinations are possible. They find their expression in different degrees of tension in the muscles of the larynx. With fast rates the intensive changes recur more rapidly, and hence call for more rapid muscular movements. On this account the faster rates were found exhilarating and animating, and the slower rates drowsy and soporific.

For the same reason, subject 12 found that a change from a 3-group to a 4-group gave rise to a feeling of a slower pace. Within certain limits the mind can easily accommodate itself to changes of rate. A rate which seemed unpleasantly slow or fast at first, became in time pleasant. If the rate is slow, the grouping which is first suggested is accompanied by a feeling of suspense—subject 11 said the group broke off with a “dead end”—but if it is fast there is a straining after a longer group, or perhaps a hurried, animating feeling which becomes monotonous. If a subject maintained a 2-group, for instance, with a rate which was naturally too fast for grouping by two, it became exceedingly monotonous in a short time.

If the length of the group corresponds to the normal wave of attention, the grouping gives rise to a feeling of satisfaction and repose. There is probably not an absolute psychic constant in attention which admits of no variations without feelings of dissatisfaction, but within limits a constant is easily established, which, if changed gradually, accommodates itself to a longer or a shorter interval. A sudden change, however, cannot take place without difficulty. For this reason, if the grouping enforced by an irregular recurrence of an accented sound change rapidly from one form of grouping to another, it gives rise to an alternation of feelings of suspense and straining which no one fails to perceive. The same phenomenon would arise if the temporal sequence of the impressions were irregular. Either it would be necessary to group now by three and now by four, or by two, that the interval between

the successive accents should be the same, or there would be an alternation of feelings of suspense and of straining to maintain a grouping by three or any other number. When the rate was changing rapidly, as it did just after the chronoscope was started (it required ordinarily about two minutes for the chronoscope to attain its full speed), subject 4, especially, and others remarked upon the disagreeable effect. The accommodation to any form of grouping within certain limits is easy, providing there is a perfect regularity in the sequence. The accents must recur at regular intervals, and the number of intermediate impressions remain the same, or there is no feeling of rhythm. When a slow rate was succeeded by a faster one, it gave rise generally to a disagreeable effect; but in time the subject could accommodate himself to it. Subjects 4, 9 and 15 make observations upon this point. Subjects 2 and 5 were greatly puzzled over a 5-group which was accented on the first and third sounds. They attempted to group by two and by three alternately, which gave rise to a very disagreeable feeling. When, however, they grasped the regular sequence of five, the disagreeable feeling passed away.

When a longer interval was introduced into the series, the impressions coming between the long intervals fell together into a group, but they did not form an organic unity. There was no pleasure in such a rhythm. Something seemed to be looked for in this longer interval which was wanting. When the rate was made very fast, the impressions between the long interval seemed to fuse together into a single impression and then to group by two or four.

This general principle may be stated: *The conception of a rhythm demands a perfectly regular sequence of impressions within the limits of about 1.0 sec. and 0.1 sec. A member of the sequence may contain one or more simple impressions. If there are a number of impressions, they may stand in any order of arrangement, or even in a state of confusion, but each member of the sequence must be exactly the same in the arrangement of its elements.*

The application of this principle to poetry demands that the accents in a line shall recur at regular intervals; it requires also that the successive feet in a line shall be of precisely the same character. The introduction of a 3-syllable foot into an iambic verse is allowable on this condition only, that the 3-syllable foot can be read in the same time of the two, so that there shall be no disturbance in the temporal sequence of the accents. This foot affects the rhythm in so far only as it changes the character of one member of the sequence. This is an actual disturbance to the rhythm, but it is allowable for the purpose of emphasis. The frequent

use of such a foot would be fatal. Poe's principle that the regular foot must continue long enough in the line, and be sufficiently prominent in the verse to thoroughly establish itself, is perfectly valid. In a musical rhythm, however, the measures may vary with certain restrictions in the arrangements of their elements. But it is just this variation which constitutes the melody to a certain extent. The rhythm is varied for purposes of melody, but it is, nevertheless, a disturbance to the rhythmical flow in so far that it changes the measure. The melody is a new and higher unifying agency, which corresponds in a way to the use of rhymes in poetry. The temporal sequence of the accents is always preserved.

It remains now to make my acknowledgment to those who have assisted in the work.

To President G. Stanley Hall I am indebted not only for the subject itself, but for a large amount of material which he had already collected upon it; also for suggestions as regards the direction of the experiment and references to literature.

To Dr. E. C. Sanford, the director of the laboratory, is due much of the credit for the success of the work. But for his skill in devising and constructing apparatus, the work could not have been carried on. His suggestions as regards methods for making the experiment were no less valuable than his assistance in devising apparatus. To all others who so generously gave up their time to sit through long and tedious experiments, I acknowledge my indebtedness. Space forbids me making special reference to each one.

CLARK UNIVERSITY,
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MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY
OF CORNELL UNIVERSITY.

Communicated by E. B. TITCHENER.

I.

"MEDIATE" ASSOCIATION.

By H. C. HOWE, A. B.

Our object was the investigation of the "mediate" association, in Scripture's sense (*Ueber den associativen Verlauf der Vorstellungen*: *Phil. Studien*, VII. pp. 60 ff. Espec. pp. 81 ff.). The literature of the problem answers the question as to the occurrence of this form of association both negatively and affirmatively, in terms of experiment and criticism (Wundt: *Bemerkungen zur Assoziationslehre*: *Phil. Studien*, VII. pp. 329 ff. Espec. pp. 360, 361. Münsterberg: *Beiträge zur experimentellen Psychologie*, Heft IV. pp. 1 ff. Espec. p. 9. Titchener: *Mind*, N. S., I. pp. 226, 227; II. p. 235). Our experiments were carried out with six subjects: Messrs. Hinman (*H.*), Irons (*I.*), Knox (*K.*), Leighton (*L.*), Smyser (*S.*), and Watanabe (*W.*); in the months January to mid-March, and April to June, 1893. Two days' experimentation was devoted to practice in each case. The experiments were of two kinds.

(1) Auditory experiments.

The subject was seated in a dark room. To him were read regularly and distinctly (*a*) a series of six words, each word having a nonsense-syllable attached to it; (*b*) a second series, with the same nonsense-syllables; (*c*) the first series again, the reader waiting after each word for the subject to state his first association. All pauses were controlled and kept as constant as possible. The lists at first contained monosyllabic English words. For these were substituted, later, pairs, of which the one term consisted of English, the other of Latin, French or German words. In the course of 557 experiments there occurred eight apparent cases of "mediate" association, distributed as follows:

Observer.	Expts.	Cases.
<i>H.</i>	123	3
<i>I.</i>	36	0
<i>K.</i>	160	2
<i>L.</i>	26	0
<i>S.</i>	154	2
<i>W.</i>	58	1

These were, however, variously explicable. (a) In four cases the observer was able to state that he had directly associated, mentally, before association was called for by the experimenter. (b) Twice the word recalled was the first word in a series. (c) Once the word recalled was the most "striking" or unfamiliar word in a series. [It was also a first word; so that this case has been already counted, under (b).] (d) Once the observer remembered the whole second series of words, in their order. The first series was not called over in order as first given, but the place of the recalled word had by chance escaped change. (e) One case remains unexplained.

Remarks.—This was the first part of our work. The subjects, at the outset of their practice, were unduly impressed with the importance of the nonsense-syllable. They associated with the word called either the syllable, an extraneous idea, or nothing. The extraneous association was ruled out for experimentation, the subject being required to make his association within the series. He then did give words of the corresponding series: but, if the correct word, always by simple memory. Throughout, too large a share of the attention appeared to be directed upon the nonsense-syllable. Error thus arising is very difficult to eliminate. We guarded against it so far as possible, but put no great faith in this whole method. Again, in the case of all of our subjects, the visual memory was stronger than the auditory. If word and link were visually presented, a second view of the word recalled a visual image of the link in its former position; but during the presentation of an auditory series, each word seemed to blur the memory-image of preceding words.

(2) Visual experiments.

Scripture's apparatus was reproduced (*Phil. Studien*, VII. p. 36), and his procedure exactly followed, except that for the 4" limit a 2" limit was substituted (p. 54). Our practice-experiments suggested this as an improvement. We found a source of error in the shortness of Scripture's series: the subject is able to memorize. Moreover, if two word-series were employed, we discovered that the subject was apt to associate together words of the same series. These difficulties were overcome by the employment of a series of six pictures and a word-series of equal length. That the observer should have no trouble in naming his associations, we arranged the experiment in such a way that a picture series was always shown in the third place. As links we used variously colored rings of various sizes, and small variously colored figures (diamond, triangle, fleur-de-lis). The aim was, of course, to present an object which should be perceived, but not apperceived; which should neither be conspicuous nor very readily named. Finally, since the subjects tended still to associate links rather than words, they were urged to concentrate their attention more strongly upon the pictures and upon the words. More could not be said by way of direction, without their being informed of the precise nature of the problem. Of these experiments we performed 961. There occurred 72 apparent cases of "mediate" association, distributed as follows: (See table on opposite page.)

These were, again, variously explicable. (a) In 21 cases the observer was able to state that he had directly associated, mentally, before association was called for by the experimenter. (b) In nine cases, both word and link were given as association; but the word was given first. [Six of these come also under (h), one under (f), and one under (g).] (c) In twenty cases a picture called up the picture following it, independently of the word. [Seven, also, under (h), seven under (i), four under (j) and (g), one under (g) and (h).] (d) In five cases association took

place through an extraneous connecting idea. [One, also, under (f).] (e) Once the word recalled was the most "striking" word in a series. [Also under (f).] (f) Seven times it was the first word in a series. [Four cases also under (i), one under (b), one under (d), one under (e).] (g) Eight times it was the last word in a series. [Once also under (b), once under (i), four times under (i) and (c), once under (c) and (h).]

Observer.	Expts.	Cases.
H.	224	15
I.	71	1
K.	238	14
L.	33	3
S.	196	9
W.	199	30

(h) Sometimes the whole list was memorized, links and words; but, in naming the association, a link or a word would slip the observer's memory. Seventeen cases. [Seven also under (c), six under (b), one under (c) and (g).] (i) Or, in naming the association, both word and link were given, but the word first. Seventeen cases. [Eight also under (c), four under (f), one under (g), one under (b) and (f), three under (c) and (g).] (k) The word was mentally imaged before the picture was shown. [Seemed to cross other rules in two cases. These could not be arranged under (e).] (l) The word was remembered as being at a particular point of the series, though the remaining words were forgotten. One case. (m) Three cases remain unexplained. One of them occurred in a series, one word of which had been inadvertently omitted, so that only five were given.

Remarks.—The observer *W.* is possessed of an exceedingly accurate pictorial memory. His thirty apparent cases were those which threw most light on the various origins of the whole number.

Results.—We have, in 557 auditory experiments, found one unexplained case of apparent "mediate" association; in 961 visual experiments, two or three such cases. These instances may be referred either to chance, to deficient memory on the part of the observer, or to deficient analysis on the part of the experimenter. So far as our experiments take us, we may conclude, against Scripture, that "mediate" association does not occur. On the whole, the appearance of the phenomenon is a function of attention. We do not consider that the method employed is wholly satisfactory. Even in the visual experiments there are operative many conditions which lead to error in result and which are hardly eliminable. "Free" associations alone can, we think, form the basis of a certain experimental conclusion in any similar department of associative investigation.

[*Note to the foregoing.*—Mr. Howe's experiments have suggested to me a source of error in the determination of the time-value of the verbal associative reaction. This source of error consists in the fact that the "associated" word may be, and will often tend to be, a word which is not associated to, but already apperceptively combined with the stimulus. When Wundt "associated" *Wind to Sturm*, in 341a (*Phys. Psychol.*, II. 3d Ed. p. 322), he was obviously completing the apperceptive combination *Sturmwind* (cf. the *Kirchthurn* of p. 385). This error, it would seem, can only be circumstantially eliminated.—E. B. TITCHENER.]

II.

"SENSIBILITIES" AND "MUSCULAR" REACTIONS.

By A. R. Hill, A. B., and E. Watanabe, Ph. D.

In the course of his investigation *Ueber den Hautsinn* (Archiv für Anatomie und Physiologie, 1882, p. 311), Dr. M. Dessoir writes: "Ich möchte nun eigentlich mittheilen, ob von mir und meinen Versuchspersonen sensoriell oder muskulär reagirt worden ist. Ich kann das leider nicht, weil keiner der Herren [8] bei Benutzung des Fingervortantes [p. 308] den Unterschied zu stande brachte." With the ordinary key ("an dem gewöhnlichen Tastapparat"), on the other hand, the two reaction-values were obtained.

It is a well-known fact that not every person examined in a psychological laboratory proves to be a capable reaction-subject. Some cannot react with any constancy; some never reach the quickness of the muscular form; some, while reacting constantly, do not conform to the Lange type. (See, e. g., *Phil. Studies*, VIII. 138, 403 ff.) Into the reasons for these individual differences we do not wish here to enter. We have rather proposed to ourselves for answer the special question: Is the sensorial-muscular difference entirely conditioned by the technique of the ordinary reaction-experiment? Or can that difference be obtained from practised experimentees under other conditions: with, e. g., the Dessoir finger- or the Cattell key?

Our experiments were carried out in the months January to mid-March and April to June, 1888. We used the Hipp chronoscope (new pattern) in Dessoir's way (lower magnet only; pp. 306, 307); and tested the instrument by Wundt's new fall-hammer (*Phil. Studies*, VIII. 145 ff.). The apparatus was arranged on the Leipzig plan, in two rooms. Dr. Scripture, of Yale University, kindly allowed his mechanician to construct for us a Dessoir key. The stimulus was, throughout, the sound caused by the dropping of an electric hammer upon its anvil. Unfortunately, our fall-hammer was injured at the end of March, and from that time on we were compelled to rely exclusively upon the chronoscope. The previous test experiments had shown that, while the latter instrument was generally reliable, the mean variation of its control-times was apt to be somewhat greater than that of the times of the old pattern clock. This result, which is confirmed by that of some preliminary tests carried out in the Leipzig laboratory, points to the need of a thorough chronographic examination of the new chronoscope, in its four possible modes of functioning. This examination we hope to be able shortly to set on foot. Our present figures, therefore, possess: (1) no absolute (chronographic) value, except in so far as they are borne out by the results of other published investigations; and (2) even a relative value only in the rough. They are, however, sufficiently accurate for the answering of the question under investigation.

Nine persons took part in the experimentation: Miss Fanning (F.), Miss Hannum (Ha.), Messrs. Hill (Hi.), Irons (I.), Knox (K.), Major (M.), Schlapp (S.), Titchener (T.) and Watanabe (W.). Practice consisted in the taking, on four different days, of four series of twenty experiments of either type; the experimentee being required to direct the attention upon movement or stimulus as the case might be.¹ The reaction-movement with the ordinary key consisted in the snapping-off of the index or second finger

(according to the constant preference of the reagent) of the right hand from the button to the table; not in the raising of the whole hand, or of hand and arm, from the key.² With the Dessoir key, it was the opening of the closed thumb and index finger of the same hand; with the Cattell lip-key (*Phil. Studien*, III. 312), the opening of the closed lips.

Our control-experiments, January-March, gave the following result: determinations recorded, each of ten experiments, twenty-two; average chronoscope reading, 186.1σ; average mean variation, 1.9σ; mean variation of average reading, 6.06σ; mean variation of average mean variation, 0.8σ. All numbers in the following tables have been reduced to this average reading, except those whose authority is the chronoscope alone; these are distinguished by a prefixed *. The first column gives the name of the observer; the second the reaction-time; the third, the mean variation of the separate times; the fourth, the number of experiments; the fifth, the number of experimental series; the sixth, the mean variation of the average times obtained from the separate series:

TABLE I.
Muscular Reaction to Sound.

1	2	3	4	5	6
S.	123.4	12.5	29	3	3.4
F.	126.2	7.6	37	3	2.8
Ha.	111.7	8.2	66	5	6.3
I.	125.0	13.0	15	1	—
W.	123.7	16.6	27	2	3.3
H.	120.2	15.4	20	2	3.1

The seventh column in Table II. gives the time-difference between the sensorial and muscular forms.

¹We do not understand Dessoir's remarks in this connection, p. 312.

²This form of movement, proposed by Professor Wundt, was also employed in the reaction-investigation of *Phil. Studien*, VIII. 138 ff.

TABLE II.
Sensorial Reaction to Sound.

1	2	3	4	5	6	7
<i>S.</i>	206.5	18.7	28	2	30.0	83.1
<i>F.</i>	217.0	28.0	36	3	12.6	80.8
<i>Ha.</i>	206.1	25.1	32	4	10.9	94.4
<i>I.</i>	227.3	38.7	15	1	—	102.3

No experiments were recorded for *W.* and *Hi.*
The two following tables show the results obtained with the Cattell lip-key:

TABLE III.
Muscular Reaction to Sound. Cattell Key.

1	2	3	4	5	6
<i>S.</i>	128.0	10.0	1	15	—
<i>F.</i>	129.9	15.4	3	33	1.5
<i>Ha.</i>	118.7	18.0	2	30	11.2
<i>I.</i>	123.9	15.2	4	48	8.4
<i>W.</i>	158.0	17.0	2	21	2.0
<i>Hi.</i>	122.5	11.4	2	23	13.5
<i>T.</i>	137.3	12.1	2	20	16.5
(<i>K.</i>	122.8	20.6	4	40	19.6)

TABLE IV.
Sensorial Reaction to Sound. Cattell Key.

1	2	3	4	5	6	7
<i>F.</i>	218.3	20.6	6	70	8.8	88.4
<i>Ha.</i>	243.3	34.7	3	30	2.9	124.6
(<i>I.</i>	202.3	37.1	3	34	16.2	78.4)
<i>W.</i>	250.0	13.8	3	21	14.9	92.0
<i>T.</i>	254.4	22.5	2	20	2.6	117.1
(<i>K.</i>	131.0	25.2	4	35	11.5)	

Remarks.—III. *F.* gave many premature reactions. The "sensorial" time of *S.* tended to be central (Martius), but both the extreme forms occurred, and the mean variation of the result was very large. IV. *I.* gave typical central times. The reagent himself stated that he was unable to fulfil the conditions of the sensorial action. The reagent *K.* made no distinction between the forms from the outset, and was not educable. His type is the muscular, and his large mean variations prove his unsuitability to function as reaction-subject. III. *W.* seems to show a slight leaning towards centrality. It is curious to compare I. *I.* with III. *I.*, but the difference is so slight that the apparatus may be to blame for it. *Hi.* gave a central (2040) for a sensorial time, admitting, like his inability to concentrate his attention on the sense-impression.—It is obvious that our experiments are not directly comparable with those of Cattell (*Phil. Studien*, II. 305 ff.). The two final tables show the results obtained with a Dessoir trigger-key.

TABLE V.

Muscular Reaction to Sound. Dessoir Key.

1	2	3	4	5	6
<i>F.</i>	* 157.2	15.8	8	86	11.0
<i>I.</i>	* 152.8	14.8	3	24	5.6
<i>W.</i>	* 161.0	16.2	3	28	6.6
<i>Hi.</i>	* 155.0	13.0	1	9	—
(<i>K.</i>	* 136.1	21.6	9	91	24.4)
(<i>M.</i>	* 117.6	12.2	5	50	13.4)

TABLE VI.

Sensorial Reaction to Sound. Dessoir Key.

1	2	3	4	5	6	7
<i>F.</i>	* 313.0	23.2	8	80	28.1	155.8
<i>I.</i>	* 306.0	28.3	3	28	16.7	153.2
<i>W.</i>	* 300.0	23.6	3	27	30.6	139.0
(<i>K.</i>	* 123.3	14.7	9	106	9.6)	
(<i>M.</i>	* 113.7	13.7	4	44	1.5)	

Remarks.—Three sensorial series taken from *I.* gave valueless results, the subject finding a difficulty in handling the key. One series showed a central form of reaction, 182.7 σ ; mean variation, 7.5 σ . This has not been employed in the construction of Table VI. *Hi.* again gave a central (*203 σ ; mean variation, 21.8 σ) for a sensorial time, and again declared his consciousness of the nature of the result. He had, throughout, but little practice. The results obtained from *K.* are similar to those of Tables III., IV. Practice brought about no alteration in them. Those of *M.* are parallel with these. *K.* had not had practice with the ordinary reaction-key. *M.* had used neither this nor the lip-key.

We would conclude from our experimentation:

(1) That the new pattern chronoscope requires a thorough testing before its times can be accepted as of absolute value.

(2) That the sensorial-muscular difference is not, as Dessoir thinks, a matter of the form of the reaction instrument. We have obtained the difference with three reaction-keys, involving diversity of muscular action. It averages, in these cases, 90 σ , 105.5 σ , and *149.3 σ .

(3) That we may confirm the view that not every person is able to function as reaction-subject. Rather is there required for the work a special kind of mental disposition or *Anlage*. If the volitional temperament is unfavorable, practice will have no effect in determining the two types of reaction time; if favorable, Lange's distinction holds even of the first practice-experiments.

EXPERIMENTAL STUDY OF SOME OF THE CONDITIONS OF MENTAL ACTIVITY.

By JAMES A. DEANSTADT.

duction.—The experimental work described in this paper was done at Clark University, at intervals during the two academic years of 1891-'92 and 1892-'93. The first part gives an account of experiments upon the daily variations in the rate of certain mental processes, with a view to determining whether there is a daily rhythm of mental activity or not. Under the head of constant variations, the results have been grouped to exhibit certain features of mental periodicity. Accidental causes of variation have, as far as possible, been avoided, though such as barometric changes, the experiments of Dr. W. P. Lombard and pathological conditions have shown to be important, have necessarily occurred. The relative variation of a number of different processes has also been studied. The great instability of the nervous system and the various causes of variation prevent the results from being as accurate as could be desired, but some general facts may be determined with a fair degree of probability.

The second part, in which more satisfactory experimental conditions were possible, consists of experiments upon physiological fatigue and nervous activity which will here be especially considered.

The work has been done chiefly under the direction of Dr. E. C. Hall, but I wish also to acknowledge my great indebtedness to Dr. Hall and Dr. W. P. Lombard and those who have given me time to the experiments.

Methods.—Two classes of experiments have been selected as tests, the first being the repetition of old, the other the formation of new combinations. To the first belong reading, adding and multiplying numbers, and the classification of words; to the second, the learning of nonsense syllable and number series and the sorting of cards. For the number experiments use was made of the columns in Peirce's logarithmic tables. The vertical columns containing the fifth and sixth places of the logarithms are sufficiently irregular in order in the first fifty pages of the book, if four or five pages be selected, to serve as material for the experiments. The tables offer a further advantage in that the horizontal lines dividing the numbers into groups of ten make counting of the amount done easy. The two end figures of each logarithm were multiplied or added, as many of these operations as possible made in a minute. The numbers were read either by ones or by threes with the abbreviated form of expression—thus, "seven-fifty-three," instead of seven hundred and fifty-three. Reading by threes was substituted for

reading by ones, because a certain order would often recur, while in reading by threes, the figures were perceived as a combination and no repetition noticed. In the few experiments with the classification of words, uniformly printed newspaper articles were used. The classification was based upon use. A test of the rate of voluntary movement was also employed in a few experiments. The plan was to simplify the ordinary writing movement, as far as possible, so that there should be no considerable qualitative change, and at the same time to make the amount of work done easy to count. Ten parallel lines were drawn across ordinary ruled paper, thus making ten squares on any horizontal line. These squares were filled with five crosses or ten strokes. The number made in a minute was taken as a test. This number could be seen at a glance. A convenient test of the precision of movement may also be found by using this ruled paper. With a fountain pen or a needle point make a dot or a needle prick, and then attempt to put the pen or needle point in the same place. The number of errors will be inversely proportional to the precision. Of course a given movement and rhythm must be chosen. If five dots are made in each square, counting of the errors will be easy.

An inexpensive but accurate timing apparatus was made for use in these experiments. A pointer on the second's axis of a clock dipped into a drop of mercury and thus made the circuit of an electric bell. This gave minute signals. For fractions of a minute, stars with the required number of points could be put upon the second's axis. By putting stars with the required number of points upon the minute axis, signals can be had for any number of minutes. Here large errors would probably come in if the closing of the current was made to depend upon the slow moving minute star. The circuit is, therefore, so arranged that both the minutes and the seconds points must be in their respective drops of mercury simultaneously before the circuit is closed. This makes the signal depend upon the movement of the second hand; and since there is only one point to consider, and not the distance between two successive points, the errors are very small.

The nonsense syllables were learned by the method used by Ebbinghaus. A series usually contained ten syllables. The number-series consisted of thirty digits, learned by threes. The series were read over in a given rhythm and the attempt made to repeat them. As soon as there was hesitation over a syllable, the reading was begun with it and continued to the end, and then an attempt was again made to repeat the series. The series was considered learned at the first perfect repetition. The card-sorting memory experiment consists of sorting a pack of eighty cards into ten different piles. Each pile is to contain eight cards bearing the same word or picture. In subsequent experiments different arrangements of the piles were used, so that as a memory experiment it consists essentially, like the nonsense syllable series or the memory span test, of learning different permutations of the same symbols. Care was taken that there should be no possibility of association or grouping of the words or pictures. As a memory experiment, it is especially valuable for the study of the interference of associations. The time of all the memory experiments was taken by a stop watch.

Only such material was used in the experiments as was of uniform quality and of practically unlimited amount. The comparative uses and value of the different tests is to be judged by the results. As regards errors, the effort was made to correct

them and thus include their value in the total time of the operation. In the experiments upon the daily variations in rate, the general plan was to make experiments every two hours throughout the day. The effect of practice and fatigue for the tests themselves was reduced by preliminary practice to a comparatively small quantity. The influence of the interference of associations in the memory experiments is more troublesome; but at the end of two hours its effect is small, so that great changes must be due to other causes. The results of a number of days were then averaged, giving the constant daily variations. The subject of the experiments was asked to be as regular as possible in his habits, and to follow an average routine of work and rest. This is, of course, an essential requirement. In the majority of cases it was quite carefully complied with. Since large changes in the tests themselves have been eliminated, any such that occur will be due to general changes in the nervous system and the circulation.

Constant Daily Variations.—Table I. aims to give a general view of the changes in rate throughout the day. The figures under morning, afternoon, and evening represent seconds; and for E. C. S. give the average time required for learning three series of nonsense syllables of ten each at those times of day. The rest give the average time required for sorting two packs of cards in the way described. Two similar packs were used by W. O. K., and by A. F. for the first average. In all the other experiments, two different

TABLE I.

	Morning.	Afternoon.	Evening.	No. of Days.
E. C. S.	478.3	544.	573.3	10
M. N.	188.7	197.	214.	11
J. A. B.	211.94	225.66	249.7	7
P. E., Av.	± 1.99	± 2.07	± 2.32	
L. N. B.	225.4	224.3	269.6	11
A. F.	$\left\{ \begin{array}{l} 269.4 \\ 223.49 \end{array} \right.$	$\left\{ \begin{array}{l} 264.4 \\ 216.85 \end{array} \right.$	$\left\{ \begin{array}{l} 288.8 \\ 244.3 \end{array} \right.$	$\left\{ \begin{array}{l} 8 \\ 8 \end{array} \right.$
W. O. K.	288.4	300.	282.2	6
P. E., Av.	± 2.09	± 2.48	± 3.19	
E. T.	200.34	196.45	193.4	5

packs were used. Differences in manipulation will make it impossible to compare the rates of all the different subjects, but this was constant for any given one. The right hand column gives the number of days from which the average was made. Usually there were four experiments in the morning and three in the afternoon and evening each. The morning average of E. C. S., for example, is made up from forty experiments—or one hundred and twenty series. A low record indicates rapid rate, and a high record the contrary.

The records of three other subjects averaging ten days each, but covering only the morning and afternoon, show no decided difference between these two periods.

The averages show that the rate of the first three subjects diminishes throughout the day, while that of the next two is as good or better in the afternoon, though poorer in the evening. The rate of E. T. increases steadily. W. O. K. makes the best records in the evening, but is poorer in the afternoon than in the morning.

Only the probable errors of the records which do not receive special study in Table II. are given. The daily variations in rate are not of a single type such as would be required if a natural, inherited rhythm of activity existed. The daily rhythm is the resultant of a number of nervous and circulatory influences, which will be discussed after a statement of the results has been made.

Table II. gives a more detailed statement of the records which are adequate as regards number, and regularity of experimental conditions for such treatment. The probable errors—as a measure of the closeness with which each subject adheres to his type and of the reliability of the experiment—are greater than they should be since the effect of practice is added to that of accidental variation. Only a rough correction can be made, however, and the general results can be established without such correction. The effect of practice can be seen in the averages of the successive days. For L. N. B. there is an average decrease of about three seconds per day at first, the last four days being nearly the same. A. F., whose preliminary practice was considerable, shows no decrease of time. The records of M. N. diminish about four seconds the first four or five days, after which the averages are about the same. Those of T. L. B. decrease about one second per day. The effect of practice in these cases has evidently little influence upon the daily curve. The experiments of E. T. extend over only five days, but they were made very carefully and are of special interest, since they represent a distinct type. The effect of practice during the experiment can be seen from the averages of the five days, which are 203.67, 202.28, 202, 195.54 and 184 respectively. The great increase of rate on the fifth day is probably not to be attributed so much to practice as to change in health. With the exception of an experiment in which a subject sorted cards for an hour or more continuously, no signs of fatigue for this experiment itself have been observed. A few trials were made to see if a second experiment would give different results from the first, especially when an unusual record was made. The two records usually differed but slightly, if the effect of the interference of association is taken into account. In another experiment with four other subjects, about as many experiments were made in an hour as were here made in a day, but no evidence of fatigue is to be seen, though the average endurance is probably no greater. The general effect of the interference of associations is to make the first of a series of records shorter than the second, while the rest will not differ much from the second. It would not influence the general curve of the day except with respect to the first record in the morning, which would be shorter than it should

be. The fact that with more intense work the interference is slightly greater is a disturbing element, however, which needs to be taken account of. The amount of the changes and the individual peculiarities of the curves show at a glance that they are not due to small and comparatively constant changes in the experiment itself. The experiment of E. O. S. was performed at three different periods, three days being used the first and second time and four the third. The averages of the records for the three periods stand

TABLE II.

A. M.					P. M.						Number of days.
Hours.	7.30	8.30	10.00	12.00	2.00	4.00	6.00	7.00	8.30	10.00	
N. B.	234.2	218.1	225.8	223.6	215.8	231.2	226.	253.	280.8	275.	11
E.	± 2.2	± 1.8	± 2.89	± 1.79	± 1.76	± 3.3	± 2.79	$\pm 2.$	$\pm 4.$	± 4.16	
F.	233.4	229.4	216.8	214.3	213.3	211.9	225.3	244.6	236.3	250.4	8
E.	± 4.32	± 4.21	± 2.8	± 3.8	± 2.29	± 1.33	± 4.3	± 3.8	± 3.68	± 5.8	
Hours.	7.30	8.30	10.00	12.00	1.30	3.00	6.00	7.30	9.30		
M. N.	192.8	185.	183.6	193.4	186.3	197.1	207.6	207.2	222.6		11
P. E.	± 4.4	± 3.24	± 2.7	± 2.55	± 3.5	± 3.5	$\pm 3.$	$\pm 5.$	± 3.7		
E. T.	198.	205.3	204.5	193.54	197.6	199.3	192.45	201.6	185.2		5
P. E.	± 4.8	± 4.6	± 4.7	± 4.48	± 2.4	± 1.5	± 3.48	± 3.3	± 1.8		
Hours.	7.45	9.00	11.00	12.45	2.00	4.00	5.45	7.00	9.00	10.30	
E. C. S.	452.7	484.7	467.9	508.	531.4	507.7	593.	536.3	578.	605.6	10
P. E.	± 12.48	± 19.4	± 13.44	± 16.53	± 25.49	± 14.24	± 20.98	± 14.76	± 20.47	± 15.01	
Hours.		8.00	10.00	12.00	2.00	4.00	6.00				
L. H.		179.7	189.4	187.8	190.7	189.3	192.7				17
P. E.		± 3.6	± 2.1	± 2.56	± 1.84	± 1.33	± 2.63				

as one hundred to eighty-six and seventy-nine respectively, but in each of the sets of days there was an increase rather than a decrease of time, showing fatigue rather than practice. The effect of interference was so slight that it can't be demonstrated between successive series. It is believed that changes in the experiment itself are inadequate to account for the great daily variations. A three days' experiment with card-sorting gives variations corresponding with those for the nonsense syllables. The general daily routine differed a little with the different subjects, but in general the hours of sleep were from 10 to 7; breakfast was eaten immediately after the first, dinner after the 12 o'clock, and supper after the 6 o'clock experiments. With the exception of E. C. S. the subjects were between twenty and thirty years of age; E. C. S. is a little over thirty. All were members of the psychological department except M. N. and E. T., two ladies, one of whom is a student of medicine, the second a teacher. All were in good health during the experiments except E. T., who had not yet recovered from a severe attack of nervous prostration. The constant daily variations of E. T. will be seen to show the typical symptoms of morning tiredness and depression after meals, though the rapid rate at all times shows that there is no lack of power to concentrate.

The accompanying chart will give the general picture of the daily variations at a glance. The first curve shows that breakfast and dinner are stimulating. The middle of the morning and afternoon shows lower rates than the beginning and end of those periods. The high records and consequently low rate after supper is probably due to a habit of relaxation at that time. The subject complained of sleepiness. The better record at 10 P. M. than at 8.30 P. M. may be accidental, but is probably due to the fact that 10 to 12 P. M. were found to be good hours for study in college and had been so used. The subject took tea or coffee morning and night, but evidently not in sufficient quantities to give the characteristic effect of meals, since the records after breakfast are low, while those after supper are high. The curve of A. F. does not show any decided effect of meals. The records after meals seem rather to take their proper place in the general tendency of mental activity to increase or diminish. Coffee or tea was taken at each meal. The first two records show a morning depression. L. N. B. was up, on the average, thirty minutes earlier than A. F. and felt wide awake, while A. F. complained of sleepiness in the morning. From 10 A. M. to 4 P. M. there was a steady increase of mental activity. At 6 and 7 P. M. the records are higher, in spite of recreation and supper. The time from 8 to 10 P. M. was usually devoted to study, and the better record at 8.30 P. M. is probably due to the increase of mental tension. Early morning and evening depression, with an intermediate period of constant activity, is the characteristic of this curve. The curve of M. N. is marked by a decrease in rate during the day, interrupted by the stimulating influence of meals. Tea or coffee was usually taken at meals. The curve of E. C. S. shows a decrease of rate from morning till night. The low rate before dinner and supper is a noticeable feature. After supper there is a marked increase of rate. The effect of breakfast and dinner is not certain, on account of the large probable errors, though the average is higher in both cases. The curve of E. T. differs wholly from the rest. It shows an increase in rate throughout the day interrupted by depression after meals. Tea or coffee was usually taken at meals.

¹ *Accidental Variations.*—At the suggestion of Dr. W. P. Lombard a comparison of all the records with the barometric readings was

FIG. I.

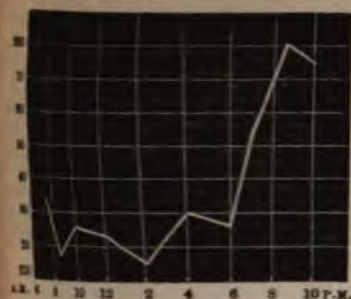


FIG. II.

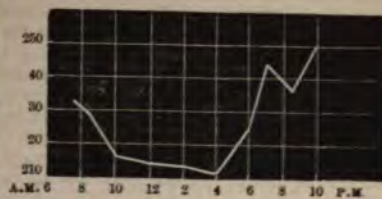


FIG. III.

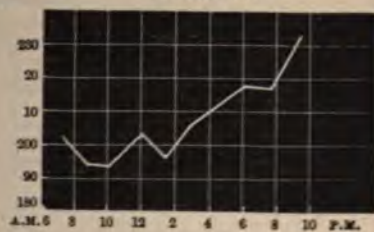


FIG. V.

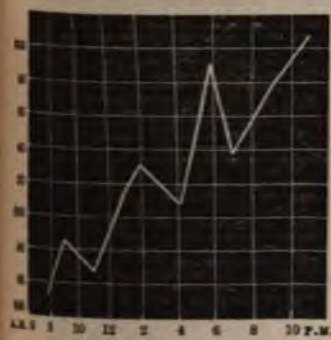


FIG. IV.

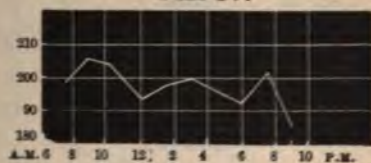
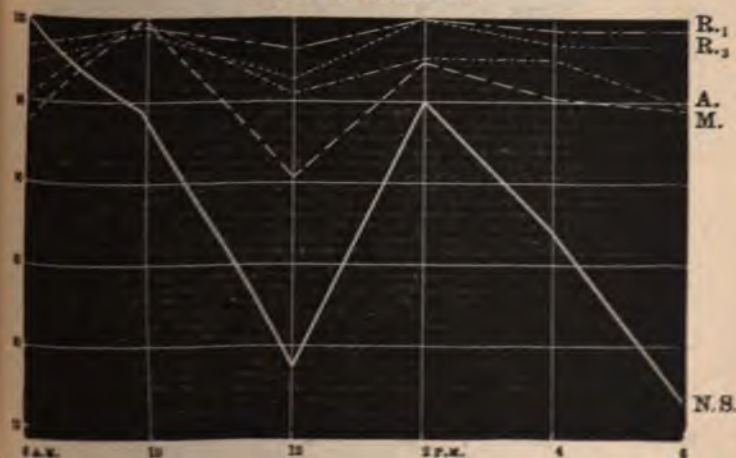


FIG. FOR TABLE IV.



made. The method adopted was the following: The curve of the average representing the constant daily variations was plotted over the curve of variations for each day. The state of the barometer, whether rising, falling or stationary, was then marked upon the curve for the day. The constant barometric variations, with maxima at 10 A. M. and 10 P. M., and minima at 4 A. M. and 4 P. M., are very slight in this latitude and can only be demonstrated by the average of a large number of days. The great fluctuations are irregular. The direction of change of the daily curve from the average is of course either the "same" or "above" or "below." The classification of these changes of direction under rising, falling and stationary barometer gives the results in Table III. The first two subjects evidently show no dependence upon barometric

TABLE III.

		Rising.	Falling.	Stationary.
L. N. B.	Above.	14	19	12
	Below.	15	19	11
	Same.	13	9	3
A. F.	Above.	13	13	3
	Below.	14	11	4
	Same.	6	4	4
T. L. B.	Above.	7	21	17
	Below.	17	13	13
	Same.	3	1	9
M. N.	Above.	9	22	8
	Below.	13	12	8
	Same.	1	4	6

changes, since there are about as many records above as below the average for the various states of the barometer. T. L. B., whose records are very variable, though showing no constant daily changes from 8 A. M. to 6 P. M., gives a preponderance of low records for rising and high records for falling barometer. The same is true of M. N. in a less degree. There would seem to be a correspondence between rapid rate and rising barometer and poor rate and falling barometer in the last two cases. Observations sufficient to establish a safe independent proof of the influences of barometric changes should be more numerous than those we have, on account of the improbability that such slight changes of quality and distribution of the blood as ordinary barometric fluctuations probably produce, will have any effect on the mental processes. The results may be taken to support the view that some people are largely influenced by barometric variations, while others are not. Most of the experiments were taken in March, April and May, so there were in general no extreme temperature changes.

The charts of a Draper self-registering barometer were kindly supplied for the work by Mr. Martin Green, of Worcester.

Other causes of variation occur only a few times. They are mainly external and such as tend to concentrate or distract the attention—though some are of a purely physiological nature.

The Relative Variation of Different Processes.—Table IV. gives the result of an eleven days' experiment by the writer. Six experiments were made a day, beginning with 8 A. M. At each experiment the pulse rate was noted; and then the rate of reading, adding and multiplying numbers, and of learning nonsense syllable series, was obtained in the way described above. Four ten-syllable series were learned each time. A uniform daily routine was maintained—8 to 10, reading of psychological literature; 10 to 12, reading and listening to a lecture; 12 to 2, dinner and recreation; 2 to 4, laboratory work; 4 to 6, walk and recreation. The experiment required from fifteen to twenty minutes each time. For the sake of ready comparison the results have been reduced to percentages, the quickest record being counted 100. The numbers for the nonsense syllables are the inverse of the times required, so as to correspond with the other records. Since the question is here how much can be done in a given time, and not, as in the former case, how long will it take to do a fixed amount, the meaning of high and low records is the reverse of that in the previous tables—a high record meaning rapid rate and a low record poor rate. At the best time it required, respectively, 0.325, 0.346, 0.857, 0.907 seconds for reading a number in a set of three, by ones, and adding and multiplying two together, inclusive of pronunciation; 110 seconds were required for a ten-syllable series at this stage of practice.

The right hand column M. V., which gives the mean variations of the six averages of each test, shows the relative amount each changes under the same physiological influences. With respect to the amount of variation, the tests may be arranged in a series, beginning with the reading of numbers by ones and ending with the nonsense syllables. The same series will be formed if they are arranged in the order of complexity or difficulty, the time required being taken as a measure of the difficulty. An exception must be made of reading by threes, though this is probably a more complex process than reading by ones, in spite of the fact that less time is required per figure.

Some facts may be found here for deciding what test would be most serviceable in similar experiments, and how far variations in the rate of certain processes may be taken as an index of changed

TABLE IV.

Hours.	A. M.			P. M.			M. V.
	8	10	12	2	4	6	
R. ₁	98.27	99.71	97.92	100	99.13	99.2	.765
P. E.	±1.10	±.59	±.97	±.80	±.61	±.85	
R. ₃	97.23	99.29	96.04	100	98.59	98.37	1.02
P. E.	±1.21	±.82	±1.10	±1.52	±.92	±.76	
A.	95.29	100	95.7	97.57	97.57	94.86	1.55
P. E.	±1.53	±1.43	±1.67	±1.68	±.90	±2.	
M.	93.8	100	90.32	97.43	95.01	94.4	2.37
P. E.	±1.01	±1.04	±1.84	±1.22	±1.43	±.87	
N. S.	100	94.24	78.5	94.9	86.88	76.06	7.95
P. E.	±3.16	±3.98	±3.54	±3.72	±3.36	±5.10	
Pulse.	66.9	62.2	62.6	66.8	65.4	65.2	

R.₁ and R.₃ refer to reading numbers by ones and by threes respectively; A. and M. to multiplication, and N. S. to the nonsense syllable series.

physiological conditions. If only very slight, or even no change could be observed in the simpler processes, very striking variations might, nevertheless, be found in the more complex ones. These facts are in harmony with the common experience that easy work can be done almost any time, while the most difficult work can only be done well at rare intervals. The easier tests might be difficult for persons weakened by sickness and so give considerable variations. Reading of numbers would perhaps be sufficiently difficult in many cases to indicate the more considerable fluctuations. With opportunity for plenty of experiments, the most difficult test will give the most striking and satisfactory results; but with only a few experiments no conclusions could be drawn on account of its great irregularity. The greater regularity of the simpler tests makes fewer experiments necessary, but the variations will be very small if demonstrable at all.

A glance at the cut will show that with one exception the variations of the different processes are the same throughout the day. The exception is the nonsense syllable record at 10 A. M. This does not, like the rest, show an increase of rate at 10, though it is not

tain that it shows a diminution. The time between 8 and 10 was supplied with reading, and the result is probably due to a differential effect of this kind of mental work upon the two kinds of processes. The interference of associations is quite marked in the case of the subject. This would account for a small part of the difference, but it should theoretically only affect the first series at 8 A. M., making that a little longer than it would be without the word at 8 A. M., so that the influence would be much too small by itself to account for the result. The variations of the processes under the influences of rest and dinner, fatigue and exercise, it will be seen, are the same. A test of the matter, by exclusion, between 8 and 10 A. M., was not made on account of lack of time. The increased mental excitement at 10, shows itself in greater rapidity of all learned processes, but also by the presence of unconscious tendencies to distraction, which make themselves known by increasing the time of learning the nonsense syllables. At 12 M. there is a crease of all the processes, but at 2 P. M. a considerable increase, after dinner and rest. Four and 6 P. M. again show poor records. The fact that easy operations hardly change at all, while the more complex show considerable variation, was the first result noticed in the preliminary experiments, and formed the basis for further work. In a five days' experiment by two subjects, the following times were required for sorting an ordinary pack of playing cards into four piles at the times of day designated. Three records were made at each experiment. Though the process is far more complex than simple reaction, the changes in it are very small:

TABLE V.

	A. M.		P. M.			A. M.		P. M.	
	9	11	4	10		9	11	4	10
	53.33	50.66	51.2	53.93		53.1	51.4	51.7	57.1
E. C. S.	19.95	19.81	19.3	19.1	J. A. B.	22.9	21.7	21.6	24.9

The first row gives the hour of the experiment; the second, the average time required for the sorting; the third, the time for distribution into four piles *without sorting*. The records of E. C. S., with the nonsense syllables, were taken at a different time, but otherwise under the same conditions, so that a rough comparison may be made. It is evident the ability to learn nonsense syllables changes much more during the day than the ability to sort cards into so few positions. A similar rough comparison of the variation in card memory test for the writer, gives 2.6% as the mean variation of the six records of the morning and afternoon. This makes no stand between multiplication and nonsense syllables in its sensitiveness to variations, though much nearer multiplication than the other. Another series of experiments of four days, besides preliminary practice, with the classification of words and the test of the rate of movement, gave the following results, which, though taken at another time, correspond quite well with those of Table IV.:

TABLE VI.

J. A. B.

A. M.				P. M.					M. V.
7.15	8	10	12	2	4	6	8	10	
262.5	272.5	278.5	267.75	277.25	270.75	269.75	260.25	260.75	1.97%
120.75	132.	135.	124.	135.5	135.5	140.25	131.5	120.	4.52%

The first line gives the hours, the second the number of strokes per minute, the third the number of classifications made in two minutes. Only a rough comparison with Table IV. is intended. The mean variations in the right hand column show that the classification of words undergoes the greater changes.

The experiment, the first eleven days of which gave Table IV., was continued thirty-two days longer, with various changes in the memory test. On ten of these days the regular occupation from 4 to 6 was changed to exciting physical exercise—ball playing or tennis. The results are shown in Table VII. The memory test was not the same on the different days, since they were made for a different purpose. It consisted, usually, of two number series alternating with two nonsense syllable series, but other combinations occurred. The probable errors cannot therefore very well be calculated, but the extraordinary change in rate does not leave any doubt of its existence. It will be remembered that with ordinary quiet recreation there was, on the whole, rather a decrease than an increase of rate at 6. The intermediate days, on which no exciting exercise was taken, show this also. If the 6 o'clock records are put at 100 and the 4 o'clock records expressed as percentages of them, another proof is obtained that the variation of the more difficult processes is relatively greater under the same physiological changes, which may be either *depressing* or *stimulating*.

Some of the 6 o'clock records were made immediately after the exercise, others after about a half hour, but the effect was nearly the same. In the third series of experiments upon the parallel law for lifted weights, Fechner brought on intense muscular fatigue by lifting nine and one-half pound weights in a rapid rhythm. The exercise aroused his whole system, as a very rapid pulse rate indicated. The number of right cases increased instead of diminished after the operation. In a fourth series of experiments, in which the weights were lifted slowly, though the fatigue operation was performed five times in succession, so as to get a cumulative effect, only a comparatively small increase of the pulse rate was noticed, and the number of right cases after the operation is about the same as that before. It seems not unfair to say that these experiments show that with a general physiological change—in this case of a stimulating nature—there is a corresponding change of the power of discrimination, even if this does not change with local fatigue of the sense organ, which latter fact Fechner had especially in view. The analogy with the experiments reported in the last table is, of course, evident, though in the one case rate, in the other

TABLE VII.

Date.	S. P. M.				S. P. M.			
	R. ₂	A.	M.	N. S.	R. ₂	A.	M.	N. S.
April 4.	238.	73.	70.	222.7	221.	77.	75.	222.3
" 5.	205.	77.	68.	202.7	219.	76.	74.	242.3
" 10.	236.	79.	72.	171.25	226.	85.	87.	150.75
" 11.	207.	80.	68.	217.8	222.	82.	74.	222.8
" 19.	222.	82.	72.	222.3	222.	86.	88.	242.
" 26.	196.	86.	85.	152.5	214.	91.	97.	96.5
May 5.	227.	80.	80.	247.	240.	90.	86.	120.5
" 8.	224.	88.	87.	207.	242.	105.	100.	190.5
" 10.	227.	92.	88.	190.	227.	100.	101.	162.5
" 11.	224.	85.	85.	201.5	225.	101.	100.	220.5
Average.	212.2	83.4	76.7	201.73	220.4	91.2	88.4	184.35
	92.00	91.5	90.7	71.2	100.	100.	100.	100.
Pulse.	67.3				95.4			

R.₂, A. and M. refer to reading by three's adding and multiplying respectively; N. S., to the nonsense syllable series.

discriminative sensibility, is measured. The stimulating effect of exciting physical exercise is a matter of common experience. A mathematician, who kindly replied to a circular of questions regarding habits of mental work, finds the first hour and a half after a game of tennis especially valuable for original work. Fechner attributes the increase of the power of discrimination to the acceleration of the circulation. As will be seen later, certain changes in the nervous system probably coöperate with this to produce the result.

Tables IV. and VII. give some data for a study of the relation of the rate of mental work to the circulation. In experiments with the plethysmograph and balance, Momo and others have shown that any mental activity from a simple sensation to the solution of a problem is accompanied by an almost instantaneous alteration of the circulation. The theory of Momo that, in this case, the cerebral

circulation increases while the peripheral diminishes, seems to be based upon too few observations. According to Féré, moderate and pleasant mental activity may be accompanied by an increase of the peripheral circulation, while intense or disagreeable mental effort is marked by a depression. The same author says the tipping of the balance toward the head does not necessarily show an increase of the cerebral circulation, since the majority of organs which express the emotions are on the head side of the centre of gravity. Direct tests of the intracranial blood pressure make it probable, however, that mental activity is accompanied by an increase of cerebral circulation, though the actual amount of blood in the brain does not change very much, since the skull is a closed cavity. M. Gley found a dilation of the carotid, a contraction of the radial artery, and an increase of one to three pulse beats per minute during hard mental work. Leumann, in some experiments upon boys in a gymnasium, found that the normal scansion of poetry was in direct ratio to the pulse frequency. He also explains the so-called apperception rhythm of minimal differences of sensation and the periodic umbrations of mental images as due to the influence of respiration upon the circulation. This subject will be referred to again in the discussion of the results.

A comparison of the rapidity of the pulse with the rate of the mental processes, in Table IV., will show that there is no necessary correspondence between the two. The pulse rate falls from 8 to 10, while the rate of all the processes but the nonsense syllables increases. The pulse rate is about the same at 12 as 10, but the rate of all the processes has diminished. For the afternoon there is a fairly close correspondence. The lack of correspondence between the pulse rate and mental activity is much more striking in the following experiment, which, though it failed in its direct aim, has several points of interest. The object was to study the effect of one kind of work carried on till considerable fatigue appeared, upon another kind of work entirely different. In experiments with the ergograph, Mosso found that Dr. Adduco made a better record in the middle of a four or five hour period of exciting mental work than at either the beginning or end. No measure of the mental work is made, so it is impossible to say whether the increase and decrease of the test work coincided with similar changes in the main work. The object here was to measure both kinds of work, substituting tests of the rate of mental processes for the ergograph record. The translation of German was the main work. Adding, multiplying and reading of numbers and the experiment upon the rate of movement were the test work. A record with the tests was made at 8, 10 and 12 in the morning. The rest of the time from 8 to 12 was divided into eight equal parts by the stroke of a bell. The subject (O. H. J.) translated with the greatest possible rapidity, marking off at the strokes of the bell the amount done. The experiment was made on twelve days, but only the last seven were under proper conditions. Scientific German had been taken up as a study only a few weeks before. Wundt's "Physiological Psychology" was the book read. The number of words for which the dictionary had to be used was also noted. It was supposed that with advancing fatigue they would become more numerous. The experiments were made the first part of August.

TABLE VIII.

No. of lines.	23.8	25.1	29.9	25.9	27.9	24.2	28.1	29.4
P. E.	±2.98	±2.46	±1.61	±3.39	±4.78	±.95	±1.54	±3.03
Unknown words.	7.	7.1	6.7	7.6	7.6	7.4	6.9	6.9
P. E.	±.39	±.35	±.44	±.52	±.46	±.42	±.53	±.44

The first horizontal row gives successively the number of lines translated in each of the eight equal divisions.

The table below gives the rate of the test processes at the times mentioned. The probable errors of the tests are given in the right hand column at the given hours.

TABLE IX.

8 A. M.		10 A. M.		12 M.		
71.3	±2.3	60.7	±1.82	53.	±.39	Pulse.
97.4	±1.46	101.2	±1.26	102.6	±.86	Addition.
64.4	±.74	63.3	±.69	61.7	±1.51	Reading.
81.	±.79	81.	±1.08	81.6	±.62	Multiplication.
260.6	±1.27	268.4	±1.68	268.7	±1.86	Movement.

A possible source of error is that there may have been an unconscious allowance for the amount which had to be done. Toward the end of the four hours' work there was a severe feeling of fatigue, but there is no corresponding diminution of rate either in the main work or test work. After dinner there was a disinclination to effort of any sort. A glance at the records will show that the rate of the main work and the other processes is nearly the same throughout. During the first five days, a recess of fifteen to twenty minutes was taken between 9 and 10. With that routine there was an increase of all the processes at 10, and a diminution at 12, as in Table IV., and there was no feeling of fatigue after dinner, but work was usually taken up at once. The noticeable thing is the great change in the pulse rate, namely, from 71.3 to 53, while there was no change in the rate of the mental processes. The high pulse rate at 8 A. M. is due to the breakfast which has just been eaten, and there is a similar increase after dinner. While an increase of mental activity probably causes an increase of blood flow to the brain, mental activity can evidently not be said to vary with the pulse rate.

Discussion of Results.—The experiments, so far reported, show that the subjects, whose records cover the entire waking period,

have a well marked periodicity of mental activity. There is, however, no *general* type of daily rhythm, and individual differences of the most striking sort occur. The same influences have different effects upon different individuals. The more complex mental processes have relatively greater variations than the simpler. Under the influence of fatigue, rest and physical exercise, the processes studied vary in the same direction. Statistical investigations show that those who are engaged in mental work have generally observed a daily rhythm of power. In connection with other questions, Heerwagen sent out the following: What part of the day do you find mental work easiest? To which, 182 said the morning, 133 the evening, six the afternoon, forty-three noticed no difference, while twenty-eight found it easy at all times. Professor Earl Barnes, in a study of the intellectual habits of Cornell students, received in reply to the same question, sixty-six votes for the morning, six for the afternoon and thirty-nine for the evening. The average student, he says, sleeps eight and one-fourth hours, begins work at 8, but is in doubtful condition, is best at 9, at 10 still in good condition, at 11 he is tired and at 12 is at his worst. He works from 3 to 5 in the afternoon, but in inferior form. After supper he goes to work at 7, and reaches his best at 8. From 9 P. M. he is not at his best, and at half past ten goes to bed. The smallness of the number who chose the afternoon is probably due to the hard work of the morning which all had. Forty-five said their power was uniform from day to day, 109 that it was variable. Three-fourths accounted for the variations by the weather, dry, clear days being approved, dull, damp days denounced. The state of feeling, regular sleep and meals were the causes next in order of importance. Many said they were less able to work after a rest than when they had gotten at it. Thirty-eight said they could get the most rest in an hour's time from exercise, thirty-one from sleep, twenty-three from a walk, six from light reading, four from a bath and three from music. The location of the best hours depends largely upon the hours of retiring and rising. It will be seen that actual measurement and statistics of opinions both show that in a certain number of cases there will be persons whose maximum activity comes at almost any given hour of the waking period. The rhythm of activity may or may not correspond with the actual energy at the person's disposal. Other things being equal, the total amount which can be accomplished decreases as the interval since sleep increases, but the rate of work may be most rapid a little before retiring, as in the case of E. T. and W. O. K. While most persons thus recognize the existence of a daily periodicity of activity, which is of great importance for the quality and quantity of work they can do, this does not conform to any general type, and is, therefore, not an inherited modification of the nervous system. The daily rhythm is the result of a number of stimulating and depressing causes, whose influence habit tends to fix upon the system. Changes of hours of work are often made, though with more or less difficulty.

Some of the answers to the circular sent out by the writer state that a change from evening to morning work had been made. The evening hours were still preferred, but had to be abandoned, because they were "expensive" and sleep was interfered with.

The preceding results do not furnish the material for their own explanation, except in a few points. A brief review of some of the literature bearing upon the subject may therefore be permitted. The hope of finding a thoroughly satisfactory explanation is of course not entertained; but the following facts are suggestive:

Dr. Hodge has demonstrated that nerve cells under electrical stimulation and ordinary fatigue show, for the nucleus, a marked decrease in size, a change from a smooth to a jagged appearance and a darker staining; for the cell protoplasm, shrinkage and vacuolation. The materials of the cells are highly complex and unstable, while the waste products of their activity are more simple and stable. Change from unstable to stable compounds, in this case chiefly by oxidation, is accompanied by the setting free of energy, part of which is used to build up its own substance and part as free nervous energy for the stimulation of muscles and other cells, while another part, as Schiff has shown, appears as heat. Of special interest here is the experimental study by Wundt of the relation of the anabolic and katabolic processes, and the changes of these in sthenic and asthenic conditions of the nervous system. If a strong electrical stimulus is used to test the condition of a nerve during the progress of stimulation from another current of moderate strength, its effects upon the muscle contraction will be reinforced. If a weak test stimulus is used, it is usually suppressed. This is interpreted to mean that during the excitation of a nerve there is an increase both of positive or katabolic and negative or anabolic work. The positive work predominates, especially if the nerve is in the asthenic condition from poor nutrition or cold. The fact that stimuli are retarded and weak ones, entirely suppressed on passing through the spinal cord is evidence that the stimulation of normal central cells causes an increase, especially of the anabolic process. Poor nutrition, exhaustion, cold, and various drugs, especially strychnine, produce the asthenic condition, in which the anabolic process is relatively weakened and the katabolic increased. The fact that nerve conduction is only in one direction through the central cells and that interferences of stimulation take place, gives the foundation for the assumption of regions in the central cells, in one of which the positive work predominates, in the other, the negative. The stimulation of one region causes its characteristic work to spread over the entire nerve cell, so that inhibition or excitation takes place according to the region stimulated. Here as well as in the other cases the asthenic condition diminishes relatively the inhibitory or negative work. Stimulations which would inhibit in the normal condition, in this, produce reinforcement. The fact that reinforcements of sensations and muscular movements are more prominent in neurasthenic and hysterical persons seems to be paralleled here. The greater excitability and sleeplessness in excessive fatigue are another illustration.

The activity of a nerve is a function of both the inhibiting and exciting influences, that is, of the anabolic and katabolic processes. The greater the energy of a nerve, the greater both the negative and the positive work; both of which diminish with exhaustion, especially the negative.

The results of the activity of the nervous system upon its own condition have been grouped under the summation, facilitation and diffusion of stimuli, practice, fatigue and habit. The object here is to refer to these facts simply as a means of explaining some of the daily variations observed. A series of induction shocks applied to the posterior roots of the spinal cord or to the cortical centres may produce a contraction by the summation of effects in the nerve centers when a single shock of the same strength is too weak to have any result. A reflex produced by the stimulation of a given sensory nerve will be facilitated or strengthened if, shortly before, a contraction of the muscle has been produced by the

stimulation of this or any other sensory nerve. The familiar "warming up" to work is probably to be explained in large part by these facts of nervous activity.

In the reflex animal a weak stimulation of a sensory nerve causes a contraction of the muscles of the same side; a stronger one, of those of the opposite side at the same altitude; a still stronger one, a contraction of the muscles lying higher and lower, but predominantly on the side stimulated. A nervous process set up anywhere in the spinal cord tends to diffuse itself in all directions. Similar facts are brought out by experiments upon men, though for this purpose hysterical patients give the most striking results. Féré has shown that the muscular power measured by the dynamometer is greatly increased by previous movements on the same side, and slightly increased by movement upon the opposite side. Any mental activity augments the muscular power on both sides. Musical sensations have a dynamogenic power in proportion to their intensity and height. A similar dynamogenic scale may be made of colors, beginning with red and ending with violet. Sensations of taste and smell have a similar power. An increase of the discriminative sensibility for colors was also noticed as the effect of mental effort or sensory stimulation. Not only were the energy and speed of the movement increased, but also its endurance. Fatigue brought on by too long exposure to a color produced contrary results. Féré notes that reaction time does not reach its greatest rapidity till the stimulating influence of light and heat has operated for some time. Nocturnal paralysis and morning tire are explained as exaggerated phenomena of this sort. The blood distribution is in rapport with these increments of power. These facts, the reinforcement of minimal sensations by other sensations, the influence of central nervous processes upon the knee jerk and the sweat glands, the slight unconscious movements which accompany attention, and many familiar facts of the influence of mental states upon respiration, circulation and secretions, show that central nervous processes diffuse over other centres. If fatigue sets in, there seems to be a corresponding depressing influence diffused over the nervous system. The diffusion of the effects of nervous activity and their retention and summation by physiological memory are the chief facts on the nervous side, which seem to account for the increase of the rate of certain processes with hard or exciting mental or physical work. The increase of power by practice is probably also connected with these facts.

The tendency of work is thus to increase in amount till exhaustion takes place. It is a common experience that the more mental work is done, the more can be done up to a certain limit, beyond which come collapse and despondency and other symptoms of over-training.

While the nerve fibre is comparatively independent of oxygen and food supply, as is shown by the fact that it will function either in air or in indifferent gases for a considerable time after excision, the metabolism of the nerve cells is very rapid. In the experiment with Bretino, Mosso found that unconsciousness and convulsions were produced by only a few seconds' compression of the carotids. The hole in Bretino's skull made the reduction of the blood supply easy, since this was not opposed by the atmospheric pressure. The muscles of the arm still respond to the will after the blood supply has been shut off for half an hour. This shows that nerve cell metabolism is much the more rapid. Dr. Corning has also experimented with compression of the carotids to reduce hyperæmia,

and study the effect of diminution of the blood flow upon mental processes. Compression of both carotids was followed by facial pallor, drooping of the eyelid, dilation of the pupil, soporific tendency, dizziness, heaviness and confusion of ideas, and finally by syncope. He notes that dizziness and confusion of ideas come more quickly if the compression was made toward evening than if made in the morning, which points to fatigue of the brain cells at evening. The latter result would probably not be generally true. Account should be taken of the different rhythms of mental activity, which, as has been shown, is not a problem capable of a general solution.

The great influence of qualitative changes of the blood stream upon nervous activity is well known. Asphyxia can be produced by suspension of breathing for a few minutes. The poisonous atmosphere of crowded rooms produces headache and various other nervous troubles. Especial interest attaches to the qualitative changes of the blood in connection with the theory of fatigue. The poisons of various contagious fevers are probably the waste products of various bacteria. The activities of the cells of the body similarly produce waste products which are more or less poisonous. Washing a fatigued muscle will restore it. Mosso found that the injection of the blood of a tired dog into the circulation of a fresh dog produced in the latter all the signs of fatigue, while a similar injection of blood from a fresh animal had no such effect. Mosso attributes the decrease of muscular power after the four or five hours' intense mental work in the experiment referred to above, as due to the poisonous effect of brain work. Choline and neurine, decomposition products of lecithin, one of the chief constituents of nervous tissue, which may be formed in cell metabolism, have a poisonous effect like curare. Xantho-creatin, which appears in physiologically active muscles, produces depression and excessive fatigue. The presence of uric acid in the blood produces marked symptoms of mental depression and irritability. According to Haig, the neurasthenic symptoms of morning tire and depression after meals are to be attributed to its influence. Its presence depends upon the alkalinity or acidity of the blood. If the blood is alkaline, it removes uric acid from the tissues; but if acid, it causes its storage. By making the blood artificially acid or alkaline, symptoms of depression or exaltation can be produced at will. He assumes that there is an alkaline tide in the morning, after meals and in the spring, to account for depression at those times. In this connection, it is of interest to compare the psychometric investigations of Kraepelin, Münsterberg and others on the effect of drugs upon the mental processes. Experiments with the ergograph upon muscular power have usually shown that the effect of meals was stimulating. In the experiments reported in this paper, three subjects showed a decided stimulating influence of meals, but one showed as marked a depression from them. The effect is probably not a constant one for the same person, but varies with his power of digestion. In the experiments by the writer in Table IV., the stimulating effect of dinner is well marked. In another series of experiments, extending over fifteen days, but not reported here, a depression after meals was equally well marked, while the general daily curve was otherwise the same.

The term fatigue includes a number of facts, not all of the same kind. Its fundamental idea seems to be diminution of power from excessive work. It refers also to the painful feeling accompanying such work. Sometimes it is applied to the decrease of rate of work. If the energy, rate of work, and feeling of fatigue varied concomi-

tantly, so that one could be taken as the symbol of the other, this use of the word would be convenient. In the experiment of C. H. J. there was a severe feeling of fatigue at 12 M., but no corresponding decrease of rate of work. If energy is measured by the endurance still possible, this evidently diminished from the start. In the so-called second stage of fatigue, there is fatigue anæsthesia, a high rate of activity, but an actual store of energy less than in the preceding state of normal fatigue, as is shown by the after effects of the experience. The true physiological condition is not, however, open to direct inspection, but must be inferred from the blunting of the sensibility, the failure of mental processes, and the accompanying feeling of pain, together with various physical signs, such as loss or gain in weight. The rate may not run parallel with the store of energy, but may be ahead or behind, according to various stimulating and depressing causes. According to Cowles, mental symptoms are the most sensitive accessible indices of exhaustion. The order in which they appear is depression of spirits, decrease of voluntary control, morbid introspection, and finally, diminished sensitiveness.

The curve of fatigue calculated by Dr. Hodge from the shrinkage of nerve cells shows that fatigue is first rapid, then slow, and again rapid under continuous stimulation. The curve of recovery is symmetrical with it. He compares this with the somewhat similar curve of muscle fatigue obtained in experiments with the ergograph, where central rather than peripheral fatigue is really involved in many cases.

Bowditch and others have shown that nerve fibres are not easily exhausted. The sense organs differ greatly with respect to fatigue. The sensibility of taste, smell and sight falls off rapidly under stimulation. The retina seems also to undergo daily changes. To two observers, objects seemed twice as bright in the morning as in the evening. Fatigue of the ear comes on more slowly, but has been demonstrated for high tones and loud momentary sounds. Local fatigue for certain tones has also been shown to occur. Stumpf thinks his discriminative sensibility for tones is greater in the evening than in the morning; but as this depends upon the mental processes rather than upon the condition of the ear, this may be due to the fact that his mental activity increases towards night. A continuous series of discriminations of pitch from morning till night, with an intermission for dinner, was made by H. K. Wolfe to study the effect of fatigue upon the number of right cases. In spite of painful fatigue, no diminution in the number was observed. It is to be noticed that he was in good training for this kind of work. Within certain limits, fatigue for a sense organ does not involve a diminution of the power to discriminate. Fechner calls this the parallel law, because it is to be looked upon as the application of Weber's law to inner physiological changes. His experiments have already been referred to. v. Kries found that two lights appeared the same relatively, whatever the fatigue.

That the daily rhythm of mental activity is much influenced by habit is a familiar fact. A brief habituation to certain hours of sleep is sufficient to leave a tendency to sleep at that time. The subject of Experiment VIII. had been accustomed to a short rest in the middle of the forenoon, the month before the experiment. The effects of this seem to show in the records, the average of which for the first three days are 18.3, 20, 26.3, 15.3, 12.7, 21.3, 22.7, 22.3. There is no evidence of its lasting longer. This influence is in part analogous to that of certain eccentricities of eminent men.

Many writers feel the need of being in particular spots, of using peculiarly colored paper or ink before they can do well. Rousseau found composition difficult unless he was walking. Neander composed best lying on his stomach. Coleridge liked to compose when walking over uneven ground. Sheridan composed at night with a profusion of lights around him. Lamertine had a studio of tropical plants. Dr. George Ebers imagines himself more at liberty to write with a board on his lap than at the desk. Vacano composed at all times, but the place he was in was important, and he could write best in the hubbub of peasant life near an old mill. Maurice Jokai must have fine pens and violet ink. These habits, however acquired, evidently have great power of distracting the attention if they are not satisfied, and so retard work. As a positive influence, they may serve as a sort of hypnotic signal for the state of composition. Similarly certain times, certain occupations may serve as a signal for rest or activity during the day. The increase or decrease of mental excitement may thus be due to no special physiological change, but to the influence of suggestion and habit, and they are factors which should be taken into account in mental or physical training as well as the more prominent physiological facts.

Other Experimental Work upon the Subject.—In his experiments upon memory, Ebbinghaus found it required twelve per cent. more time to learn a sixteen syllable series from 5 to 7 P. M., than from 10 to 12 A. M. Oehrn notes incidentally a single trial of one hour in the morning and one hour in the evening by two subjects, one of whom added faster in the morning than in the evening, while the other did the contrary. Dr. W. P. Lombard, in experiments with the ergograph, found a remarkable twenty-four-hour periodicity of the power of making voluntary muscular contractions. Both in the experiments upon the causes of variation in the knee-jerk, and in these just referred to, a marked influence of barometric changes was noticed. Rising barometer was followed by an increase, falling barometer by a decrease of muscular power. The actual barometric height was unimportant. The constant daily variations had their maxima at 10 A. M. and 10 P. M., and minima at 4 P. M. and 4 A. M., thus corresponding with the constant barometric changes. Daily variations from the constant curve followed the accidental fluctuations of the barometer. While there is a little evidence that two subjects were influenced by barometric changes in the present investigation, there is none for such a daily periodicity. In his case, the effect of the slight regular changes of pressure is supposed to have become organized into the habit of daily variations referred to. Dresslar found a daily rhythm in the rate of tapping which seemed to correspond with his habits of work as teacher for the two previous years. The rate was increased by exciting mental work, but diminished by long walks.

PART II.

Some experiments upon memory by means of the interference of associations were reported in the *AMERICAN JOURNAL OF PSYCHOLOGY*, Vol. V. No. 3. The object here is to give an account of a few additional experiments and especially to show the influence of interference upon mental activity. Table X. gives a summary of the results of the article referred to. The memory experiment consists of sorting two packs with the same words or symbols successively into different positions. On the average sixty-five seconds were required for the first pack, but eighty-five for the second, if

this was sorted immediately afterwards. The difference is called the interference time, since it is due to conflict of associations and not to fatigue. The time for sorting the cards decreased considerably with practice, but the amount of interference did not. The average of the interference time for the first four subjects, the first seven or eight days, is 17.10 seconds, for the next seven or eight days, 17.53 seconds. This shows that it is not a temporary phenomenon which a little practice may obviate. The interference time is not due to fatigue, since, if two packs with different symbols are sorted in succession and the order in which they are used is changed to compensate for differences in them, the average of the first pack for four subjects is 62.89, for the second, 61.99, showing no increase. If there were general fatigue of the attention or of the nervous susceptibility, that would show itself by an increase of the time of the second pack with different words. The effect might be due to local fatigue of a striking sort; but three facts set aside this hypothesis. Less time is required for the second pack if the cards are sorted into the same places as before. In sorting pack after pack with the same symbol continuously for an hour or so, the time of the first is short, that of the second long, that of the rest is about the same as the time of the second, showing only toward the end a slight tendency to longer records, which may be due to fatigue. The chief fact which shows the nature of the process is the very

TABLE X.

	3''	15''	30''	60''	120''	240''	480''	960''	Number of Experi- ments.
F. D.	26.62	21.59	20.69	21.08	20.11	15.88	13.56		112
P. E.	±1.23	±1.25	±1.5	±1.45	±1.18	±.96	±.97		
O. C.	17.33	13.48	14.18	13.45	10.66	11.72	11.34		126
P. E.	±.91	±.74	±1.17	±1.26	±1.26	±1.25	±1.18		
M. E. B.	28.79	23.66	19.89	17.34	13.57	13.09	10.08	7.79	94
P. E.	±.70	±.92	±1.20	±1.01	±1.31	±1.23	±1.62	±1.11	
J. A. B.	33.09	25.18	20.42	15.76	13.75	12.	11.04		147
P. E.	±1.2	±1.07	±1.1	±.83	±.81	±.65	±1.04		
T. L. B.	21.85	21.42	17.88	12.25	10.02	11.2	10.25		66
P. E.	±2.28	±2.19	±1.87	±2.14	±2.02	±3.96	±1.44		
Average	25.54	21.07	18.61	15.98	13.62	12.78	11.25	[7.79]	

great tendency of some subjects to make false motions in the direction of the places the cards were in for the first pack. The delay can be seen to be caused by the actual making of a great number of incorrect movements. To a large extent interference is unconscious. With the longer intervals where the interference amounts to twenty or twenty-five per cent., the subject frequently feels no trouble. With the shorter intervals there is usually great confusion, but the false movements are not known to be such till they have been made. This shows the reflex nature of interference. The sorting of cards involves the learning of new associations and requires an intense effort of attention, any distraction of which causes a great lengthening of time. Interference demonstrates experimentally certain relations of mental activity and memory to the nervous system, since its persistent, involuntary, reflex nature proves it to be physiological. It shows that even such complex processes after a very few repetitions are carried on largely by reflex activity; and that reason comes in chiefly in case of error. The first horizontal row gives the intervals between the two packs. The following rows give the amounts of interference for the different subjects as the interval increased, together with the probable error.

The general feature is a rapid decrease of interference at first, with a very slow diminution afterwards. The attitude of the subject is to forget rather than to remember the previous positions.

Interference in the other memory experiments has only been studied upon the writer; 226 series of nonsense syllables, ten each, were learned from March 20th to March 31st. Four series were memorized each time, with about ten seconds' interval between to give an opportunity to mark down the result. A similar experiment was made with number series, each containing thirty digits. Three series were learned each time. There are eighty-eight in all. The average of those which were learned in the first, second, third and fourth places gives the following result:

	1	2	3	4
Syllables.	104.55	131.05	134.78	137.16
Number.	271.75	302.12	306.96	—

The time of the second series is considerably longer in both cases, but the increase in time after that is very slight. A similar result is found in Ebbinghaus' records. The average of ninety-two groups of eight twelve-syllable series gives for the successive series 105, 140, 142, 146, 146, 148, 144, 140. The great increase of the second above the first and the slight difference afterward is noticeable. The same difference between the first and second is to be found in his other experiments. The series whose averages are given was taken at the beginning of his experimental work and shows a slight increase of time up to the sixth, which may be due to fatigue. In subsequent experiments there is no such increase, but after the great lengthening of time for the second series, there is a certain oscillation above and below the average with no evidence of fatigue. Ebbinghaus notes especially the rhythmic oscillation of the averages. The odd series were learned more quickly than the even. He attributes it to a rhythm of the atten-

tion or the sensibility, but does not explain the fact further. The interference of association will explain the sudden increase of the time of the second and probably also the rhythm of the odd and even series. As has been said, nonsense syllable series are essentially different arrangements of the same symbols. The opportunity for interference thus exists. To test the matter experimentally, nonsense syllable and number series were made up from the first half and the last half of the alphabet and digits respectively. Nonsense syllable and number series could now be learned in succession without interference. If the lengthening in time took place nevertheless, fatigue might be the cause. Series of nonsense syllables and numbers were also learned alternately, thus shutting out interference. The result shows that when interference is avoided, no increase of time takes place. The fact that reading written and printed letters may be learned separately, gave rise to the theory that if the second series was written and the first was printed, or vice versa, interference might be avoided, but this was not verified. Interference will therefore explain the increase of time of the second. It is also an influence which is fitted to give rise to the rhythm noticed by Ebbinghaus. The third series is probably learned more quickly than the second, because the interference from the first has died away, and the second series was not learned so well and does not retard so much as the first did the second. Since the third is learned more quickly, the interference again becomes greater for the fourth, so that if the oscillatory variation is set up it would tend to perpetuate itself.

Two series of numbers and two series of nonsense syllables, one of each kind described, were learned at the same hours as the series last referred to. Sixty-six series of nonsense syllables and sixty-eight series of numbers give the following result:

	SYLLABLES.		NUMBERS.	
	1	2	1	2
Average.	151.	138.99	228.34	193.29
P. E.	±8.21	±7.14	±10.61	±6.13

1 and 2 in the second row indicate that the series was learned in the first or second place. Forty-four experiments, in which a syllable and a number series alternated in the first and second places, give 172.75 and 173.65 as an average. There is an actual decrease of time for the second series of numbers, while the averages of the alternating series are about the same. The experiments with the series give, of course, only an individual result, and chief reliance is placed upon the experiments of others with the cards.

The following explanation of the decrease of the time of the number series may be offered. The subject had been learning series in which all the digits appeared, for considerable time. Series containing half the usual number may be supposed to call up the absent members. There would be simply stimulation of the nervous tracts, but no formation of associations. The effects would summarize and the series containing the absent digits be more easy

to learn. The same thing is noticed in the syllable series to a less extent. If packs of cards are sorted in immediate succession the results resemble those for learning a number of series. The averages of a series of card experiments, on five different days, by F. B. D., were 113.2, 140.6, 135.4, 144.8, 148.8, 140, 139.4, 143.2, 138. The increase of the time of the second and the oscillation of the rest above and below the average are to be noticed.

That the interference is not due to any local nervous association of the centres of the eye and hand, but is the after effect of a more central or apperceptive mental process, is shown by the following experiment. Instead of actually sorting the first pack, and thus learning the associations which afterwards retard the second, the subject is asked to learn them by repetition, like nonsense syllables, till he can tell where they are. This excludes special training of the hand centres, and in case the positions are learned by ear, of the eye centres. Had the interference disappeared for sorting by hand, that would be evidence that it was due to some local association; but the fact that it appears strongly shows that the nervous process is central. Table IX. gives the results of the experiment. Under "before" and "after" is given the time for sorting a pack of cards without interference from a previously learned pack. Under "eye" and "ear" are given the time required for sorting a pack when different positions of the cards have been learned previously by seeing them on the table or by being told where they were. Under "eye" and "ear" the numbers are averages of three, so that the final average is made from twelve experiments. Different packs were used "before" and "after" on different days, to compensate. The experiment required nearly an hour each time, but it will be seen that there is no evidence of fatigue. The fact that the interference is greater when the eye is used to learn the positions on the table, is perhaps connected with the fact that the positions were learned more easily by eye than ear, the average time required being 78.9 and 126.6 seconds respectively. Some time was, of course, lost through the person who told the positions in learning by ear.

TABLE XI.

M. E. B.

	BEFORE.	EYE.	EAR.	AFTER.
	64.6	85.33	79.33	56.6
	55.5	87.53	77.73	58.4
	56.4	83.66	80.09	54.2
	51.8	88.	77.8	56.
Average.	57.05	86.13	78.76	56.3
Interference.		29.45	22.02	

After-images of all sorts are of special interest for the theory of the relation of mental states to the nervous system. The possibility of the experimental variation and measurement of interference, which may be called an after-image of central activity, makes it important in this respect. After-images and practice give, in a way, more minute and tangible, if not so full, evidence of mental and nervous concomitance as the great lines of argument from the mutual influence of bodily and psychic states, brain lesions, and comparative anatomy and psychology. The last experiment shows that similar impressions, in so far as they are identical, go to the same central tract, irrespective of what sense they were learned by. It emphasizes the importance of the central associative nervous process, and is opposed to any rigid dismemberment of the memory for facts into different sensory types, except where by this distinction is meant that the same thing can be learned more easily by one sense than by another.

With reference to the rate of mental processes, the fact brought out by these experiments is that if certain sensory data have been associated in one way, it is temporarily more difficult to make a different association of them than if the first did not exist. Table X. shows the rate at which this difficulty diminishes with time. If the interval between the two packs is twenty-four hours, the second will be sorted more quickly instead of more slowly, as is shown by the steady increase of the rate of work by practice. There are two opposing tendencies in the experiment—one, the general training of the attention and the organization of all the movements; the other, the intense associations just made by learning the positions in any given experiment. The latter is temporarily able to produce the retardation noticed, but, since it is the result of only a few repetitions, it fades away quickly, while the first is continually strengthened by practice. The resultant of the two tendencies thus makes the sorting of the second pack or the learning of the second series immediately afterwards much more difficult, but, after a considerable time, easier. The application of the fact to memory experiments and memory work generally is, of course, evident.

It has probably, however, more important applications to inventive mental work. For this view there is no direct experimental evidence, but only such general reasons as will be adduced. The influence of physiological memory in retarding the progress of thought is in some respects recognized. After reading a certain author, many persons find it hard for a time to assert their own stand-point or style. It is usually necessary to let new views sink down to the level of intensity of older views before a fair estimate of them can be made. Many have been obliged to form the habit of letting matters which arouse their interest rest over night for a fair judgment. In mathematical calculations, many persons will repeat the same error over and over again, and be temporarily unable to obtain a solution. After a few hours they return to the work and are surprised at the ease with which they arrive at the correct result. While the memory of the error was recent, it prevented the true association; but when its intensity had diminished sufficiently, the correct association again became easy and natural. Similarly, in the translation of a foreign language, the mind may have become set for a certain meaning, or some error may have crept in and the passage is for the time misunderstood. Some hours later the proper sense of the passage may appear at a glance. In investigation, there may be a clear knowledge of the end desired and the power to estimate the value of facts and theories which

bear upon the problem; but the solution is conditioned by the association of ideas. In invention of all sorts, mental processes form with difficulty, and slight hindrances are sufficient to obstruct them entirely. Many of the processes which belong to the solution of the problem do not enter distinct consciousness at all, and yet may be essential for a correct result. These are especially liable to error. Everyone has perhaps heard long debates upon such questions as: "Which should we say, that five and six is, or that five and six are twelve?" With the attention focused upon *is* and *are*, the wrong association, five and six equals twelve, is not noticed. A mathematician says that in studying geometrical figures, only a few relations can be made out at a time. Others are seen so easily some time afterwards that the wonder is they were not seen at once. In this case, one grouping of the facts produced a certain discovery which gave a given direction to interest and association. Other associations, not in the same direction, would be temporarily more difficult. With this, there may co-exist certain errors like those in computation and translation, whose intensity must die away before progress can be made. In recalling names or facts which are nearly forgotten, we sometimes succeed at once; at other times we find it impossible. A second trial a few hours after our failure may bring success at once. The resultant influence of recent experience perhaps sent the associative movement in the wrong direction. This error would still further increase the difficulty. Both influences must diminish in intensity before the correct association can take place. A persistent hunt for clues is fatiguing, but, of course, often succeeds.

As a class, these facts have usually been explained by some theory of unconscious cerebration, or they have been attributed to the summation of stimuli, or to rest.

That these phenomena are due to a summation of stimuli which gradually gathers sufficient strength to break through the nervous resistance into the correct association, seems improbable, since every nervous excitement diminishes in intensity if left to itself, and the quickest and most striking results are usually obtained by putting the problem entirely out of mind for a while.

Fatigue and rest have, as is well known, a considerable influence upon the rate of mental work, but these cases seem to occur, just as in the memory work, when there is no fatigue present. The unreliable and often fantastic character of mental processes in the indirect field of consciousness, in reverie, and in dreams, the stupidity of secondary consciousnesses, together with the absence of fatigue from this imagined unconscious cerebration, makes it probable that it is of little importance, and that conscious attention is the forge in which most, if not all, valuable mental work is done.

It is not believed that the formation and fading away of certain errors and tendencies of association offer a complete explanation of these and similar facts, but simply that this influence should be given a place in any list of those which hinder mental processes, and that it is a prominent cause of the surprising retardations in the cases described.

I.

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(2) Some of the Influences which Affect the Power of Voluntary Muscular Contractions. *Journal of Physiology*, XIII. 1 and 2.

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III.

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EBBINGHAUS. Über das Gedächtniss, p. 47-61.

MÜLLER AND SCHUMANN. Experimentelle Beiträge zur Untersuchung des Gedächtnisses. *Zeitschr. f. Psych. u. Physiol. der Sinnesorgane*, Bd. VI. 2 and 3, p. 173. 1893.

A NEW ILLUSION FOR TOUCH AND AN EXPLANATION FOR THE ILLUSION OF DISPLACEMENT OF CERTAIN CROSS LINES IN VISION.

By F. B. DRESSLAR.

I recently had occasion to test the experiment described by Loeb¹ with reference to the illusion which arises in touch, when two edges are placed so as to cross each other on a level, at a moderately small angle, and the finger tip be passed along one edge and over the junction point. The illusion which he noticed is to the effect that the segments of the line along which his finger moves, appear to be raised up where they meet the diagonal line, something like the rafters to the centre pole of a roof. As he remarks, the illusion is only present when some little pressure is exerted on the edge by the finger, and not when it is run along the edge lightly.

According to the author the illusion arises from the fact that as the finger approaches the junction point, instead of all the pressure being exerted on the one edge, it is exerted on two, and in this way the finger is slightly raised, and is kept so until the junction point is passed, when it begins to sink and so gives a basis for the illusion.

There is another illusion connected with this experiment which, so far as I know, has not yet been described. Although it is quite distinct in using two edges, as Loeb did, it can be more easily shown by pricking pin holes through a card along two lines crossing in the same way as the two edges described. In Figure 1, let A D and B C represent two lines of pin holes on the roughened side, crossing at O. If the finger tip be passed along B C, toward C, when it crosses the junction of the two lines at O, A D will seem not to be a straight line, but a broken one, and the section O D will seem to be displaced and to occupy the position o' d'. Likewise if the finger passes along A D, toward D, the line B C will appear broken, and the part O C will take a corresponding displacement. The illusion is quite striking, and the following explanation is offered:

The finger, as it passes along the edge followed, touches the part of the diagonal line, making the acute angle much sooner than the other part, because the finger tip extends for some distance beyond the sides of the edge it follows, and since the ability of the finger tip to distinguish two points as separate requires a separation of these points from 2 to 4 mm., the sensation from the first one touched will have disappeared as a distinct sensation before the sensation of the other as distinct is given. And thus, from the basis of the sensation received, an apparent displacement of the line must needs follow. This view is strengthened by the fact that the illusion does not appear if the cross line is approached at a right angle. Neither does the displacement appear if a portion of the finger tip be pressed through a small hole in a

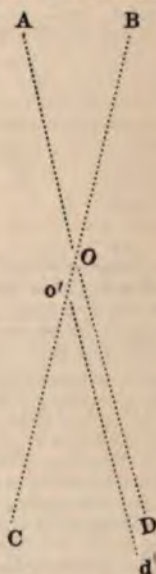


FIG. 1.

¹Pflüger's Archiv., Vol. 46, p. 33.

card, and so fastened that only a small portion of the sensitive surface of the finger be allowed to touch the edge followed.

Since a similar illusion appears for the eye, it has occurred to me that the same explanation given for the illusion of touch would answer for the illusion in vision if the fovea of the eye be considered as receiving the stimulus in the same way, or at least in a way comparable to that of the finger tip.

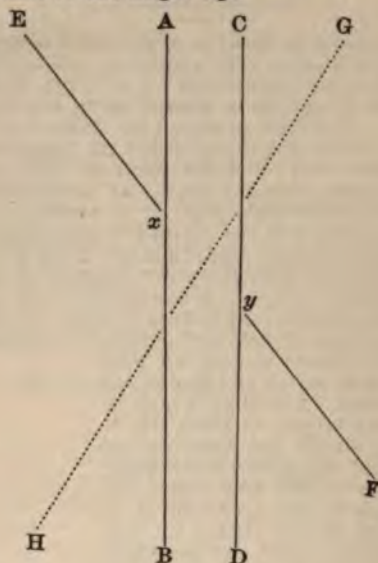


FIG. 2.

If, in Figure 2, the eyes follow the line A B, from A toward B, the point *x* will enter the field of clearest vision sooner than point *y*, and as the eye must move down some little distance before it gets the clearest view of *y*, the tendency is to make the displacement appear in the line, as if the point *x* had been met with in the same line (C D) as *y*, but as much higher as the difference in time of receiving the impression would indicate. The illusion still appears if the lines A B and C D be removed, but all other conditions remain unchanged.

The following observations are in harmony with this explanation:

1. The illusion is more marked when the eyes move along the line A B (Figure 2), from A toward B, than from B toward A; while in following with the eye the line C D, the illusion is greater in passing from D toward C than contrariwise.

2. The illusion disappears if the cross line be approached along the dotted line G H.

3. The illusion also disappears if the eyes be directed immediately to the point in the centre of the space between the perpendiculars which the cross line, if completed, would cut.

4. The illusion diminishes as we pass from the most sensitive part of the retina, and, when the light is sufficiently strong, in the outer part of the retina, it is wholly lost, whether the eye be in motion or at rest.

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PSYCHIC EFFECTS OF THE WEATHER.

By J. S. LEMON.

Psychology is now studied from many points of view, but the relation of psychic processes to weather has never received much attention. The subject opens a field as large as it is new, in which the writer has for some years been collecting materials. The following note may suffice to show the scope of the subject, till a fuller and more systematic presentation I hope to make ere long:

That all the world is interested in the weather is shown in the forms of salutation in every language I have been able to learn of. This interest was, of course, deeper when the race lived more out of doors; but we shall never be independent of thermometers and barometers. Things in general remain fixed, but weather is variable. What attracts attention first daily is still for most people the sky, clouds, wind, etc. What we term "weather" is a product of several variables—moisture, temperature, pressure, ozone and electricity at least. Several of these usually change together, if not all, so that to study man as a weather indicator, we need not go to the hospital or sick room, nor consider exceptional phenomena like tornadoes, cyclones and hurricanes. "Weather" is most prominent in the temperate zones, where the quantum of changes in all these variables is greatest and most frequent. Every long hot or cold term, or other exceptional state, affects the death rate. Unseasonable or anti-seasonable changes are especially fatal.

Many savage and ancient people believed deities, friendly and unfriendly to man, contended in hot and cold, moist and dry, and strove to appease storm and wind gods. From very early times, predictions have been essayed, and weather prophets have won renown. Some of the great crises of human history have been determined by weather. So that it is no wonder omens have been sought and held in high favor. That the sun plays so great a rôle in weather changes is one cause of the prevalence of solar myths, descriptions of storms as battles, of hostile deities, the lightning myths Kühne has collected, and from those times down to the modern sun bath, blue glass craze, hot and cold baths, ozone inhalations, electricity, weather factors have entered prominently into therapeutics. So out of the range of human control have been balmy winds and lovely skies and sunshine, that it is not strange that religious people in Christian lands and ages have been moved to seek the help of God for propitious weather. We notice in the last revision of the Episcopal Prayer Book that prayers for favorable weather, for rain, for safe voyages, for protection from lightning, etc., have been retained. So the Romans sought skyey auspices, and the Athenians prayed for rain. Atheists often become quite orthodox in storms, and the superstitions of seamen are well known. Once, when it was thought bad weather was due to Satanic

influences, Pope Innocent had a manual prepared to be used by the clergy in unfavorable weather, and for the expulsion of demons. Seneca attributed weather to certain inexorable beings and left no place for appeal to deity to change it. Very likely one difficulty monotheism has constantly had against it at every stage, is the difficulty of conceiving how one deity could produce such diverse and opposite kinds of weather.

Sidney Smith wrote: "Very high and very low temperatures establish all human sympathy and relations. It is impossible to feel affection above seventy-eight degrees, or below twenty degrees F. Human nature is too solid or too legend beyond these limits. Man lives to shiver and perspire." Seneca said: "The empire of the world has always remained in the hands of those natures who enjoy a mild climate. Those who dwell near the frozen north have uncivilized tempers." Some think clothing, which has so profoundly modified man's moral and physical constitution, was due, not to modesty chiefly at least, but to weather extremes. We seem more delicate, and less able to bear weather than our ancestors, without modern protection.

Dr. Farr, of England, and Dr. Stark, of Edinburgh, almost lead us to think morality is registered on the thermometer, so surely does it measure certain kinds of criminality. S. A. Hill, of India, believes forty-eight per cent. of crimes of violence disappear in cold weather. I shall dwell at length later upon the psychic effects of weather on the insane and idiotic, and upon certain forms of disease. Certain tendencies are repressed and counter influences set at work or made dormant. A sudden rise of temperature predisposes those liable to an attack of mania. Some patients are so sensitive to and dependent on the weather that they anticipate the thermometer and barometer. One sign of growing neurotic diathesis is inability to keep at the top of one's condition and in good tone in unusual weather. On suicides the effects of the weather are well known. The interesting little book of weather proverbs, published by the weather bureau, is a psychological treasure-house upon all this subject.

Nearly all vocations—some, of course, more than others—are affected by weather. Men of science are often as much subject to weather as seamen. Some writers must have the weather fit the mood, character or scene, and can do nothing if they are at variance. An adverse temperature brings them to a dead halt. If one will but read poetry attentively, he will be surprised to find how much of it bears weather marks, scattered all through it. A popular writer thinks weather often affects logic, and that many men's most syllogistic conclusions are varied by heat and cold. Diverse weather states may be one cause of so much diversity and even disagreement in thought processes, usually regarded as scientific. I have collected opinions of many experienced teachers, and nearly all think there should be modification of both school work and discipline to correspond with weather. Animals respond to it promptly and with no restraint, and almost constitute a sort of weather signal service if observed. Ancient prognostics were based here. Dunwoody, *e. g.*, gives lists of animals which become restless before rain.

The fact that now our property as well as our comfort depend on the weather has given weather a new power as an excitant which dwellings and clothing had lessened. Jesus, Mat. 16:23, rebukes over-sensitiveness to weather. Man should be joyful or depressed, optimistic or pessimistic over other things than this. The anxiety expressed by the directors of the world's fair about the weather of

the opening day was a strain which suggested suffering. Some have said that a certain very famous criminal was acquitted last August solely because the stifling air of the court room made it impossible to follow a severe logical argument. The weather bureau does a great work in economic psychology.

We have gathered a long list of ejaculatory expressions, and unpremeditated remarks concerning weather, which furnish data of interest. Commercial travelers often make much account of it. The head of a factory employing 3,000 workmen said: "We reckon that a disagreeable day yields about ten per cent. less work than a delightful day, and we thus have to count this as a factor in our profit and loss account." Accidents are more numerous in factories on bad days. A railroad man never proposes changes to his superior if the weather is not propitious. Fair days make men accessible and generous, and open to consider new problems favorably. Some say that opinions reached in best weather states are safest to invest on. "I never," says one, "make any important agreements or bargains when the sun shines and I am at my best." Most remarks about the weather are made in the morning, then we settle to it as an accepted fact. Women accept the weather, and, unlike men, very rarely express indignation if it is not to their mind.

Nothing is more curious than the way these ideas have been wrought into description of heaven and hell, whether in heathen, classic or Christian writings. Hell is very hot or cold, and aggravates pains of all diseases. The *Dies Irae* was a day of wrath suggested by a day of storm and earthquake. Read the well-known hymns beginning, "Hark, hark my soul," "O mother, dear Jerusalem," "Jerusalem, the golden," and many, many others.

Laboratory investigation of the subject meets at the outset the difficulty of distinguishing results of weather changes from similar states otherwise caused. This difficulty is no greater than are many other topics of research, and we believe will not invalidate our methods and results. All our senses put us in rapport with the external world. The knee-jerk seems proven to have a weather factor. It is not strange if the eye, *e. g.*, which wants the normal stimulus, in long dark weather causes other changes. Changing moisture in the air changes odors, and many appetites are affected, as touch is still more obviously. Tea tasters work best on fair days.

CLARK UNIVERSITY, July, 1893.

PSYCHOLOGICAL LITERATURE.

I.—ANTHROPOLOGICAL PSYCHOLOGY.

A. F. CHAMBERLAIN, PH. D.

A. PAIDOLOGICAL, PEDAGOGICAL (and related subjects).

Some Comments on Babies. M. W. SHINN. *Overland Monthly* (San Francisco), XXIII. (1894), 2-19.

General discussion, supplemented by observations of the author. Illustrated by numerous drawings and photographs of infants of various races.

The Study of Children at the State Normal School at Worcester, Mass. E. HARLOW RUSSELL.

Thoughts and Reasonings of Children. H. W. BROWN. (Reprinted from the *Pedagogical Seminary*, Vol. II. No. 3), 54 pp. 8vo.

Professor Russell's paper explains and illustrates the system of child study in vogue since 1885 at the Worcester State Normal School. Mr. Brown's contribution consists of some 375 records of the thoughts and reasonings of children, classified under the following heads: Misunderstandings of words; Applications of sayings; Explanations of things; False reasoning; Thoughts and Reasonings about God, Christ, heaven; and according to the age of the children observed. These papers must be of the greatest interest to the psychologist and the anthropologist.

The Hearing of Children. O. CHRISMAN. (Reprinted from the *Pedagogical Seminary*, Vol. II. No. 3, 1893), 45 pp. 8vo.

A résumé of all experiments upon the hearing of children by observers in various parts of the world. The only work giving a general idea of the labor done in this department of research.

Correlation of Mental and Physical Powers. J. VENN. *Monist* (Chicago), IV. (1893-'4), 5-19.

Based upon anthropometric tests of students at Cambridge, England, combined with standing in class-lists at examinations. Author holds that there is decided evidence in support of the view that bodily and mental excellence are independent of each other, and calls attention to the Hindoo scholars in support of this contention.

Ueber die geistige Ermüdung von Schulkindern. L. HÖPFNER. *Ztschr. f. Psych. u. Phys. d. Sinnesorgane* (Hamburg u. Leipzig), VI. (1893), 191-229.

The first part of this paper (191-203) is devoted to the consideration of the experiments and investigations of other observers,

especially Galton, Sikorski and Bürgerstein; part second to Höpfner's own observations on a class of some fifty boys, averaging nine years of age. The test consisted in the writing (which took up more than two hours) of nineteen sentences, averaging thirty words each, and part third (211-229) to a psychological analysis of the mistakes for the purpose of the study of fatigue.

Die Ferienkolonien in der Schweiz in den ersten, 15 Jahren ihrer Entwicklung, 1876-1890. A. MARTHALER. *Ibid.*, 473-489.

The first idea of children's colonies for health and pleasure was put forward by Pastor Bion in Zürich. An appeal to the public of that city resulted in the subscribing of 2,300 francs, and in 1876 three colonies, consisting of sixty-eight children, were sent from Zürich into the mountains of Appenzell. In 1878 Basel followed suit; in 1879, Aarau, Bern and Geneva; in 1880, Chur, Neuenburg and Schaffhausen; in 1881, Winterthur; in 1882, Euge bei Zürich; in 1883, St. Gallen; in 1884, Lausanne. Then there was a cessation until after the international congress of children's colonies held at Zurich in 1888, when two other towns, Biel and Töss, adopted the custom in 1889. The following table shows the growth of the movement since its inauguration:

	Number of Colonies.	Number of Conductors.	Number of Children.	Expenditures in Francs.
1876	3	10	68	2,361
1890	53	86	1,403	52,185

Die philosophische Bedeutung der Ethnologie. T. ACHELIS. *Vierteljahrsschr. f. wissensch. Philos.* (Leipzig), XVII. (1893), 286-311.

The author, the acknowledged German authority on ethnological jurisprudence, treats of ethnology in its relations to psychology, to the theory of knowledge, and to ethics.

Exhibit of Games in the Columbian Exposition. S. CULIN. *Journ. Amer. Folk-Lore* (Boston and New York), VI. (1893), 205-227.

In this paper Mr. Stewart Culin, of Philadelphia, gives an account of the exhibit of games (based upon the collection in the Museum of Archaeology of the University of Pennsylvania, the result of two years' gathering) at the Columbian Exposition. The exhibit begins with puzzles and the simple games of children, and ends with complicated card games. Many interesting facts of invention and distribution are brought out.

The Pursuit of Happiness. D. G. BRINTON. *A Book of Studies and Strappings.* Philadelphia, 1893, XIV. 292 pp. 8vo.

Treats of the possibility of happiness, the definition of happiness, the relative value of pleasure, the distribution of happiness, self-education and the promotion of one's own happiness, bodily and mental constitutions, physical surroundings, luck and its laws, occupations of necessity and choice, money-making, pleasures of the senses, pleasures of the emotions, pleasures of the intellect, satisfaction of the religious sentiment, cultivation of individuality, safety, liberty, education, morality, duty, benevolence, business,

society, fellowship, comradeship and friendship, love, marriage and the family relations, the removal of unhappiness, the inseparable connection of pleasure and pain, the education of suffering. All these topics are written of in Dr. Brinton's best vein. The book is epigrammatic, entertaining, inspiring and excellent in point of literary style. Among the author's final words are these: "Thus, at the end of our wide wandering in pursuit of happiness, we look back and see that it is absent from nothing in life, not even from pain and sorrow; nay, that when all else has gone, when youth and health and fortune and love have left us, when we look forward despairingly to naught but loneliness and suffering, our very despair may prove to be divine, 'begotten by the finite upon the infinite,' and from its depths we may draw a rapture unknown to common pleasures, and taste the sweet waters of a bliss that is celestial." The "Pursuit of Happiness" is a book the reading of which is sure to take away some of the sting from the thought of Dante, so beautifully turned by Tennyson, "A sorrow's crown of sorrow is remembering happier things."

Der Blutbergglaube in der Menschheit, Blutmorde und Blutritus.
H. L. STRACK. 4te, neu bearb. Aufl., München, 1892, XII. 155 S. 8vo.

This is a thorough study of superstitions and ceremonies connected with the use of blood, abounding in bibliographical and historical references. Among the topics considered are blood oaths; healing by blood; superstitious uses of blood and other parts of executed criminals, suicides, infants, etc.; human sacrifice, etc. The main portion of the book, however, is a scientific defense of the Jews against the horrible charges promulgated throughout Europe by the "Jew-baiters." Professor Strack's book is a valuable contribution to the history of religion and psychical perversion.

The Ethics of Tribal Society. E. P. EVANS. *Pop. Sci. Mo.* (New York), XLIV. (1893), 299-307.

A general discussion of the subject with reference chiefly to the Indo-European nations.

On the Delicacy of the Sense of Taste Among Indians. E. H. S. BAILEY. *Kans. Univ. Quarterly*, Vol. II. 1893, 95-98.

Results of tests on thirty-one boys and thirty-six girls, of from twelve to twenty-one years of age, at the Haskell Institute, Lawrence, Kansas. Also twenty-six males and twenty-six females, white, of about the same age as the Indians. The order of delicacy is about the same for the two races. The ability to detect the different substances when they are in very dilute solutions is less in the Indians. The males (of both races) seem able to detect a smaller quantity than the females. In other cases the females have the more delicate organ of taste.

Un Primo Passo alla Pedagogia Scientifica e la Carta Biografica.
GIUSEPPE SERGI. Milano. Roma. Napoli. s.d. 35 pp. 8vo.

Professor Sergi is one of the educators and scientists who are striving to place pedagogy upon a scientific basis of observed facts and phenomena, and to have it proceed according to natural methods. In 1885 he instituted anthropo-psychological investigations in the Italian schools, after the manner of Galton; the pupils being examined on entering schools and when their course was completed. In the city of Rome alone, 2,500 pupils (of both sexes)

were thus examined, making a total of 250,000 separate items of observation. The present pamphlet enlarges upon the importance of such investigations, explains the methods and instruments employed, and gives the blanks to be filled up, but does not indicate the results obtained. Professor Sergi has discussed the same subject in his book, *Educazione ed Instruzione* (see *Pedagogical Seminary*, II. 473), from which this brochure seems to be a reprint.

B. ARTISTIC AND ÆSTHETICAL.

Evolution of the Æsthetic. W. H. HOLMES. *Proc. Amer. Ass. Adv. Sci.*, Vol. XLI. (1892), 239-255.

The address of the vice-president, section H of the American Association, is devoted to the study of the non-essential arts of man, the science of the beautiful, which has to deal with actual phenomena, with facts as hard, with principles as fixed, and laws as inflexible, as do the sciences of biology and of physics. Professor Holmes treats briefly of the æsthetics of the individual, of national and race culture evolution, and discusses the probable order of the development of the various æsthetic arts, which he thinks to be: painting, sculpture, architecture, music, poetry, the drama, romance and landscape gardening.

Indian Songs. Personal Studies of Indian Life. ALICE C. FLETCHER. *Century Illustr. Mag.* (New York), Vol. XLVII. (1893-'4), 421-431.

An investigation, by personal experience, of the general character of the music and poetry of the Siouan tribes, with whom the author is intimately acquainted. There are mystery songs, thunder songs, war songs, choral and historical songs, children's songs, lovers' songs, religious songs, etc. The musical instruments were the flute, whistle, drum, rattle. The following passage is worth remembering: "The native ear is precise as to time; a retard occurs only in the mystery, dream and love songs; in any other a variation of the value of a thirty-second or a sixty-fourth of a beat is sufficient to throw the tune out of gear to the Indian. Syncopation is common, and the ease with which an Indian will sing syncopated passages in three-four time to the two-four beat of the drum is remarkable. One of our own race could hardly do this without careful training and much practice. An Indian's ear is as keen for time as his eye for tracks in the forest."

C. SOCIOLOGICAL (and related subjects).

Le Rire et la Liberté. A. PERIJON. *Revue Philos.* (Paris), XVIII. (1893), 113-140.

Laughter is, in a general way, the sign of liberty—visible liberty, in fact, the *vis comica*, the sense of the ridiculous, the tendency to laugh, belong to every age, to every people; some have more, others less. M. Perijon writes interestingly of the sociological aspects of laughter.

La Logique sociale de Sentiments. G. TARDE. *Revue Philos.* (Paris), XVIII. (1893), 562-594.

A study of the social *Gefühl*. According to M. Tarde, the heart of society is a piano, which, from time to time, gets out of tune, and for centuries would fail to do full justice to any one if there did not appear at long intervals some tuner—apostle, founder of a religion, mystic, great popular reformer. When one of the chords

vibrates no longer, or is dissonant, society is ill. In the ages past, much progress has been made. To be sure, society has still its disturbances and its revolutions, but the old spirit of coterie and clanship with its bloody feuds has given place to the spirit of party, which is surely an advance towards social peace and quietude. The dialectic of social logic consists, therefore, in according and equilibrating the diverse or even antagonistic sentiments, and in substituting for them a system more stable by increasing the proportion of sympathetic sentiments at the expense of the antipathetic, which are bound up with them. The most general fact which the history of human society reveals to us is the continual increase of the social group in extent and in depth; family, tribe, city, state, federated dominion, mark the line of progress. The system of social logic tends ever to base itself upon a maximum of love and a minimum of hate. The author touches briefly on loyalty, democracy, war, glory, religion, social unions, national hatred, class hatred, domesticity, friendship, love, morality and urbanity, amusements, recreations, public festivals. Everywhere he sees the advance of that international spirit, that instinct of common desire, common ideas, common hopes, common beliefs, which are agitating humanity more and more as the years go by.

School Statistics and Morals. W. T. HARRIS. *School Review* (Ithaca), Vol. I. (1893), 218-225.

In this paper the United States commissioner of education tells us what the late census has to say of the relation of education to morals. Dr. Harris thinks that while the claim that the number of convicted criminals has increased must be allowed in face of the facts, the fostering of honesty, truth, temperance, fortitude, thrift, etc., in the schools has had a large share in producing the favorable moral and industrial conditions existing in the state giving the largest amount of schooling to each inhabitant.

Interesting from another point of view is W. Addis' paper: "Ten Years of Education in the United States," *Ibid.*, 339-353. Here statistics of taxation, salaries, attendance are considered.

The Psychological Basis of Social Economics. L. F. WARD. *Proc. Amer. Ass. Adv. Sci.*, XLI. (1892), 301-321.

The author's conclusion is that "the advent with man of the thinking, knowing, foreseeing, calculating, designing, inventing and constructing faculty, which is wanting in lower creatures, repealed the biologic law or law of nature, and enacted in its stead the psychologic law, the law of mind." The old political economy is true only of irrational animals, and is altogether inapplicable to rational man.

The Relation of Economic Study to Public and Private Charity. J. MAVOR. *Annals Amer. Acad. Polit. a. Soc. Sci.* (Phila.), IV. (1893), 34-60.

This is the inaugural address of the new professor of political science in the University of Toronto. Professor Mavor discusses at some length Le Play, who, in 1829, began the series of family monographs, and General Booth, whose life and labors amongst the poor of London are called upon for many illustrations. The use of economic students lies in their investigation and interpretation of conditions and facts. What we need in the study of economics to avail us in practical affairs is insight, insight, and always insight. It should not be said: "You are disobeying the

laws of political economy," but "You are disregarding the lessons of history"—it is mainly from disregarding the plain lessons of history, frequently from ignorance of these, that men go wrong in political action.

L'Education Nationale. FRANCK D'AVERT. Rev. intern. de l'Enseignem., 13me Année (1893), 308-320.

National education, says the author, is education given by the nation; its nature, its sphere, are vast problems of public pedagogy, requiring careful investigation. To arouse and to develop the national conscience in the child is a species of education which belongs peculiarly to the state, and to the state alone. At his birth three concentric circles surround the child—the family, the church, the state. Between the family education, which forms the "enfant de la maison," and the moral (religious or lay) education, which makes of the child a member of humanity, comes necessarily the national education, which makes of the individual a citizen. This last the state alone is fit to give. Upon this topic M. d'Avert writes the rest of his article.

Anthropometry as Applied to Social and Economic Questions. C. ROBERTS. *Humanitarian* (London), III. (1893), 422-429.

After referring to the anthropometric investigations of various races, of children and the sexes at various ages, etc., Dr. Roberts treats of the application of anthropometry socially and economically—the endeavor to determine whether England is stationary, improving, or degenerating physically; the physical conditions of the various classes, etc. The government returns show during the forty years from 1833 to 1873 a decided gain in stature and weight of factory children; the physical condition of men offering as recruits has greatly improved; while the statistics of the Friends' School at York, extending over twenty-seven consecutive years, indicates a like improvement in the better classes of the population.

Geschichte des Armenwesens im Kanton Bern von der Reformation bis auf die neuere Zeit. KARL GEISER. *Ztschr. f. schweizerische Statistik* (Bern), 29 Jahrg. (1893), 532-591.

A brief and interesting sketch of the condition of the poor and their relief in the last three centuries and a half in the canton of Bern.

A Study of Omaha Indian Music. By ALICE C. FLETCHER. Aided by Francis La Flesche. With a *Report on the Structural Peculiarities of the Music.* By J. O. FILLMORE, A. M. *Archæological and Ethnological papers of the Peabody Museum, Harvard University, Vol. I. No. 5.*

When first hearing Indian music, it is difficult to penetrate the noise and hear what the people are trying to express. The noise of their drum affects us as the hammers of the piano do an Indian when their songs are rendered thereon. Below the noise is finally discovered matter worthy of study and record. The first studies were crude and I was more inclined to distrust my ears than my theories. During the investigations, an illness came on. While attended by Indian friends, they would frequently sing softly and with no drum. The beauty and sweetness of the songs were thus revealed. The return of health was celebrated by customary ceremonies and music which bespoke the kind inner life of the Indian. Then I ceased to trouble about scales, rhythm, etc., and

trusted the accumulating facts. The songs of one tribe are frequently sung by others and those far distant, but they are always credited to the tribe to which they belong. Indians are not plagiarists. Among the Indians there is not a phase of life that does not find expression in song. Music is also the medium through which man holds communion with his soul and with the unseen powers which control his destiny.

Songs are handed down through generations of past events, and are retained only by memory. Unlike people who possess written music, and have a device by which a tone can be uniformly produced, the Indian has no pitch or uniform key for a song. It can be started on any suitable note and the intervals preserved. Those having good voices and memories are the music teachers. The Indian enjoys a tremolo and vibrations of the voice. In love songs and some others, he waves his hand to and fro from his mouth to produce pulsations. Comparatively few Indian songs are supplied with words, for they are taken apart and modified so as to make them more melodious. Rhythm of the music demands this. We seem here to come upon the beginnings of versification. We fail to find evidence of the sustained intellectual effort essential to the development of poetic art. Sounds that lend themselves easily to singing are used instead of words, but have no definite meaning. If a composer sets syllables to his song, they are preserved.

A collection of ninety-two songs is given with their music; some have syllables. They are of three groups: class, social and individual. They are very melodious when played and show how they permeated the avocations and beliefs of the Omahas. The accompanying instruments are the drum, rattle and whistle. The words giving only a hint, render it difficult for the unheralded melody to secure our attention before it is finished. These songs—the product of Indian tribal life—suggest the question whether sustained thinking, without which there can be no full expression of thought in music or any other art, is possible in a state of society where labor is not coordinated, where each person stands individually against hunger and mortal enemies. While it is true that evidences of sustained thinking are wanting, these songs show nascent art, both in poetry and music. Whenever one man yearns toward the mysterious unseen powers that environ him and seeks an expression of his personal loves, hopes, fears and griefs, his song will answer in its fundamental directive emotion to that of every other man. This is true of our folk-music, such as the "Mystery Songs," as compared with Indian songs. In comparison with our more modern music, the divergence is upon the intellectual rather than the emotional plane. Our music has gained power by its being written. The eye has reinforced the ear, developing a broader field for musical expression. It is noticeable that there are no labor or guild songs. These originated in a society where labor had become secularized, both in feeling and association, unlike the Indian who directs labor with supernatural influences. As the Omahas, as a tribe, have ceased to exist, and the young people are being educated in English, their directive emotion will hereafter take the lines of our artistic forms. Therefore there can be no speculation as to any future development of Omaha Indian music.

Structural Peculiarities of the Music.

Investigations covered the following points: 1. The scales on which Indian songs are built. 2. The harmonies naturally implied in the melodies of the songs. 3. The tonality of the songs as indi-

cated by melody and harmony combined. 4. Rhythms. 5. Phrasing and motivization. 6. Quality of tone and correctness of intonation. 7. The Indian flageolet.

It was found that: First. The five-toned major and minor scale was used; i. e., the common scale with the fourth and seventh omitted. Second. The Indian was not satisfied with the melody when played without the addition of chords, and whatever was satisfactory to the primitive man was to the trained musician. Also in common with the trained musician, he accepts the common chord as a perfect concord. Third. The question of tonality was often difficult, not to say impossible, to decide from the melody tones alone. Several examples illustrating this are given. Fourth. One of the most notable rhythmic peculiarities of these songs is the grouping of pulses into measures of different lengths. Another is the mixture of twos and threes in the same measure. Rhythm is difficult, but the element most developed in music. Civilized music has not surpassed it. Fifth. There is a rich variety of phrasing. Their spontaneous expression of feeling in tones are within their limits artistic. Nature seems to have taught them "motivization," as our professors of composition teach their pupils. Sixth. Some of the melodies are beautiful. The general impression is that they are not so, on account of noise of accompanying instruments. The high pitch of the voices is distracting. The songs are the expression of excited feeling, and the singers are stirred up almost to frenzy. Strangers, too, have no idea of the meaning and spirit of the music. Many songs are the fervid expression of the Indian's most sacred beliefs and experiences. Much of the music is profound, high and ennobling, and the better it is known, the more this will be seen. If the deficiencies of Indian performance on the side of sensuous music were removed, and a beautiful quality of tone by orchestra or voice were secured, its impression would be improved. Though possibly the accessories are necessary. But these beautiful chorals will certainly always remain the expression of genuine religious feeling, and I doubt not their merit will be recognized. Seventh. The flageolet is a rude instrument of red cedar, evidently built "by guess."

The merits of Indian music consist, first, in an elaborate, well-developed rhythm; second, in fresh, original, clear, characteristic expression of the whole range of emotional experience of primitive people.

The problems presented in this study are two: First. The origin and function of the music. Second. The psychological, physical and acoustic laws, in accordance with which the musical phenomena have become what they are. In answer to the first: These songs had their origin in feeling, and their function is to express feeling. Second. The Indian song is an absolutely spontaneous natural product. What correlation of the mind with the auditory and vocal apparatus, and of these with physical laws of acoustics, determines the course of melody? This suggests numerous questions. For instance, why are melodies based on the five-toned scale? A possible solution is that the harmonic sense is universal. In the Indian the harmonic sense is latent, but his sense of a tonic chord and related harmonies is probably the same as ours.

CORNELIA W. DRESSLAR.

Russian Folk-Songs. A Study in Musical Psychology. J. C. FILLMORE. *Music*, June, 1893.

The Russian folk-songs are wholly spontaneous; the natural product of the free, untrammelled impulses of human nature. The

musically uncultivated Russian, like other primitive men, regards his music as the expression of feeling, of moods and emotions. The national folk-songs reveal the national character. The perception of the musically uncultivated man is completely in agreement with that of the most highly trained musicians as regards the primary relation of music to feeling, its function as the natural means of expressing emotion, and the characteristic types it assumes.

The utmost that music can do, is to express so definitely the emotions naturally arising from an event that, when we are once given a clue, the feelings expressed may suggest the ideas which awaken the feelings. Unlike the primitive music of other races, that of the Russian has no five-toned scale. This is the common major scale with the fourth and seventh omitted. The complete diatonic major or minor scale is used. This implies a stage of development in advance of that in which songs are made up of five tones only, or a natural musical endowment superior to that of most primitive races. The development of the five-toned scale into an eight-toned one is explained, and different theories given. But the dual nature of harmony has not yet been proven.

The theory that the minor chord is due to our perception of the undertone series, depends primarily on the assumption that the minor chord is a perfect concord. Though it is commonly assumed to be so, the fact that the Indians, as well as Bach, will end a minor song with a major chord, will have to be otherwise explained. In singing the minor, he is guided by no preconceived theory. He freely expresses himself. One reason why his effort at spontaneous emotional expression should take on the shape of the successive tones of a minor chord, with a filling in of tones which imply the dominant and sub-dominant chords, is that the five-toned major scale is easy to sing, and the five-toned minor is not only as easy, but is made up of the same tones in the same order. In changing them from a major to a minor key, only the starting point is changed, and not the melody or harmony. Fourteen "Russian folk-songs" are given, including a "love song," "harvest song," "comic song," and others, in Great Russian and Little Russian (Cossack). All are supposed to be characteristic types, and to reveal the state of feeling which prompts each song. C. W. D.

II.—NEUROLOGY, MORBID PSYCHOLOGY, INSTINCT.

By ASSISTANT PROFESSOR C. F. HODGE.

The Nerve Cell Considered as the Basis of Neurology. A. E. SCHÄFER. *Brain*, Vol. XVI. pp. 134-169. 18 Figs. in text. 1893.

An important point in the terminology of the subject is first to receive attention. In 1891 Waldeyer proposed the term neuron to designate the anatomical unit, cell-body with processes attached, of the nervous system. Schäfer insists that just as we include processes with the cell-body in all other tissues, so here the term nerve-cell should be held to its primitive meaning and cover body and processes. Waldeyer's term "neuron" Schäfer appropriates to designate the axis-cylinder process. Protoplasmic processes are given the appropriate name of dendrons. With these terms clear, it becomes possible again to classify nerve-cells intelligently. "All possess at least one neuron." They may be dendritic or adendritic,

mononeuric, dineuric or polyneuric. For the branches of neurons, Cajal's term, collaterals, is adopted. While thus distinguishing between these two kinds of processes, the author frankly states that "it is impossible to say positively that there is any essential difference between the neurons and dendrons." Neurons and dendrons end finally alike in terminal arborizations; and this fact is taken to support the view that the structure of the axis-cylinder is fibrillar. The length of a neuron before breaking up into its terminal arborization has proved serviceable for purposes of classification. Upon this distinction Golgi based his classification into "motor" and "sensory." Objections have accumulated against this classification to such an extent that the author deems it necessary to substitute for Golgi's cell of the first type projection-cell, and intermediary-cell for "central," or cell of the second type.

The more important views advanced in the paper may be gathered from an abbreviation of the seven conclusions: 1. "That every nerve-cell forms a structural element which is anatomically isolated from, but in physiological continuity with other nerve-cells." 2. "That the physiological continuity of these elements depends on the contiguity either of the ramified cell processes of different nerve-cells with one another or of the ramified processes of one cell with the body of another cell." 3. "That the same nerve-impulses do not necessarily pass from one element of a nerve-chain to the next, but that more probably new impulses (often of different rhythm) are generated in the successive elements of the chain." 4. The converse of 3. 5. "That either the body of the cell or any of its processes may be concerned both with the starting and with the transmission of nerve-impulses; and, that these may originate by acts of contraction, causing waves of pressure or variations of surface-tension to traverse the fibrils." 6. "That the body of the cell is especially concerned with presiding over the nutrition of the whole cell-element; this trophic function being intimately associated with the presence of the nucleus. Nevertheless nerve-impulses may both originate in and be conducted by the cell-body. The dendrons or protoplasmic processes, being extensions of the protoplasm of the cell, may primarily serve to assist in the nutritive processes, as was supposed by Golgi, but they undoubtedly also, like the cell-body itself, may in some cases convey nerve-impulses." A. 7. "That the ordinary centrifugal paths are blocked for centripetal impulses, although the centripetal paths may convey centrifugal impulses, this physiological difference being correlated with a difference of anatomical relationship at the junction of the respective cell-elements."

The figures are culled from best sources, new and old, from M. Schultze, Ranvier, Cajal, Retzius, Lenhossek; and these are supplemented by original diagrammatic compilations, which add clearness to the subject.

Zur Frage über den Bau der Nervenzellen und über das Verhältniss ihres axencylinder (Nerven) Fortsatzes zu den Protoplasmafortsätzen (Dendriten). A. S. DOGIEL. *Archiv für Mikroskopische Anatomie*, Bd. 41, S. 62-87, Taf. IX. and X. Bonn, 1893.

Zur Frage über das Verhalten der Nervenzellen zu einander. A. S. DOGIEL. *Archiv für Anatomie und Entwicklungsgeschichte*, 1893, S. 429-434, Taf. XVI.

These papers are the latest in a series of six which have appeared from the above writer since 1888. Their chief interest in the present connection attaches to the strong evidence which Dogiel has

been able to bring forward for the direct anatomical continuity of nerve-cells through their dendrons. Nerve-cells, as he finds them in a number of retinæ of different animals, are not isolated elements, as is generally taught at present, but cells usually of similar types are joined by their dendrons into cell-colonies. Further an axis-cylinder may arise in three ways: a, from the cell-body direct; b, from the network formed by branching of neurons; c, from the network of dendrons. The method employed, methyl blue staining, exhibits a difference between neurons and dendrons similar to that by the Golgi method. But since fibrillæ from these processes may unite to form an axis-cylinder, they must be unconditionally considered to be of "Nervennatur." Thus far the author's work has been confined to the retina; but there is no good reason for supposing that relations of cells are different here from their relations in other parts of the central nervous system. This work, therefore, if confirmed, must negative the accepted doctrine of isolated cell-elements.

A Physiological, Histological and Clinical Study of the Degeneration and Regeneration in Peripheral Nerve Fibers after Severance of Connections with the Nerve Centers. W. H. HOWELL AND G. C. HUBER. *Journal of Physiology*, Vol. XIII. 1893, pp. 335-406; Plates XII. to XVI.

The above paper was awarded a prize offered by the American Physiological Society for the best essay upon the subject. The chief object of the research was to test experimentally the possibility of "union by first intention" of a nerve severed from its central connections; together with a thorough study of histological steps in processes of degeneration and regeneration. Dogs were used in all but one experiment, which was made upon a rabbit, and the ulnar and median nerves were either cut, cut and sutured, crushed by a ligature, immediately loosened, or coagulated by contact with a tube, through which water at 80° was allowed to flow. The first result to be noted is that in no case did "union by first intention" take place. In all the experiments degeneration of the peripheral end was complete through its entire length. Certain authors have described experiments in which both sensory and motor functions became re-established in a severed nerve in a few hours. In these experiments the least time in which irritability began to return to parts peripheral to the cut is twenty-one days; and at this time regeneration is found to have progressed some distance beyond the wound. Sensory nerves regain function before motor. Both sensory and motor function is found to be imperfect at the end of seven weeks and nearly normal by the end of eleven weeks.

The histological evidence as to the processes concerned has been made quite complete and is well illustrated by seventy-six figures. This evidence favors the view that embryonic fibers form in the distal nerve and subsequently unite in the cicatrix with the axis-cylinder as it grows out from the central end.

Histogenesis of the Retina in Amblystoma and Necturus. F. MALL. *Journal of Morphology*, Vol. VIII. pp. 415-432; 12 Figs. 1893.

The above paper fills a long-felt need by giving in a brief form a clear orientation of the layers and elements of the retina. Two principles of universal application to the growth of nerve tissue are stated at the outset. These are: 1. "The primitive growing point of all vetebrate nerves is in the layer of cells on the outermost side of the ectoderm, and the axis of division is parallel

with the ectoderm." 2. "The direction of transmission of an impulse is already determined by the position of the cell in the ectoderm." That is, the receiving side of a cell is the one originally toward the surface, while the giving pole is turned toward the interior of the body. Under these principles, and keeping in mind the formation of cerebral and optic vesicles, primary and secondary, it is made perfectly clear why the optic nerve fibers should grow first toward the vitreous chamber and afterwards pierce the retina in order to reach the brain. This also explains the inversion of the rod and cone layer, these elements being the receiving poles and the line between them, and the pigment layer of the retina being the original external surface of the body. The well chosen cuts render this intricate problem doubly lucid.

On the Method of Transmission of the Impulse in Medullated Fibers.

E. R. EDES. *Journal of Physiology*, Vol. XIII. p. 431.

Experiments described in this paper were made in the physiological laboratory of the Harvard Medical School under the direction of Dr. Bowditch, and results confirm in the main that author's previous work upon the non-fatigability of nerve fibers.

The method employed consists in using the action current as a measure of the nerve impulse. This is read by means of a delicate capillary electrometer. The muscle was retained, and although not used to measure the impulse, gave a fine comparison of muscle and nerve fatigue. This is expressed in two charts (p. 437), both of which show that the muscle tires rapidly for the first few minutes, then more slowly and finally very slowly; the nerve on the other hand practically holds its own. Up to five hours' continuous stimulation, the action current suffered no diminution. That this is not true for longer periods was due to trouble with the electrometer. Experiments let run over night (11-14 hours) showed an action current of about one-fourth the original strength. According to Maschek, when such diminution occurs on cutting the nerve off so as to place a fresh cut section on the electrodes, the action current returned to normal. This was not the case with Edes' experiments. Herzen's strychnine experiments were also repeated on rabbits and frogs, the conclusion therefrom being, contrary to Herzen's, that the "exhaustion obtained could be located wholly in the muscle."

In a short addendum are summed up the results of a number of experiments made for the purpose of repeating Demoor's recent work upon the action of silver nitrate upon normal and exhausted nerve fibers. Demoor's statement is that "Frohmann's striations" are not found in exhausted nerves. The experiments of Edes gave the impression that stimulation "does make some slight difference in the behavior of the nerve fiber towards nitrate of silver."

Der Hund ohne Grosshirn. Siebente Abhandlung über die Verrichtung des Grosshirns. F. GOLTZ. *Archiv. für die gesammte Physiol.* Bonn, 1891, 2 Bd. LI. S. 570-614. 1 Taf.

This paper forms the strongest protest yet uttered against the doctrine of cerebral localization, so far at least as the dog is concerned.

Goltz gives us the results of removing the entire cerebral cortex (except a mere shaving of the inferior temporal lobes, left to protect the optic tracts) in three dogs. The first lived fifty-one days; the second, ninety-two days; the third lived eighteen and one-half months. In order to more fully meet the arguments of his opponents, the operations were performed with the knife.

Since results were uniform, mention of the third case will suffice.

Upon June 27, 1889, the left hemisphere was removed. The right hemisphere was similarly excised a year later, June 17, 1890. The dog, in general, continued in good health, and was killed December 31, 1891.

Three days after the second operation, the dog could walk without help. Subsequent tests demonstrated that hearing was present in some degree, the animal being awakened by the blast of a horn. He also reacted to light, and was found to be sensitive to touch and pain in all parts of the body. Even the presence of smell, Goltz seems to consider, admits of question, since this could not be satisfactorily tested. The animal sneezed when tobacco smoke was blown in his face. He could taste, as was evinced by his refusing, with every expression of disgust, meat which had been rolled in quinine. The same meat was similarly rejected by his own dog on first tasting, but was subsequently gulped down "out of politeness." A brainless dog does lack politeness, as the author humorously adds.

Two points are of special interest to brain physiology in general. The first of these is that this dog required much shorter periods of rest or sleep than normal animals; and also became more quickly fatigued. This leads to the second point, which is that if over-excited or over-tired, the dog is likely to be thrown into a fit of epilepsy (p. 591). That an animal deprived of all motor cortex can exhibit typical epilepsy, is certainly revolutionary to post-Jacksonian ideas of the cause and origin of epileptic fits.

The brain was turned over to Schrader for examination and description. Dorsal and ventral views are given in the plate.

The Arrangement of the Sympathetic Nervous System, Based Chiefly on Observations upon Pilo-Motor Nerves. J. N. LANGLEY. *Journal of Physiology*, Vol. XV. pp. 176-244. Plates VII.-IX. Sept., 1893.

Reactions of the hair muscles are found to be of great service in determining the course of sympathetic fibers from the cord, through the sympathetic ganglia to their distribution in the skin. In brief this course is found to be the same as that of vasomotor and secretory fibers; viz., out of the cord by the spinal roots, through the white rami to the sympathetic ganglia, from this back to the spinal nerves, by the grey rami, and finally along with the cutaneous nerves to the skin. In the cord pilo-motor nerves are shown by properly graded stimulus to lie in the lateral columns; and their course out of the cord is entirely by the anterior roots. By the nicotin method, injection of ten milligrams into a vein, for the cat, it was demonstrated that all pilo-motor fibers are interrupted by cells in the sympathetic ganglia in passing through them to the skin. Distribution in the skin is found to coincide with that of the sensory nerves. It is unilateral, overlapping the mid-line very little, if at all; and successive grey rami supply successive sensory areas, generally quite sharply defined. A minute's description of relations of skin-areas to the different nerves is given for the cat, and the paper closes with deductions therefrom as to the arrangement of the sympathetic system in man.

On Disturbances of Sensation with Especial Reference to the Pain of Visceral Disease. HENRY HEAD. *Brain*, Vol. XVI. pp. 1-133. Plates I. and II. 42 illustrations in text. 1893.

A convenient paper for reference upon distribution of sensory nerves in the skin, aside from its main purpose. Areas for touch supplied by the spinal nerves have been shown by Sherrington to

overlap considerably; whereas, according to our author's observations, areas for pain, heat and cold do not overlap perceptibly. They correspond closely to the area of trophic influence supplied by each spinal nerve, these latter being indicated by areas of eruption in herpes zoster. In connection with disease of any visceral organ, disturbances of dermal sensations for pain and temperature are likely to arise over sharply defined areas. Pain in these cases is projected peripherally by allocheiria, i. e., pain in an insensitive portion, e. g., a viscus, being projected to a more sensitive part, the skin, supplied from the same segment of the spinal cord. The present paper deals with arrangement of nerves and skin-areas below the clavicles. The author promises a paper in the near future to cover the region of head and neck.

Untersuchungen über die Entwicklung der Area und Fovea centralis retinae. J. H. CHIEVITZ. *Archiv für Anat. u. Entwicklungsgeschichte.* 1890, pp. 332-365. Plates XVIII.-XX.

Development of retinal elements, especially in the region of the area or fovea centralis, is outlined in four species of bird, viz., crow, finch, domestic pigeon and one of the gulls, *sterna cantiaca*, in one lizard, *lacerta vivipara*, and in a teleost, *sygnathus typhle*. The rabbit possesses no fovea proper, but an area centralis, "streifenförmig," which extends horizontally through the entire retina just below the entrance of the optic nerve. All the birds were found to have a central fovea well developed, and in the gull two foveas were demonstrated, a nasal and temporal, and in addition a "streifenförmige" fovea, which the author does not discuss. The lizard has no fovea, but a circular area centralis situated just above the optic papilla. A "punctförmige" fovea was demonstrated in *sygnathus* located caudad of the optic papilla, somewhat nearer the papilla than the ora serrata. The greater part of the posterior half of the retina is modified into an expanded area centralis having the fovea in its center. The fovea assumes its special characters late in embryonic life.

Untersuchungen über den electrischen Leitungswiderstand der thierischen Gewebe. K. ALT, AND SCHMIDT. *Archiv f. d. ges. Physiol.* Bd. LIII. S. 575. Taf. 13.

Recent work upon this subject has given currency to the idea that the fluids contained in a nerve cause its electrical resistance to be about that of the blood or lymph. The above paper tends to bring us back to the notions of the physiologists who wrote before it was demonstrated, that a nerve impulse is not an electrical current.

The method employed consisted in placing a given length of tissue in the circuit, composed of semi-circles of zinc and copper; contact completing the circuit on the other side being made by a micrometer screw. The zinc arc was connected to a friction machine, the copper with a water-pipe. The electricity generated could thus go to ground either through the tissue or through the micrometer screw. By manipulating the screw it was thus possible to measure the length of the spark, and this was taken to indicate the resistance.

Results of experiments on a large number of organs are given in a table at the end of their article. The following figures are extracted:

Tissue.	Resistance.	Tissue.	Resistance.
Nerve	0.17	Brain	1.57
Muscle	1.00	Tendon	3.25
Blood	1.00	Bone	14.10
Skin	1.25		

The Industries of Animals. FRÉDÉRIC HOUSSAY. Pp. 258, 44 illustrations. London, 1893. English edition revised and enlarged with author's coöperation.

The especial *raison d'être* of this book lies in the parallelism which the author continually holds in mind between the industries of man and the industries of animals. Men placed before a given problem, the attainment of a definite end, have come to act in a certain way; before the same problem, animals from insects to apes proceed in much the same manner. Men hunt in ambush, dig pit-falls, arrange concealed traps; so do beetles, spiders, foxes and cats. Through the whole range of human activity the same is true. The sphere of action may be very small for any animal compared with that of man; but within this narrow sphere the animal solves his problems in general as man would under like circumstances.

What man does intelligently certain writers would insist animals do instinctively. But this distinction is breaking down on all sides. Houssay's view of the relation between instinct and intelligence is clearly expressed in a few words. Instinct cannot be regarded as the "rudiment of intelligence," as is often done. It is rather the essence of intelligence, intelligence "condensed and accumulated" from generation to generation. As actions laboriously learned become reflex and habitual with man, so do adaptations on the part of animals acquired by "reflection, sagacity and intelligence" become by natural selection the common stock of knowledge, the instincts of a species.

Animal industries are grouped under six heads, treated in as many chapters, beginning with the simplest and most primitive, "hunting, fishing, wars and expeditions," and closing with Chapter VII: "The defense and sanitation of dwellings." Chapter VIII is devoted to conclusions. The subjects relating to dwellings, provisions and domestic animals, rearing of young, of course receive their share of attention. Some striking instances are cited. Of the many, I will note a single one (p. 49): An ant is observed to abandon its burden at the foot of a little hillock, over which she has tried in vain to lift it. She soon finds a comrade, also carrying a load; the two consult by means of their antennæ, and both start in the direction of the hillock. On reaching the spot, ant No. 2 lays down her burden, "and both together then seized a twig and introduced its end beneath the first load, which had been abandoned because of its weight. By acting on the free extremity of the twig they were able to use it exactly as a lever, and succeeded almost without trouble in passing their booty on to the other side of the little hillock." The above is given on the authority of Parseval-Deschênes, Paris, 1848. Many other examples are nearly as striking.

The key-note of the book is again struck in a concluding sentence.

"The industries in which the talents of animals are exercised demonstrate that, under the same environment, animals have reacted in the same manner as man, and have formed the same combinations to protect themselves from cold or heat, to defend themselves against the attacks of enemies, and to ensure sufficient provision of food during those hard seasons of the year when the earth does not yield in abundance."

- Ueber Gesichtsfeld-ermüdung und deren Beziehung zur concentrischen Gesichtsfeldeinschränkung bei Erkrankungen des Centralnervensystems.* WILHELM KÖNIG. pp. 152, 13 Figs. Leipzig, 1893.
- On the Physical Nature of Hysterical Unilateral Amblyopia and Sensitivo-sensorial Hemianæsthesia.* BERNHEIM. *Brain*, 1893, pp. 181-90, 4 Figs.
- On the Visual Path and Center.* S. E. HENSCHEN. *Brain*, 1893, pp. 170-80.

A discussion of the psychical side of fatigues and defects of sight, which are usually ascribed to the optical mechanism, finds a suitable preface in the briefest possible outline of the cerebral tracts and centres concerned in vision. The present short paper by Henschen would seem to be the essence of this author's work, "Beiträge zur Pathologie des Gehirns," and may furnish such an outline so far as this field is concerned. The visual path is divided into three portions respectively: the frontal, optic nerves, chiasma and tracts; the middle, the ganglia of the brain-stem with which fibres of the optic tracts connect; and the occipital, the course of visual nerves in the cerebral hemispheres. In following this tangled mesh of connections a distinction must be made between "visual" fibers and "optical" fibers. Optical fibers serve as reflex paths, but have no visual function. Of the three great central ganglia into which the optic tracts flow, in the pulvinars and the anterior corpora quadrigemina, the external geniculate bodies, the last are "the main sight ganglia in man." This is the result of the author's analysis of clinical cases. Destruction of one external geniculate body invariably causes hemianopsia. The occipital portion of the visual path is more difficult to determine. Analysis of all clinical cases bearing on the subject leads Henschen to place the visual path in the "optic radiation of Gratiolet," in a bundle of fibers less than a centimeter thick, lying at the level of the second temporal gyrus and sulcus. Lesion of parietal lobes induces disturbances of vision only if this bundle be compressed.

The visual centre Henschen would limit to the calcarine fissure, which he would like to call the "cortical retina." Other portions of the occipital lobe so often and persistently included in the optical centre, possibly have functions closely associated with vision. Word blindness would indicate this. As to the organization of the visual centre, a single case, that of Hun, is taken to prove that the upper lip of the fissure represents the upper retinal quadrant. Clinical evidence is considered to support Wilbrand's conclusion that the macula is innervated by both hemispheres. Those afflicted with hemianopsia always retain vision at the point of fixation.

Bernheim demonstrated, in 1896, that subjects of hysterical or suggested amblyopia unconsciously neutralize their correct visual images by an act of mind. "They see with the bodily, not with the mental eye." A most instructive case by way of further demonstration is the one now reported, that of a youth of nineteen, who came out of an attack of influenza nearly hemianæsthetic on the left upper half of the body. Amblyopia of the left eye with dichromatopsia were prominent features. With this eye he cannot see a finger held before the face, and white and blue are called red; yellow, blue, and red, gray. Yet, with a prism held before the right eye, he sees two images, and tested with Snellen's (Stöben's modification) apparatus, he sees all six letters and recognizes their colors. Further, when sent to an oculist, whose glasses throw him off his guard, he is able to read perfectly well with the blind left eye alone. The visual fields are contracted, the left more than the right, as is usual in hysteria. A cure is sug-

gested and both return to nearly normal. The left ear and nostril were likewise affected and it was possible to show that here, too, the difficulty was altogether psychic. With eyes closed the patient could not find his left hand with his right. Hypnotized and given the suggestion that his right hand was a magnet, the hands came fairly together. All the above symptoms and tests demonstrate that all the disturbances of sensation, special and common, were of cerebral origin; "a disease of the conscious æsthesodic cells," is the way the author expresses it. Three weeks' psychical treatment restores all functions.

König's paper is, for the most part, a careful, detailed statement of clinical cases. For testing the fatigability of the visual field, he used Wilbrand's simplification of Förster's method, the perimeter tests being made only in the horizontal meridian. In all, seventy-four cases were examined, in which contraction or fatigue of visual field was demonstrated. The result of chief interest at present in the present connection is the conclusion which he reaches, viz., that visual fatigue is probably of retinal origin; while contraction of visual fields is to be considered, at least in a number of the cases, as depending upon functional disturbances of the cerebral cortex. This corresponds in the main with Wilbrand's results and with the findings of Pflüger and Schiele.

III.—EXPERIMENTAL.

On a Photometric Method which is Independent of Color. O. N. ROOD. *American Journal of Science*, XLVI. Sept. 1893, 173-176.

To determine the luminosity of a color in terms of gray or any other color by ordinary photometric methods is by no means easy, very slight differences in color making comparison more or less uncertain. The method proposed by Professor Rood has the advantage of great simplicity and does away entirely with the need of comparing the colors in the ordinary sense. It depends upon the observation that when a colored disk is combined on the color-top with an equally luminous gray disk, no flickering is to be seen, even with slow rotation, while, if a difference in luminosity of two, or even of one per cent., is present, a flicker can be detected. When the flicker is absent the colors blend in "a soft, streaky way." A test of the method, made by measuring separately six disks, (forming three complementary pairs) and calculating the brightness to be expected from combining them, and then actually making the combination, resulted as follows:

	Observed.	Calculated.	Difference.
Purple and green.	27.5	27.5	.0
Red and blue-green.	20.2	21.1	.9
Yellow and blue.	27.85	29.1	1.25

The method is equally applicable to comparing two colors or two grays. A considerable series of grays is necessary for making the original determination (the author used 100), but when a few standard disks of bright color have been accurately measured, other disks can be measured by matches built up with these standards and black and white. It is, as the author observes, "a matter of some interest in physiological optics to know that the sensation called 'flickering' is independent of wave length and connected with luminosity."

E. C. S.

Experimentelle Untersuchungen über die Helligkeit der Farben.
 EDUARD GRUBER. Wundt's Philos. Studien., IX. 1893, 429-446.

The method used by Gruber for determining the relative brightness of colors was the natural one of direct comparison, applied, however, in the form of the method of minimal change. Two color-tops, one carrying a gray disk, i. e., a black and white one (*A*), and the other a colored disk (*B*), were set up before a dark background. The gray disk (*A*) was at first set decidedly too dark and gradually brightened till it seemed to the observer of equal brightness with the colored disk (*B*). The condition of *A* was noted and then made much too light and gradually darkened till a match was again reached. The average of the two determinations, repeated several times on each side, gave the brightness of the color. The experiments made in this way, after a little practice, yielded excellent results, except in a few cases where the observers seem to have become habituated to a particular intensity of gray and to have judged by its return rather than by an unbiased comparison. For the special precautions employed the original must be consulted.

The author also investigated the Purkinje phenomenon and the effect of changes in saturation on brightness. The experiments on the first, so far as they went, gave results in accord with the similar experiments of Hillebrand. With decreased illumination, the blue and green not only lost less in brightness, as compared with the red and yellow, but less even than the neutral gray. The effect of change in saturation was tested by replacing a portion of the color on *A* by an equally luminous gray, but no change in the total brightness was to be observed. Experiments on a color-blind observer (red-green blind), made in the hope of deciding whether green appeared to him relatively brighter than it did to an observer with a normal eye, which might be expected on Hering's theory, unfortunately had to be discontinued before clear indications appeared. It is, perhaps, only fair to add that the same method of measuring the brightness of colors was used by Rood more than fifteen years ago; see *American Journal of Science*, Ser. 3, XV. 1878, 51-52. E. C. S.

On a Color System. O. N. ROOD. *American Journal of Science*, Ser. 3, XLIV. 1892, 263-270.

In this article Professor Rood describes a method of working out a reproducible color system with the color-top, provided that there is at hand a single disk of known hue and power of saturation, i. e., efficiency in the formation of gray when mixed with its complement. By combination with the standard disk, the power of saturation of its complement and of colors differing but slightly from that, are determined, and from these in turn the powers of saturation of other disks, till a considerable range in the color scale has thus been measured. When three colors widely enough separated to form the corners of a color triangle have been reached, such a triangle may be constructed in the usual way from the equation giving their mixture for gray, taking into account also their power of saturation, as already determined. This forms the basis of the system and other colors are assigned places in it in the usual way. For the details of the method and the discussion of the nature of the system thus constructed, the reader is referred to the original. The author unfortunately does not specify how the *hues* to be placed at the corners of this color triangle are to be selected, whether by reference to the spectrum, to well-known pigments, or in some other way—an omission that would have to be supplied before any-

one could exactly reproduce the color triangle described, though the author's generous offer of samples of colored paper with coefficients of saturation determined, might for the present supply this defect.

E. C. S.

Eine neue Theorie der Lichtempfindungen. CHRISTINE LADD FRANKLIN. *Zeitschrift für Psychologie*, IV. 1892, 211-221. This paper is a full statement of matter presented in abstract at the International Congress of Experimental Psychology in London, 1892. The author's abstract will be found in the Proceedings of the Congress, pages 103-108, also in the *Johns Hopkins University Circulars*, June, 1893, and in *Science*, July 14, 1893.

On Theories of Light Sensation. CHRISTINE LADD FRANKLIN. *Mind*, Ser. 2, II. 1893, 473-489.

To propose a new theory for matters so long and carefully studied as those of physiological optics is a considerable feat, but one that Mrs. Franklin has accomplished with such success as to receive friendly notice in the address of the president of the British Association (*Nature*, Sept. 14, 1893, p. 469). The author's own abstracts are so accessible that no summary of her theory need be given here. Suffice it to say that, like all the better modern theories, it has been given a photochemical form. Two visual substances are assumed in the retina, one whose decomposition yields the stimulus for white (sensations of the black-gray-white series) and another whose decomposition is different for different kinds of light, giving by partial decomposition the stimuli for red, green and blue, and by complete decomposition the same decomposition-product as the first visual substance, and thus also the white sensation. How the theory fits with various classes of facts is set forth in the original, together with the chief difficulties in the current theories of Helmholtz and Hering. The theory most resembling this of Mrs. Franklin's is that of Donders, by whose this was in a measure suggested. Completeness is too much to expect in an account that the author herself regards as tentative, and some gaps may have been purposely left to be filled hereafter. Something certainly should be said with reference to black, and the explanation of simultaneous contrast will need radical revision.¹ A great advantage of the theory is that it makes the phenomenon of complementary colors a matter of retinal chemistry, instead of a matter of mingled sensations or of opposing anabolic and katabolic processes. Its assumption of three primary colors enables it also to avoid the difficulties that color-blindness offers to four-color theories.

E. C. S.

Grundzüge der Physiologischen Psychologie. WILHELM WUNDT. Vierte umgearbeitete Auflage. Engelmann, Leipzig, 1893. Two vols., pp. xvi. 600, and xii. 684.

In this fourth edition, Wundt's standard work has received a general revision and an increase of nearly 180 pages, of which two-thirds is in the second volume. The main changes specified by the author, aside from such as were needed to bring the work abreast of present information, have been in the way of greater explicitness in the description of psycho-physiological methods and apparatus, and many new cuts of apparatus have been added. These changes will make the work more necessary than ever to the many laboratories now getting under way. The value and convenience of the

¹The reviewer understands that this matter has already received the author's attention.

new edition would have been increased if the parts newly added had been indicated, as in the new edition of Helmholtz's *Optik*.

Experimentelle Beiträge zur Untersuchung des Gedächtnisses. Von G. E. MÜLLER und F. SCHUMANN. *Zeitschrift f. Psy. u. Phys. d. Sinnesorgane.* Bd. VI. 2, 3, 4 and 5. 1893, 192 pages.

According to the modest statement of the authors, the aim of these series of experiments, which have been made at intervals from 1887 to 1892 inclusive, is not so much to add a number of important and interesting facts to the science of memory as to test and develop the experimental method introduced by Ebbinghaus.

While it is perhaps possible to summarize briefly the numerical results obtained, only a slight suggestion can be given of the praiseworthy spirit of experimental carefulness and thorough criticism in which the work has been done. The original must be referred to also for all details, since only the general plan can be mentioned. There are in all thirteen series of experiments, each usually extending over several months.

The procedure in the first two experiments resembles that of Ebbinghaus, except that the syllables are read off through a slit from a revolving drum, and that the experimenter and the subject are different persons. This mode of presenting the syllables was retained to the end and has the advantage of permitting only one syllable to be seen at a time, so that we have to deal with successive association exclusively. The rate with which they are presented can be easily regulated and the experimenter controls the correctness of the repetitions. Certain irregularities were observed in the nonsense material, and a new plan of constructing it devised for subsequent experiments.

The seventeen initial and twelve end consonants, and the twelve vowels and diphthongs used were written on cards and placed in three boxes. A syllable was made by taking one card haphazard from each box. Since they only used twelve syllable series, this method enabled them to construct what they call normal series, each of which has the following properties: All initial and end consonants and vowels are different. The initial consonant is not the same as the end consonant of the preceding syllable, or the end consonant of the second syllable of the same measure. Successive syllables do not form familiar words. Repetition of the same syllable is avoided till after a considerable time. All series, whether original or derived, are normal. Various ingenious devices make the otherwise laborious task of bringing this about comparatively easy.

The number of readings required for the first correct repetition is taken as the measure of the work done.

The central value as well as the arithmetical average are given for any given set of observations. The errors of psychological measurements are asymmetrical with respect to the average. There is much greater possibility of making large positive than large negative errors. The central value, which represents that number in a series of observations above and below which an equal number of records is to be found, is accordingly smaller than the average and represents more nearly the most probable observation.

Experiments I. to V.—A series of nonsense syllables is naturally read in a certain rhythm, preferably trochaic. The problem of the experiment was to see whether the associative bond was stronger between syllables of the same measure than between the adjacent syllables of different measures.

Except in the first two experiments, which were preliminary, a careful allowance is made for the various factors which might influence the rate, such as practice, fatigue, unconscious association, position, and interference of association.

This was done by learning comparison series which are influenced by all the factors the test series are, except the one to be tested.

In experiments III., IV. and V., six new or original series were learned each day, and the six derived series constructed from them were learned after twenty-four hours.

The derived series consisted of two comparison series, two series made up of couples belonging to the same measure, and two of couples belonging to different measures. The result shows that the association between syllables of the same measure is decidedly stronger than between adjacent members of different measures, but that there is a weak association in the latter case also. The experiments were made upon the authors and two other subjects. Subject M. gives the following central values: comparison series, 16.4; series with couples of the same measure, 11; of different measures, 14.5.

Experiment VI. was undertaken to study the strength of association between two syllables separated by a third from one another. Ebbinghaus' results showed that the associative bond extended not only to the next, but to the 2, 3, 4 and 8" syllables. The writers doubt the validity of Ebbinghaus' conclusion that association extends to the 8", since the small saving of 3.3% may be accounted for by some of the factors which influence the rate of learning, like unconscious preparation and association, or the absolute position of the syllables. Their method excludes Münsterberg's objection that these associations might be due to simultaneous impressions, since only one syllable is seen at a time. The derived series differed from those of Ebbinghaus, which had the syllables to be tested adjacent. Every other syllable of the original series was removed and a new one put in its place, in one case the odd or accented, in the other the unaccented. The result of the experiment is, on the whole, not satisfactory, since the comparison series are not in every respect but the one to be tested equivalent to the test series, but have less positions in common with the original series than these. The conclusion that there exists an association of measurable strength between every other syllable of a series once learned is, however, quite probable. Subject S. gives the following result: comparison series, 14.2; test series, 12.1 and 12.3 for accented and unaccented syllables respectively.

Experiment VII. shows that the absolute position of a syllable in a series has an associative tendency. For subject H.: comparison series, 9.6; test series, 7.7. As before, the syllables of the test series have the property to be tested; while it has been excluded from the comparison series.

Experiment VIII. shows that learning series with an iambic rhythm takes longer than with the trochaic; and that relearning a series takes longer with a different than with the same rhythm.

Subject H.: original series trochaic, 18.5; iambic, 20.; in relearning, trochaic after trochaic, 7.8; iambic after trochaic, 10.; trochaic after iambic, 9.; iambic after iambic, 7.4.

Experiment IX. tests the question whether there is an association from a syllable to the one immediately preceding it.

Only syllables belonging to the same measure were tested. Ebbinghaus, it will be remembered, found a saving of 12.4%, due to the association of a syllable with the one immediately, and 5% to the one second preceding in a sixteen syllables series relearned after twenty-four hours.

et Sch. gives for comparison series, 13.5; for test series, there is therefore a slight backward association. As the remark, this may be due to the fact that the two syllables are treated by the mind as a group.

Experiments X. and XI. were designed to test Ebbinghaus' thesis that association of syllables can take place when they are properly stimulated in the proper order without actually appearing to be conscious. The saving in time he found in learning the first of two derived series when its syllables had been suggested in proper order by the first, might be due to a certain preparation and not to actual association. There is a small probability that there is actual unconscious association.

Experiment M. gives for comparison series, 13.62; for test series, 13.12. Experiment XII. is concerned with the interference of association, but gives no definite results and the work is still in progress. An observation seems to show that it is an influence of considerable importance, especially in learning new series.

According to experiment XIII. the first and second half of a series are learned equally well. A recent objection to the method, that repetitions and readings can't very well be coordinated is not since the first part, which receives relatively more repetitions, is not learned either better or worse.

The authors call attention to the importance of rhythmic groupings for retention. The mind treats the syllables as units and their combination into measures as units of higher order. These again are to be divided by caesural pauses into groups of still higher order.

The experiments may be taken as a demonstration of the general assertion that words are known as wholes.

There are several other matters of interest which space will not permit us to enter upon, such as the discussion of individual characteristics of memory, the tests for the various types, and the function of attention in such experiments.

BERGSTRÖM.

die Beeinflussung einfacher psychischer vorgänge durch einige pharmazeutische mittel. DR. EMIL KRAEPLIN. 1892, 258 pages.

In this book the author summarizes the results of his well-known investigations, which were begun in Wundt's laboratory in 1882, and the influence of drugs on mental processes, and gives an account of some later experiments by improved methods. The discussion is very full, since the aim is not simply to present certain results but to develop an experimental method for the study of these similar questions. The introductory chapter on methods is especially of great interest. The measurement of mental processes is especially liable to error. Not only does the general mental and physical condition influence the results, but fatigue and other causes cause variations in the tests themselves. Such sources of error can only be taken into account by the most careful criticism and laborious repetitions of the experiments.

The chronoscope or "intermittent" method of studying mental processes, which was employed exclusively in the older experiments, is used in the later only where the aim is to study the rate as well as the quantitative changes of associations. In this exception, the continuous method was used in all the later experiments and gives by far the most satisfactory results. It was first employed by Oehrn, at the suggestion of Prof. Kraepelin, for studying individual differences in rate, practice and fatigue. The subjects in the recent experiments with alcohol and fatigue, with one exception, the same as those for Oehrn's

experiments. The normal records obtained in the latter are used for comparison with those obtained under the influence of drugs. The tests employed were reading, adding and learning twelve-place numbers. The subjects read, added or learned, two hours continuously, marking off every five minutes the amount done. The averages of the records for every fifteen minutes, give the picture of the changes the processes undergo. In some experiments twenty, in others thirty, grams of alcohol were taken at the end of the first half-hour. The changes afterwards in comparison with the records of the first half-hour and those of the normal series are attributed to the influence of the drugs.

The Hipp chronoscope proved to be the most serviceable for the intermittent experiments. In the later experiments upon association time, lip keys were used by both experimenter and subject, instead of Trautscholdt's method of making and breaking the circuit by hand simultaneously with the pronunciation of the word.

An important departure is made in the mathematical treatment of the results. Even the older protocols are worked over and presented in the new way. The arithmetical average is no longer, as in the earlier experiments, given as the most probable value of a group of observations, but rather the middle or central record. In the still later publication of Müller and Schumann upon memory, it will be observed that the middle record is also preferred. The reasons for substituting it for the arithmetical average in the treatment of psychometric observations seem on the whole well founded. The middle record is much less than the average disturbed by accidental influences, which, as is well known, have much greater power to lengthen than shorten records. The central zone, in which one-half the observations fall, cannot, as in the case of the average, be represented by the usual probable error with its double sign, but by two unequal values with different signs. These are found by subtracting the middle value of the whole group from the middle value of the upper and lower halves respectively.

Aside from the determination of the variability and sensory or motor character of mental work, the only way at present of studying the qualitative changes of mental processes is by classifying associations. The difficulties are very great. The rubrics chosen were: outer associations, including those of co-existence, sound and habitual expressions; and inner or logical. Practice will in a short time change cases of the second type into those of the first.

The later experiments are concerned chiefly with alcohol and tea.

The data for the study of alcohol comprise twenty-seven early experiments, ten each for simple and adaptive reactions, and seven for reactions with discrimination; three early series of association experiments and seventeen later ones. To these must be added twenty-seven experiments by the continuous method with seven subjects, ten each for adding and learning, and seven for reading. A few tests were also made with Ejner's time-estimating experiment and the dynamometer. From seven and a half to sixty grams of alcohol were given as doses.

The experiments with tea are about as numerous and were made in much the same way. With the exception of a few association lists, the experiments with other drugs belong to the early period of the work.

Small doses of alcohol have a stimulating effect upon reaction time, reading, and in some cases on the learning of the number series. This begins shortly after taking the dose and lasts twenty or thirty minutes, and is followed by a depression, or lengthening of

the records. Even in small doses it has a depressing effect at once upon adding and usually upon association; and in large doses, upon all the processes. The contrast in the effect of alcohol is greatest between simple reaction and reading, on the one hand, and reactions with discriminations and adding on the other. The other mental operations occupy an intermediate position, for some subjects showing acceleration, for others a depression. The two sharply contrasted classes contain the one, sensory and intellectual; the other, motor processes. The clue to the understanding of the results is the distinction between sensory and motor modes of reaction. Sensory and intellectual processes are depressed, but motor stimulated. The different results in learning number series are to be explained by the fact that it is a motor process for some and a sensory one for others. The dynamometer record is of the latter type, so only the rate and not the strength of movement is accelerated. The effect of tea is nearly the reverse. It stimulates the sensory and intellectual processes and depresses the motor slightly. The influence of ten grams of tea shows itself for about an hour. The effect of the other drugs differs greatly from that of these two in degree and duration, but otherwise the effect of morphine (.01 grams) is like that of tea; and the effect of small doses of ether, amyl nitrite, chloroform and paraldehyde is like that of alcohol. An increase of sensory or intellectual activity is always accompanied by a motor depression. The author suggests this may be due either to a selective chemical effect of the drugs upon the parts of the nervous system connected with the sensory and motor functions, or it may be due to the removal of physiological inhibitions by the depression of the higher centres. The author's interpretation of his results is beautifully illustrated by the chart at the end of the volume. Since the curves do not stand for any special experiment, the choice of ordinates is of course somewhat arbitrary.

BERGSTROM.

Studies from the Yale Psychological Laboratory. Edited by E. W. SCRIPTURE, PH. D. Instructor in experimental psychology. Oct., 1893.

These studies are the results of the first year's work in the Yale laboratory established and directed by Dr. Scripture. The work includes the following studies:

1. *Investigations in reaction-time and attention*, by C. B. Bliss, Ph. D.
2. *On monocular accommodation-time*, by C. E. Seashore.
3. *On the relation of reaction-time to variations in intensity and pitch of the stimulus*, by M. D. Slaterry, M. D.
4. *Experiments on the musical sensitiveness of school children*, by J. A. Gilbert.
5. *A new reaction-key and the time of voluntary movement*, by E. W. Scripture and John M. More.
6. *Drawing a straight line; a study in experimental didactics*, by E. W. Scripture and C. S. Lyman.
7. *Some new psychological apparatus*, by E. W. Scripture.

The conclusions reached in the first part of the first study were: 1. No difference in reaction-time was found when the color of the light present in the field of vision was changed. 2. No difference was found in the times of reactions in the dark and those made while looking at a stationary incandescent light of six candle-power. 3. With the light in motion, the reaction-time was lengthened. 4. No difference was detected between the times of reactions in silence and those made while listening to the steady sound of a tuning-fork making 250 vibrations per second. 5. When the intermittent sound of a metronome was substituted for the fork, the time for reactions was lengthened. 6. The reaction to a sound heard in both ears is

shorter than when the sound is heard only in one ear, even after making allowance for the difference in intensity.

The second part took up introspective observation on reactions. The results obtained here were: 1. Reaction-time is constantly affected by irregular disturbance, a large part of which may be detected by introspection. 2. Introspection is not to be trusted in estimating results. 3. Exercise shortens reaction-time. 4. Reactions to the wrong signal, reactions before the signal is heard, and the reflex nature of reactions, are not sufficient criteria to distinguish muscular from sensorial reactions. 5. There are, at least, six distinct kinds of voluntary attention: ideational attention, neural attention, feeling attention, muscular attention, preparatory attention and inattention. 6. The involuntary attention is constantly changing.

In this second part, besides noting some misquotations wholly inexcusable, the reader comes to the general feeling that the results of introspection and experimentation are unscientifically blended. One, wonders, too at such a statement as the following: "A sixth state of the attention, one which requires as much effort of a certain kind as any, is that of inattention." By this is meant a voluntary turning away of the attention from one thing to another. Or, as in this case, voluntary muscular relaxation.

Mr. Seashore found that: 1. Within certain limits the accommodation-time varies with the distance between the points for which the eye is to be accommodated. 2. It takes longer to change the accommodation from near to far than from far to near, and this difference in time varies directly with the length of the accommodation-time. 3. For equal distances in the same range the accommodation-time is greatest for points near the eye and decreases with the distance of the points from the eye.

Experiments with electrical stimulus applied to the skin show that the reaction-time decreases with the increase of the stimulus.

Dr. Slattery came to the following conclusions for reactions to tones: 1. The law that the reaction-time decreases with increasing stimulus does not hold for hearing, and the reaction-times for tones of moderate intensity are about the same. 2. The longer time registered by some for very weak tones is probably caused by hesitations as to the actual hearing of the stimulus. 3. The reaction-time to tones decreases as the pitch rises. 4. The view held by Exner, Von Kries and Auerbach and rejected by Martins, namely, that about ten vibrations are necessary to the perception of a tone, no matter what its pitch, is sufficient to explain the differences in the reaction-times for different tones.

The most suggestive and interesting of these studies is the one by Mr. Gilbert, to determine the sensitiveness of children to changes in the pitch of the tone $\bar{a} = 435$ of international pitch. For this purpose a clever instrument was devised from a pitch-pipe whose reed was fitted with a sliding clamp. Five boys and five girls were tested for each age from six to sixteen and five girls for each of eighteen and nineteen. From these tests Mr. Gilbert came to the conclusion that "the least sensitiveness occurs with children of six years, where the average least perceptible difference is $\frac{1}{8}$ of a tone, and that as age increases the sensitiveness increases, although marked irregularities in this general increase was noticed at ten and fifteen years. The decrease in sensitiveness at these periods the author thinks is probably due, the former to second teething and the latter to puberty."

Dr. Scripture and Mr. Lyman found that in "ten boys of the average age of thirteen years of the upper grammar grade" for

drawing a straight line between two dots 100 mm. apart, "the facing position is more favorable for horizontal and vertical lines than it is for inclined lines. The right side position is also more favorable for horizontal and vertical lines than for lines at 45°. Holding the pencil far from the point is in general the most accurate method; near the point is as accurate as the middle grip."

To understand the apparatus described in the other articles, the reader is referred to the original, where they are illustrated and adequately described.

F. B. D.

L'audition Colorée et les Phénomènes Similaires. Communications de MM. FRANCIS GALTON et EDOUARD GRÜBER.

The results of the investigations set forth in this paper were read at the Congress of Experimental Psychology at London, 1892. After giving a table of the "chromatisms and photisms of the senses," the results of investigations concerning colored auditions is taken more at length, and especially that of the speaking voice. The subject experimented on heard *a* as pure white; *e*, yellow; *i*, blue; *o*, very black; *u* (*ou*) black; *ä*, brown, and *i*, gray, approaching black. The same thing occurred for the consonants, except at the moment of hearing, the subject perceived two colors; one, the color of the consonant, and the other, a slight ray corresponding to the vowel used in speaking the name of the consonant. For example *f* (*ef*), is accompanied with the perception of a reddish-brown and a slight orange tint on the front side. This orange tint, the author thinks is due to the influence on the usual color (yellow) of *e*, of the reddish-brown of the letter *f*. This leads to the attempt to separate the vowel sounds from the consonants. The facts stated in this paper are very interesting, but perhaps not as important as the author thinks when he says: "These facts are of very great importance; they touch almost all the great problems of contemporaneous psychology. Moreover, they show a new path for crossing the field of the spiritually unknown, and give us a superior means of analysis."

F. B. D.

Die bewusste Beziehung zwischen Vorstellungen als konstitutives Bewusstseinsmoment. Ein Beitrag zur Psychologie der Denkscheinungen. Von DR. E. SCHRADER. Leipzig, Verlag von Duncker und Humblot, 1893, pp. xii., 84.

This pamphlet is the first of a proposed series of works upon judgment; more accurately, upon the place of the consideration of judgment in psychology and logic. I propose to devote some space to its criticism, since the author is making a serious attempt to answer a real and difficult question.

I may say at once that I do not regard the word "judgment" as a psychological, but only as a logical term. The psychological correlate of a judgment is an association or an apperceptive combination. I should, therefore, demur to the phrase "psychology of judgment" upon methodological grounds. The writer renounces it in favor of "psychology of conscious relation," for the reason that this is the more comprehensive expression—including judgments which are and judgments which are not formulated in language. But the confusion remains: a "relation" in psychology is just an association; relating is the logical way of marking associability (cf. preface, p. vi.).—A second point touched on in the preface is the relation of the association-psychology to psychology in general. While Dr. Schrader rightly refuses with decision to equate the two, he still

appears to lay too great a stress on the association-doctrine. The laws of association are coming to be more and more regarded in the way in which Aristotle regarded them,—as practical hints towards method, not as universal psychological uniformities. The notion of "association" itself has been divided up into those of "fusion" and "combination" or association proper; and we know far more of the former—new as the concept is—than we do of the latter. How much content is there in the association-doctrine of modern psychology? And how much exactness (p. viii.) attaches to the majority of extant association-experiments?

The introduction contains two sections. The first defines or describes "apperception," a term which the author uses in Erdmann's, i. e., practically, in Herbart's sense. A special critique of Wundt's view is promised. The second deals with the limits of the individual perception-idea. Its unity is a functional unity. The discussion shows (p. 4) a want of acquaintance with Külpe's work (*Zur Lehre vom Willen, etc.*). Chapter I. gives instances of judgment-like processes (Sigwart), and analyzes them into (1) ideas of perception and movement, and (2) reproduced ideas. The general problem of the idea itself (its apperceptive constituents, in Wundt's terminology), and the special one of the movement-idea, are not touched upon. Chapter II. opens with an attempt at the associative explanation of these processes. It is found unsuccessful. But the attempt can hardly be more satisfactory to the associationists than the foregoing analysis would be to the (Wundtian) apperceptionist. Not only has the author tied himself down to his own analysis, and so missed certain associative moments which an opponent would at once urge; he also speaks throughout in terms of the particular association, neglecting entirely the important process of general reproduction. He himself proposes three further experiments. [(1) Can the instances be explained on the assumption that their analysis was incomplete? This is negatived. (2) On the assumption that several of the previous ideas exercise in common a reproductive effect? Yes; if we accept Wundt's active apperception. Only partially, if we do not. (3) On the assumption of very highly complex ideas? (Constellation-unit.) Only partially.] Similar remarks apply to these: the associationist could find a ready answer to the objections raised. The proof of a constituent of consciousness other than the sensation (as basis of the associating idea) must, it is true, be looked for in the first place analytically. But why start with so complex a process as that of (technical) association? Rather is it advisable to analyze idea, impulse, attention. Then, if the non-sensational element be found, we can proceed synthetically to put it into the more complicated concrete processes in which it can belong, not caring for any charge of multiplication of entities. For this is the essence of the matter: the associationist says, "I find in this complex process only associated ideas;" his opponent says, "I find in it something more." It is introspection against introspection. What remains but a shifting of the ground to simpler processes? The result of their investigation may be a similar divergence, but, at any rate, the issue becomes so far clearer.

Chapter III. deals with conscious relation as a constitutive element of consciousness; the modification of the signification of "consciousness," which its introduction entails, and its character as positive or negative. It is a little hard that neither Herbert Spencer's doctrine¹ of the composition of mind, nor James' view

¹On p. 64 Spencer is mentioned, but the author's statement is dogmatic, and no references are given. There can, I think, be little doubt that Spencer's "relation" vacillates between *Beziehung* and *Verbindung*, and that it is not exclusively the equivalent of the latter term.

of the psychology of relation, finds mention. As for the exposition itself, I can only refer the reader to it. I hold in opposition to Dr. Schrader that the "relation" is logical, not psychological, but that it *can* be ideated (pp. 37 ff.), as every concept can. It is impossible here to justify this view at the necessary length.

In Chapter IV. is treated the relation between conscious relation and association. Association assists and prepares the way for relation; it is determined by the relation, and the latter is, in cases, resolved by it. In other words, the conscious relation stands to association very much as Wundt's apperceptive combination stands to it. Chapter V. discusses the views of some other psychologists: (1) Of the Herbartians and Münsterberg (doctrine of inhibition of ideas); (2) of von Hartsen; (3) of Stumpf (doctrine of relativity). Chapter VI. asks whether the conscious relation can serve to explain judgment. This question will be fully answered in the author's forthcoming *Analyse des Urtheils*.

If we take exception to Chapter III., it is unnecessary to criticise the superstructure raised upon it. While (as already said) I am in almost complete disagreement with Dr. Schrader on many points, I believe that his work is an honest effort to clarify our notions of the relation in which logic and psychology stand to one another. His further publications cannot but be interesting. E. B. T.

Warum müssen wir schlafen? Eine neue Theorie des Schlafes. DR. MED. EMANUEL ROSENBAUM. 1892, 58 pages.

The new theory is to be classed with those which attribute fatigue and sleep chiefly to the toxic products formed during activity. The author endeavors to show that the phenomena of sleep can be accounted for by the formation in the nervous system, and deficient elimination, of water, which is to be looked upon as a noxious waste product.

The theory is introduced by a synopsis of some of the facts of nervous and muscular activity and a sketch of the various theories of sleep from the time of Alkmaeon, 585 B. C., to the present. The reference to current theories is, however, very meager, and only that of Preyer is mentioned.

The new theory finds its support in a study of the anatomical conditions of the nervous system in diseases like scarlet fever, abdominal typhus, acute atrophy of the liver, meningitis tuberculosa, etc., which are characterized by a tendency to sleep.

The symptoms and anatomical details are taken from Ziemesson's hand-book. In all cases there is a dropsical condition of the nervous system to which the author attributes the mental disturbances, especially the abnormal tendency to sleep.

Another line of evidence is found in the statements of Schiff, Harless and Ranke, that the excitability of a nerve diminishes with the increase of water in it.

As to the source of the water, some may exude from the veins and arteries, but it comes chiefly from the chemical changes in the tissues. The water eliminated by the kidneys and sweat glands comes from the arterial, that of the lungs from the venous blood. The amount of aqueous vapor exhaled is not affected by the amount taken into the stomach, but by the amount of work done. The venous blood carries away the water formed by oxydation in the tissue metabolism. The principle of hydrodiffusion applies to its elimination. The percentage of water in the venous blood is less than in the tissues, hence they will be drained by it. The water removed by respiration is the only part of interest for the theory.

A study of the tables of Pettenkoffer and Voit shows that more water relatively to the amount of oxygen inhaled is expired during the night than during the day. The conclusion is drawn that elimination does not keep pace with formation, and that muscles must rest and the brain sleep to enable the organism to remove the accumulating surplus of water. Normal sleep is produced by the increase of water in the nerve cells. Some nerve tracts are less soaked than others and recover more quickly, hence partial cerebral activity and dreams. The winter and summer sleep of animals is said to be due to the presence of relatively greater amounts of aqueous vapor in the air, which hinders its elimination by the lungs. "Intelligence is in inverse ratio to the percentage of water in the brain and is to be measured by it, at least in the case of children."

In criticism, it may be said that other toxic products of activity, like lactic acid, urea, choline and neurine, etc., have a respectable claim to attention; that, like other theories of its class, it fails to notice the significance of the exhaustion of cell materials, which Hodge has shown to take place in normal cell activity; that it is by no means certain that the lymph which fills the spaces of the contracted cells is harmful, or that there is any noxious formation of water in nerve fibers, which Bowditch and Edes have shown to be practically unfatigable; and that it fails to notice the primary influence of habit and inhibition upon sleep, which makes sleep not simply a problem of physiology, but also of psychology.

BERGSTRÖM.

La Mémoire. J. J. VAN BIERVLIET, Professeur à l'Université de Gand. 1893, 40 pages.

The author gives a sketch of a theory of memory. He discusses briefly the modern views of the physical basis of memory; that it is the persistence of a movement, of a trace, or of a tendency to movement, in the nervous system. He believes Sergi's objection to the first theory, that the persistence of movements would bring on excessive fatigue and interfere with subsequent impressions, is valid. His own theory is a combination of the last two views, and is represented by the phrase *trace-disposition*. The theory is repeatedly illustrated by this figure: If we attach a weight to a wire of length L , which stretches it to the length $L + A$, and then remove it, the wire does not return to its original length L unless perfectly

elastic, but suffers a permanent modification $\frac{X}{A}$. A less weight will

later produce the elongation $L + A$, which represents the degree of excitation of the nerve cells required for consciousness. Retention depends upon the plasticity of the nervous system, which this figure typifies. Contiguity and succession are the laws of reproduction. Their physiological basis is the *trace-disposition* formed by the attention, which is essentially a nervous movement or excitation proceeding from one to another of simultaneous or successive impressions. Recognition, which is the essence of memory considered as a faculty of the soul, depends also upon the *trace-disposition*. Ideas which are recalled are characterized by an ease and facility which new impressions do not have. The basis of localization in the past is the degree of completeness and vividness of recalled images. The pathology of memory includes two groups of cases, hypermnesias and anamnesias. These are due to physiological influences which weaken or excite the nervous tissue, just as

heating the wire makes the requisite elongation $L + A$ more easy, while cooling it makes it more difficult.

The essay is admirably clear, but is chiefly of popular interest.

BERGSTRÖM.

Lehre vom Hypnotismus. PROF. H. OBERSTEINER. 1893, pp. 62.

This really adds nothing to what all interested know, but omits much of chief importance for his purpose. It is most surprising that no mention should be made of the new movement in Sweden, which has added a practical utilization of great therapeutic value.

Hypnotism and Mesmerism and the New Witchcraft. By E. HART. London, 1893, pp. 182.

This collection of papers and addresses is sensational and anything but thorough or systematic. The author has suffered for his interest in these phenomena, and claims to have read everything, but he makes no mention of Bernheim; thinks Charcot, whose "three states" are now abandoned, has done the best work; knows nothing, that we can infer from his pages, of the scientific work upon the subject done in Germany, in Sweden, etc. We agree with him concerning Luys and expressed five years ago the same conclusion in this journal, and only find the author uninformed. His book shows how little has been done in England upon this subject. All he says from first to last is belated and thrice told to all psychologists who are versed in modern psychiatry.

Genetic Philosophy. By DAVID JAYNE HILL. (Macmillan, 1893. 382 pp. 8vo.)

The author hopes to rehabilitate philosophy by giving it a scientific foundation.

"The problem of science is never ontological, but descriptive;" and "ontology is as little a problem for philosophy as it is for science, for there is no real problem. . . . What we seek is to know the phases of being and to unify them by discovering a continuity among phenomena which shall render them one to intelligence as they are one in reality" (p. 13).

The author disapproves of Hegel's absolute idealism, and attributes Mr. Spencer's difficulties with the unknowable to the fact that his method was synthetic rather than genetic. "The genetic method . . . consists in referring every fact to its place in the series to which it belongs." The book, therefore, consists of a series of scientific theories about the origin of matter, life, consciousness, will, morality, etc., which the author states in successive chapters, simply and clearly enough, but without doing very much to aid one in choosing between them when several conflict, or to show their metaphysical significance. But in spite of his protest against ontology, the author, like all the other writers who have made the same protest, enters the forbidden field and attempts to gather the forbidden fruit by the same old forbidden ontological method, though this method is only very partially and inadequately applied. He states, for example, that the deepest insight into the essential nature of "matter," "force" and "energy" is to be found in our own acts of will (p. 203), that inorganic processes represent "habits of the universe," and that "the universe as a whole is the expression of a 'will'" (pp. 367, 368). That "the ultimate ends towards which that will is directed" could not have been "immediately attained without the intervention of a long series of intermediaries," the author seems to regard as sufficiently

proved by the mere fact that the ends of human beings (who exist in a universe already made by another being) are realized only through progressive transformation (p. 368). The author's doctrine of personality rests upon the general "monistic" assumption that mind and matter are different aspects of a unitary being. It is as follows:

"The genesis of a personal being consists, then, not in the transmutation of physical force into psychic states, as materialism represents, but in the concentration and unification of preëxisting psychic elements, which, in their isolation are unconscious, into a conscious individual. Now my thesis is simply this: Consciousness is a complex phenomenon, not a simple state. It is made up of elements or factors which *become* consciousness in their union, but are *not* consciousness in their isolation. . . . The psychic aspect of a single brain-cell is not a consciousness, but the psychic aspects of a great many cerebral cells unified through the organic unity of an organized brain, become a consciousness" (p. 128).

What these unconscious psychic elements are, or how he knows that they exist at all, or how the organic unity of a brain can turn them into a consciousness, the author does not explain. The trouble with a "genetic philosophy" is apt to be that, as long as it is genetic, it is not philosophy, and as soon as it becomes philosophy it becomes uncritical and superficial.

H. A. A.

IMPROVEMENT AT LEIPZIG.

Former members of the first "Seminar für experimentelle Psychologie," will easily recall the upper story of the grimy "convikt," with its irregular suite of rooms, in which Professor Wundt has fostered the early growth of our science. Those quarters are now among the things that were. A modern edifice will soon occupy the site, and may possibly afford room for the psychological laboratory. Meantime Professor Wundt has taken refuge in the "Trierisches Institut," lately renovated for the accommodation of branches that were taught in the demolished convikt.

The present auditorium is inferior to the old one; it is smaller and the ceiling is too low. But the institute proper has gained by the transfer. It comprises ten rooms, all of which open on a corridor, a plan which does away with the inconvenience formerly felt of passing from room to room, at the risk of disturbing the workers. The improvement is most evident in the position of the library, which can now be reached without the trouble of rapping at half-a-dozen doors. Of the other rooms, two have been set apart for the professor and his assistant; each of the remaining seven is devoted to a special class of work, and furnished with appropriate apparatus. The dark room is considerably bettered by this arrangement, and the centering of batteries in a single apartment, from which all currents can be managed, avoids troublesome interference and loss of time. Add to these features a fine exposure in every direction, and certain provisions for comfort which not even a psychologist can forego—if the combination is not perfect, it certainly justifies the remark of Dr. Külpe: "More suitable quarters could not have been secured, had they been planned *ad hoc*."

E. PACE.

In the paper upon "Rhythm," published in this number of the JOURNAL, it was stated that sustained speech with children always became rhythmical. In support of this proposition we have to

submit the following observations made upon children by pupils of the State Normal School at Worcester, Mass. For the use of these, we are indebted to the kindness of Principal E. H. Russell:

B., age three years, had an older brother who went to school. One evening B. heard this older brother studying his arithmetic lesson and repeating the rule, "Reduce the numbers," etc. The next day B.'s mother heard him talking to himself and saying, "Renounce it by fives, an' fours, an' sevens, an' nines, and squeeze it as tight as you can." Evidently we have here a perfect rhythm. When the child attempts to repeat the rule, and being guided only by his own impulses, he selects just those words that will satisfy his rhythmical impulse.

G., age three years, heard the other children repeating the familiar incantation rhyme said when they were about to jump or run: "One to begin, two to show, three to make ready, and four to go." When G. attempted to say this to her mother, she said: "Two biggy to show, two forty go so." The child has not only caught the rhythm, but also the rhyme, and made her reproduction a type of a fairy measure.

M., age four years, watched very attentively her mother making a cake. The following day she came to the observer and commenced to repeat what proved to be the recipe for the cake which she had seen her mother making the day before. Says she: "One tea-spoonful of sugar, one cup of molasses, a spoonful of cream of soda, a little salt and some vinegar."

The rhythm is not so clear as in the previous cases, but it is impossible to read her words without feeling more or less clearly the rhythmical impulse which guided the child in the selection of her words. The subjects from which she had to select her words were ill-adapted to rhythms, and it is on this account, we believe, that the rhythm is not so perfect as in the previous cases.

THADDEUS L. BOLTON.

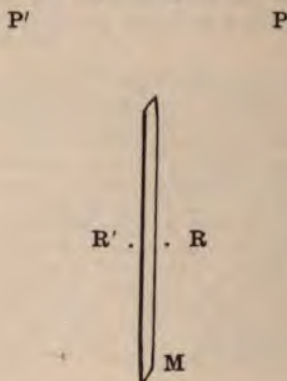
It is generally known that Pinel and Esquirol, who originated the systematic study of insanity in France, adopted the systems of Locke and Condillac as their basis; that Griesinger followed the philosophy of Herbart, although with slightly less fidelity; that Hughlings Jackson and Mercier follow Herbert Spencer in their conceptions of not only epilepsy, but of insanity generally. In education also, the influence of Herbart pervades and completely shapes nearly all that has been written in Germany concerning the theory of primary education scarcely less than did Hegelian ideas dominate theories of higher education in the days of their ascendancy. In this country and in England, Spencerian ideas seem just now likely to be no less controlling. There is much evidence that the tide is now turning and that the study of children, the brain, senses and insanity is giving its concepts to philosophy.

The following interesting observation should have had a more conspicuous place.—Ed.

A NEW AND SIMPLE METHOD FOR COMPARING THE PERCEPTION OF RATE OF MOVEMENT IN THE DIRECT AND INDIRECT FIELDS OF VISION.

While sitting in my room last winter, my attention was attracted by the image of a swinging lamp in the mirror, and it then occurred to me that here was a simple method for comparing the perception of rate in the direct and indirect fields of vision. I took a position where, by looking directly at the image in the mirror, the image from the lamp itself fell on the indirect field of vision. I thus had exactly the same rate, and, provided that I placed myself so that my eye would be near the glass, almost the same extent of movement of the images. The experiment at that time was roughly made, but it showed clearly the well-known fact that of two equal rates, the one seen in the indirect field seems to be the more rapid.

I have since tested the method under more favorable circumstances, and offer it as a simple demonstrational method: Take a small, clear mirror and arrange it in the median plane immediately between the eyes, so the eye of the observer may be near the edge of it. Make a pendulum of a small string weighted with a lead ball and place it at some distance away, but near enough to the plane of the mirror to make an angle of perhaps twenty or thirty degrees with it. Swing the pendulum, not too far, nor too fast, in a direction perpendicular to the plane of the mirror. If the observer now directs his eye toward some part of the arc, through which the pendulum swings, so that the image of the moving ball will cross the point of clearest vision, the image from the mirror will fall on the indirect field and the two rates can be easily compared. The following diagram will perhaps help to show the arrangement:



(Note: P = position of the pendulum; P' = position of images as seen in the mirror; R = position of right eye, and R' of the left; M = mirror.)

It will be well, perhaps in most cases, to cover the ball of the pendulum with white paper. It should be noticed also that the background should offer no distraction to the attention as sources of error for the judgment.

The apparent difference in the rates will be greater when the observer directs his eye toward the pendulum, because the image from the mirror will then fall on the temporal side of the retina, which is less sensitive than the nasal side, especially in an observer whose eyes are deeply set.

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STUDIES IN THE PSYCHOLOGY OF TOUCH.

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I. Psychology of Touch in General.

If the nervous mechanism of the body could be viewed, apart from the rest of the body, with all the nerve trunks and branches *in situ*, the form of the body would be duplicated in its externality by the network of nervous fibres and filaments which ramify and penetrate every part of the surface of the body. It is believed that each one of these fibres or filaments, however fine, connects directly with the nervous centre, and that whatever stimulus is received by each is carried to the centre over independent fibres, and that no transition of stimulus from one fibre to another is possible, however closely they may be placed together. Not only is the isolation thus perfect, but if a fibre be broken or cut, there can be no further communication established between the part of the surface where this fibre is attached and the nervous centres until the distal portion is regenerated. If the nerve trunk be submitted to mechanical pressure, the communication is severed so long as the pressure continues. Thus the whole surface of the body is in more or less complete independent connection at each point with the higher centres, and, as we shall see, this independence in structure is necessarily followed by a similar independence in function, whereby the great differences in local discriminative ability have arisen and the conditions are furnished for further dermal education.

One of the first things to notice and probably the most striking fact in the study of the skin as a sense organ, is the great difference in its extent as compared with the other organs of sense. If the sensitive surfaces of all the other sense organs were spread out in direct contact with the outer or objective world, the whole of these taken together would scarcely equal the surface of the palm of the hand. Though of course this comparison must of necessity be crude, for each organ's sensitiveness is measured by wholly different stimuli and these are incommensurable with respect to each other. Still when it is called to mind that eye, ear and nose, with all their special adaptation, are but "specialized dermal cells," the comparison is not wholly without value. Furthermore, embryology teaches that not only are the sense organs thus developed from the skin, but that the whole of the brain and the spinal cord are infolded and developed ectodermic cells. The influence of this fundamental connection between the higher centres and the skin, may be much more far-reaching than psychology has yet thought. There is at least a suggestion of the utmost importance in the philogenetic relation of the skin and the nervous centres. Some have dared to hint that the objectivation of the subjective states may here have a real causal basis. But it is safer to wait until biology, at least, has more to say on this point, before making assertions the proofs for the truth or falsity of which are still hidden in the abyss of our ignorance.

Then, too, the skin is situated and conditioned unlike any other of the sense organs, in that it is turned in all directions. The other sense organs are more or less directed toward the front and at all times are limited to a part even of their immediate environs. In a large measure this limitation is not true of the skin, though of course certain accommodations of hand and body are habitually made to facilitate and refine perception through touch. As a sense organ the skin is much less specialized than any of the other sense organs. Its range is not only larger, but it takes cognizance of the more fundamental properties of the material world. Our eyes are useless in the dark, our ears are without value when there are no vibrations in matter, but the conditions of touch remain so long as there is objective existence at all. The skin is the mother sense and out of it, all the other senses have been derived. The biologists tell us that the eye is only a developed sensory spot of the skin, and Preyer¹ even goes so far as to say that as a basis for the fact, we are constantly speaking of the qualities of things seen, in terms of qualities only felt, such as a

¹ Pflüger's Arch. 1881, p. 75.

warm color, a cold shade, etc., that the sense of sight was developed out of the sense of temperature.

If we take the *amœba* or any of the simpler forms of life, we can readily see that their whole life must depend on the kind of reactions they are prompted to make through skin perception. The whole surface of the bodies of such organisms presents to their environs a sort of diffused touchiness, apparently not more differentiated in one place than another. They are all eye if they see at all; all ear if they hear, all nose if they smell. Wherever they touch a particle of exterior matter, they need no further adjustment to know it as far as it is possible for them to know. There is no parallel to this in any of the other senses. As Aristotle¹ has said, we can have an animal without eyes, without the sense of smell, or taste, or hearing, but all must have the sense of touch. In fact, it is only the few in comparison that have any other sense than touch. With this primordial and far-reaching notion of touch in mind, let us examine the properties of bodies known to man through the senses. Perhaps this can be more quickly done by excluding the properties of objects derived from or through the other senses. Through the eye we get color and motion and perhaps an indistinct (until corrected and helped by the other senses) notion of distance. For the ear only sound, or vibrations within certain limits. For the nose odor, and for taste certain chemical stimulants. All the rest of our mental lumber has come through the skin either directly or indirectly. If we were asked to name the most permanent and most important properties of bodies, we should not go out of the realm of touch, including the so-called muscle sense, which is largely skin sense. We would name hardness, weight, temperature, tenacity, compressibility, shape, roughness, etc. It is easy to see that these are the fundamentals. We can easily be deceived in color, but not so with hardness. Certain interpretations of odors are very largely due to the personal factor, but weight does not exist as a variable except in degree. We have standards fast and exact for determining weight, resistance or cohesion of bodies, but not for the strength of their odor or the shades of their colors. We do not speak of a person as abnormal if his ear is not discriminative for great difference in tone, but we consider him abnormal if he could not distinguish thousands of variations in location, in quality, in temperature, etc., of objects through touch.

Then again it is perhaps helpful to see that the other senses depend largely on the skin for proof of their assertions as to

¹ Psychology, Wallace's Trans. p. 67.

the more important of their activities. We come to judge of form by the eye, but we are in constant need of tactual measurements to correct and prevent misconception. Doubting Thomas thus becomes a sort of type for realists, all of whom insist on the most certain proofs and so rely on the tangible. As Mr. Fraser ¹ says, in his suggestive essay, "In the case of the unreflective but practical thinker the question, 'What do you mean by a real world?' is openly answered and without bias by saying, 'It is a world that we can touch.' " "He can be persuaded that the object he *sees* before him is illusory; but if he is allowed to stretch forth his hand and can touch it and feel it there, the last remnant of doubt as to its real existence will have fled." Very rarely, if ever, do you see the fakir or sleight-of-hand performer practicing his tricks save to deceive through the special senses. Indeed, the phrase "sleight-of-hand" may have for its earlier psychological basis, the meaning now best rendered by slighting of the hand. Dr. Johnson's reply to the Berkeleyan philosopher who pressed him with his idealism, was, as everybody knows, "Throw a brick at him."

This fundamental and deep lying significance of perception through touch is seen in the commonest words having for their import to know:—*perceive* means to take thoroughly or to take hold of; *conceive* has for its fundamental and original meaning, to take hold of things together; *apprehend* is to catch hold of; *comprehend* means only to catch hold of more than one thing; *understand* means measurement of things by letting them rest on you; we *accept* a thing mentally when we take hold of it. The modern slang phrase "catch on" illustrates this same tendency to coin terms designating intellectual activity. And if the theory be true that slang is a linguistic atavism, this last phrase is full of psychological significance. Just what the full meaning of this strong tendency is, perhaps does not fully appear, yet it is certainly significant in preserving an early and for that matter an inherent theory of indisputable knowledge. "Why touch," says one writer, "the simplest and earliest sense, should in its higher forms be more than any other sense associated with the advance of intelligence, is due to the fact that tactual impressions are those into which all other impressions have to be translated before their meaning can be known. It is certainly true that a highly elaborate tactual apparatus comes to be the uniform accompaniment of superior intelligence." As has often been said, the skin is the boun-

¹ Psychological Foundation of Natural Realism, AMER. JOUR. PSYCH., Vol. IV. No. 3, p. 429.

dary between the ego and the non-ego ; that all that is within is subject, and without is object. So in touch we come nearer to the "thing in itself," if you please, than through the medium of any other of the senses. We can not smell a *thing*, but only the gaseous particles given off by the *thing*; we can not come into contact with a *thing* through vision, but only with the vibrations of ether which are radiated *from* the thing; in taste pure and simple, we are made cognizant only of a thing after its dissolution, and not of the thing as a whole. So there comes to be less need of interpretation in the case of skin perception than in any other, and so less opportunity of illusion. Speaking of this point Sully¹ says: "Tactual perception in so far as it is the recognition of an object of a certain size, hardness and distance from our bodies, involves the least degree of interpretation and so offers little room for error; it is only when tactual perception amounts to recognition of an individual object, clothed with secondary as well as primary qualities, that an opening for palpable error occurs."

Herein lies a pedagogical principle far too infrequently used. In the years of childhood we feel the world of sensitive appearances nearer to us; we live immediately with and in it; there is an intimate bond of living dependence which unites us to it, and which is only broken when we are removed from contact with things. The brook to the boy with his pantaloons rolled up and the water gurgling around his legs is far more a brook, far more real, than to the poet who only hears its gurgle, or sees it sparkle. This close and intimate relationship between subject and object which we get through touch has led men to say that if the facts of physiology were rightly interpreted, they would lead irresistibly to the conclusion that sensation is not confined to the brain, but is spread over the whole sensory system. Wherever there is a sensory nerve there may be a sensation.

Johannes Müller said: "We do not assert that the mental principle has its seat in the brain alone. It is possible for the mind to act and receive impressions by means of an organ of a determinate structure, and yet be present generally throughout the body. The mental principle or cause of the mental phenomena, the conception of ideas, thoughts, etc., can not be confined to the brain. It exists, though in a latent state, in every part of the organism." Or as Isaac Taylor said: "The mind is not, as we suppose, the prisoner of the attic story, but is the occupant at large of the entire animal

¹ Illusions, p. 49.

organization, acting in each part of the structure according to the purpose of each."

If we consider carefully the probable value of double cutaneous sensation to a child in giving to it the basis for a separation of the ego and non-ego, we shall see without doubt that this simple and oft-repeated experience is of far-reaching significance. When a child first takes hold of something external to itself, if there be but a single sensation or set of sensations fused, there would not of necessity grow up a notion of otherness. But it seems to me, this basis for otherness has its origin in the double sensations it receives when it touches and handles its own body. If it takes its foot in its hand there are two wholly different sets of sensations presented simultaneously, and because of their unlikeness there must come a feeling, or consciousness of otherness, which in no wise could come through any set of single impressions. Later in life we come to use the special senses more and more and the skin sensations are left to form the greatest part of the semi-conscious state. When we stop thinking, and just *feel*, we find ourselves in a flood of sensations coming from the skin. While it is in a measure true that we are accustomed to neglect all of these sensations, notwithstanding they are pouring themselves in upon us and in no small measure constitute the mood, affect the temperament and put the attention in a state of uneasiness which demands and requires more stimulus to prevent mind wandering than when the subconscious is minimized. "We are most fully conscious," says Bastian, "when we are most receptive of external impressions, and we lapse into a completely or partially unconscious condition, when the advent of such impressions is for a time prevented, or when we are intensely absorbed in some train of thought, that is, when the activity of other portions of the cerebral hemispheres in some way dwarfs or eclipses that of the sensorial regions proper."

It is, to say the least, an interesting notion of Lotze¹ that whenever we bring a foreign body in contact with the skin, the consciousness of our personal existence is prolonged into the extremities and surfaces of this foreign body. According to him a tall head-dress is worn preferably because it lengthens our own "personality," producing the pleasing illusion that we ourselves extend up to that point. Thus we can understand the "disposition in low stages of culture, perfected afterwards in higher ones," to wear plumed helmets, waving and lofty coiffures. These fortified the consciousness of the wearer with the feeling of a majestic upward ex-

¹ Microcosmus. Eng. Trans. Vol. I. p. 592 et seq.

tension of his personality, as well as served to increase the fear-inspiring or respect-inspiring effect of the figure on others. So, too, high heels and stilts, while they raise us off of the ground, do not apparently separate us from the ground. The cane to the blind man becomes merely a prolongation of his arm and the touch at its end renders him directly conscious of the distance and quality of the object with which it comes into contact. The second class of these feelings is derived from all hanging and waving drapery and ornament, which "causes us to feel as if we were ourselves present" in the floating, flapping folds and streamers. This notion, he thinks, is the key to the uses of sash and ribbons, girdles, bracelets, trains, and all manner of wearing apparel of this sort. Thus the light gauzy stuff with which the maiden drapes herself is intended not only for the graceful curves that touch the skin in a few points, so as to charm others, but she herself feels that she is present in all of these movements which she "distinguishes as to the breadth, lightness and softness of their sweep." To Lotze all this was not mere theory, but a fundamental law of dress. For he says he has performed for the exact science of dress the same service as Kepler did for astronomy.

Then again, perhaps much of our æsthetic life has a closer connection with the skin sense directly than we are wont to think. There is without doubt truth in Lotze's¹ idea, when he says: "Accordingly it [the vesture] serves to make obvious to the senses the peculiar mobility of the figure and likewise gives to it, by means of the arrangements of its hanging folds, a semblance of the feeling which the figure itself preserves through its surroundings and its carriage, and in which it delights." And again he says in the same volume: "The pose of the figure in plastic art will always have for its principal purpose to show not only the complete adaptability with which the body obeys the spiritual impulse, but also that with which every single part by corresponding counterpoise, tension or relaxation compensates for the displacement of every other, so that the body appears as an extremely sensitive and elastic system of parts which keeps itself in equilibrium."

It is a noteworthy fact that illusions and hallucinations of the insane come through the sense of touch far less frequently than through sight or hearing. According to Griesinger,² in 177 cases of hallucination, occurring mostly in sane people, the following relations occur in the senses: Sight alone, 78;

¹ Outlines of Æsthetics. Trans. by Prof. Ladd, p. 76.

² Mental Diseases, p. 81. et seq.

sight and hearing together, 46; hearing alone, 16; sight, hearing and touch together, 4; with touch alone, only 9. He adds that diminution or complete suppression of the sensibility of the skin to impressions of temperature or pain is by no means frequent, still less is it general in insanity. Snell found that in 180 patients less than a score were anæsthetic, and where anæsthesia was present it occurred in cases offering very little hope of recovery. "When we read the history of trials for witchcraft," says Dr. Michéa,¹ "we observe that the inquisitors attached a high value to the existence of *cutaneous anæsthesia* as a sign of demoniacal possession. When an individual was charged with the alleged crime, the experts, after having bandaged the eyes, passed a magnifying glass over all parts of his body, previously shaved, with a view of discovering the marks of Satan (*stigmata diaboli*). The slightest spot on the skin was probed with a needle. If the puncture did not cause a painful sensation, if it provoked no cry or movement, the poor creature was a sorcerer and condemned to be burnt alive. If, on the contrary, he felt the wound, he was acquitted."

The fundamental character of the skin perceptions appears in a curious and interesting way in the Law. It is a well established fact that "mere words will not make an arrest." That is to say, the officer must touch the person whom he wishes to arrest in order to make a binding arrest, unless in some way the person, upon whom the writ is served, submits and shows himself subject to the authority of the officer without being touched. This will be made clear from the following decisions: "Bare words will not make an arrest, but if the bailiff touch the person, it is an arrest and the retreat a rescuous. On a motion for an attachment against three persons for a rescuous of a person taken in execution, it was objected that there had not been a legal arrest, as the bailiff had never touched the defendant. The court said this is a good arrest, and if the bailiff who has a process against one says to him when he is on horseback, or in a coach, 'You are my prisoner. I have a writ against you,' upon which he submits, turns back or goes with him, yet it is an arrest because he submitted to the process; but if instead of going with the bailiff, he has gone or fled from him, it could be no arrest unless the bailiff had laid hold of him. . . . It is not necessary to show the warrant, or to tell at whose suit you arrest him, unless he demand it. And if you have two warrants in your pocket against him and produce neither, if he be rescued, either party at whose suit the warrants were made out may bring

¹ P. Gray. "Chirurgia," 1609, lib.VII. c. 10 (quoted from Winslow)

an action against the rescuers." In another old case,¹ it was decided: "Genner, a bailiff, having a warrant against Sparks, went to him in his yard, and being at some distance told him he had a warrant, and said he arrested him. Sparks having a fork in his hand keeps off the bailiff from touching him and retreats into his house. And this was moved as a contempt. It was held: "The bailiff can not have an attachment, for here was no arrest nor rescuous; bare words will not make an arrest, but if the bailiff had touched him, that had been an arrest, and the retreat a rescuous, and the bailiff might have pursued and broke open the house, or might have had an attachment or a rescuous against him; but as the case is, the bailiff has no remedy but an action for the assault, for the holding up of the fork at him when he was within reach is good evidence."

Again:² "I have not been able to find any real conflict between English and American authorities as to what constitutes an arrest. By all the authorities, a person may or may not be arrested without a manual or actual touching by the officer. Bare words alone will not make an arrest if the party resists the arrest." Many other cases might be cited, but so far as can be determined the same principle is held to in all of them, viz.: "The question as to whether or not an actual physical caption or manual touching of the body of the prisoner is necessary to effect an arrest depends upon the further question, whether or not the arrest is submitted to. If the party resists or endeavors to evade it, there must be an actual touching of his person, or some physical restraint of his liberty to depart, in order to complete the arrest; mere words will not do in such cases."

The importance of this whole matter for psychology depends upon whether touching, as used in arrest, is a survival of the attempt to take the prisoner by force, or whether it be that touch is here recognized as that sense through which errors or misunderstandings are less liable to occur than through any of the other senses. To consider it as a survival of the employment of physical force, seems by far the more natural explanation, and at first thought there seems to be scarcely any other way of looking at it. But the question comes, how would an officer arrest a blind and deaf man, or even a deaf man who could not read, except he make known the arrest through touch? This sense is never lacking completely during normal conscious life and so is always an open avenue through which the outer, or objective, becomes inner or subjective.

¹ See *Genner v. Sparks*; 1 Salk. 79.

² *Searls v. Viets*, 2 Thomp. & C (N. Y.), 224, Potter, J.

However, if it be true, as seems probable, that the touch now recognized in the law of arrest is a survival of a more forcible means of taking possession of the prisoner, its present signification is entirely different, and I am assured by high authority that it is used now to simply make known to the one arrested that he is now in the power of the officer, and there is no means of denying it, because he was made acquainted with the fact through placing his hand on him.

The rôle that touch plays in the expression of the emotions of love and friendship can scarcely be over-estimated. The psychological significance of this fact is seen in many words we use for friendship and love. The words, *attraction*, *affection*, and the phrase, *attached to*, illustrate this. As is readily seen, these words have for their fundamental meaning the notion of being bound in contact with the loved one, and so indicate that there is a desire in us for actual dermal contact in the expression of love and friendship.

Dr. Brinton, the eminent "Americanist," has called attention to the fact that the same fundamental significance of the words for friendship and love is shown in many of the Indian languages. With the Chipeways, for example, the root for the word love is *sak*, which means *to attach, to fasten*. The root for the same word with the Aztecs was *zo*, which means *to fasten one with or to another*.¹

It needs no exhaustive research to see that with all peoples, their heartiest salutations and greetings, their deepest and most fervent feelings of love and friendship require tactual expression. It has been the custom at all times and among all peoples to shake hands, to embrace, to kiss when friends meet. True it is that these tactual expressions have taken a great many forms, but each of them makes use of some form of contact.

It would be a most interesting study to trace the racial and local variations of these tactual salutations with especial reference to their development and evolution. It seems to me that a careful study of these forms would not only indicate the gradual changes and development of such feelings as they express, but that these accompany the development of finer capabilities of dermal sensitiveness.

The Feugians and Australians hug each other when they meet. Among the ancient Jews this same custom prevailed, supplemented by a kiss. We read in Genesis XXXIII, 4, that "Esau ran to meet him (Jacob) and embraced him and fell on his neck and kissed him and they wept." One of the most universal customs among the East Indians, and the

¹ Essays of an Americanist. (Philadelphia, 1890), p. 410.

Pacific Islanders in general, is that of "nose rubbing," as the travelers are wont to call it. These peoples, when they salute, have the general custom of touching their noses together and perhaps snuffing or smelling of each other. The kiss seems not to prevail so extensively among peoples of a lower stage of civilization in their salutations as it does in the higher stages. The Orientals, as is well known, have an elaborate system of salutations, in which the kiss plays an important part. Indeed, this method of salutation appears to have been practiced from the earliest antiquity by Semitic and Aryan peoples. If we should ask ourselves why we "shake hands," the answer, it seems to me, is not found in the usual explanation that we then see the one whom we meet does not carry a dagger, but in the demand for tactual expression of friendship. It is almost amusing to see to what inconveniences we will submit ourselves merely to grasp the hand of a friend and in return experience pressure from his hand.

Almost innumerable citations might be made showing how the forms have varied, but those given are sufficient to illustrate the point in question. Although, as has been said, the chief purpose of these forms of tactual salutation is that of the expression of the emotions, yet the fact must not be overlooked that a mere grasp of the hand often furnishes us with data concerning the character of a person not obtainable in any other way. The complex sensations of warmth, of firmness of grasp, of passivity, etc., etc., which we derive from a handshake, in no small way affect our opinion of a newly made friend. With some, these dermal sensations are all powerful. A friend related to me an instance which came under his direct observation, in which a young lady refused to continue her acquaintance with a young man simply because, as she said, the touch of his hand gave her "the horrors." It seems that with each of us there is a certain delicacy of touch, a certain degree of warmth, a certain amount of pressure, best suited for personal satisfaction, and corresponding to our notion of the best and fittest.¹

¹ Since the above was written, I have found that Dr. Brinton has expressed the same idea in his recent book, "*The Pursuit of Happiness*" (D. G. Brinton, Philadelphia, 1893, p. 131). He says: "When we consider how slightly most sensations of touch excite subjective states of mind, it is remarkable that in response to one stimulant, they are among the most powerful known in nature. This stimulus is that of another personality. The most positive feelings of both aversion and attraction are those excited by physical contact of the naked flesh. This is why it has been accepted in so many countries as a sign and proof of amity. The savage Africans touch noses and the civilized European shakes hands or kisses the hand or the cheek."

II. Education of the Skin with the Aesthesiometer.

The important discovery of E. H. Weber concerning the great local differences in the discriminative ability of the skin for the compass points, marked an epoch in touch psychology. His work not only stimulated to careful anatomical studies in order to know more of the skin as a sense organ from the anatomical side, but it has led to studies whose object has been to determine the possibilities of education in the discriminative ability of the skin.

A. W. Volkmann¹ found that the distance between the compass points, placed at the minimum distance they could be felt as two on different parts of the body, could be greatly decreased for any given part by exercise. And not only did this education show itself with regard to the special territory exercised, but strangely enough the symmetrical part on the other half of the body was likewise very markedly increased in fineness for this spacial discernment. He found that this increase, however, was soon lost for both sides, when the exercise was given up, and that after a month's rest, the increase was wholly lost. Suslowa observed that if the part of the skin lying between the compass points be stimulated by means of an induction current, or some light mechanical stimulus, such as touching it with a pencil, the sensitiveness for localizing was increased. Funke² found, just as Weber did, that on the median lines of the body, the skin is not so discriminative for the compass points. He also found that, after a month's exercise of a portion of the skin on the median line between the shoulder blades, there was but little or no increase in the power to discriminate between the compass points.

This ability to localize, however, varies greatly with different people. Valentine found that corresponding points on different people varied, even to a difference of four-fold, as regards their ability to distinguish the compass points as separate. But he also found that the ratios between the numbers expressing the fineness of discrimination for different parts of the skin of one person was about the same as that of another, even though much absolute difference existed in their abilities to discriminate finely.

Speaking in a general way, the more mobile the part of the body, the finer the discrimination it possesses. According to Vierordt, the relative fineness of the sense of locality of a given point of the skin of a part of the body, is a function of

¹ Berichte d. Sächsischen Gesellschaft d. Wissenschaften, p. 38. f.

² Hermann's Handb. d. Physiol. III. ii. p. 38.

its mobility depending on the relative extent of the excursion which it makes in the movements of the parts concerned, around its own axis, and increases proportionally with its separation from the axis upon which the part turns. While the facts do not support this so-called law completely, it must be regarded as expressing in a general way approximate truth.

In order to test the work of Volkmann and others, I began and carried out a series of experiments with special purpose of determining (1) how rapid the increase in sensitiveness through practice would be under given conditions; (2) to what extent the transference of this increased power would be made; (3) whether the sensitiveness of the surrounding parts would be raised or not; and (4) the curve for the loss of the power thus gained, after the exercise of the part had ceased.

METHOD OF WORK.

The tests were made on two subjects, F. B. D. (a man) and C. W. D. (a woman). For F. B. D. an area of the skin 7 cm. square on the palm side of the lower part of the left arm, beginning about 5 cm. below the elbow, was selected; while for C. W. D. a portion of the skin of the right arm 5 cm. square, also on the palm side, half way from the wrist to the elbow, was taken. In making the tests an *æsthesiometer* was used, whose points were of ivory and moderately sharp. It will be seen, by referring to the table, that two sittings for each subject were taken daily (with the exception of a few days), practically at the same time each day, and extending over a month. In the beginning of the work both arms of each subject were tested carefully on equal symmetrical parts and the discriminative ability noted for each. After this ability had been determined for both arms of each subject, experimentation was confined to the right arm for C. W. D. and to the left for F. B. D. At each sitting from twenty-five to forty observations were made on each subject. The subject being blind-folded, the skin was touched with the *æsthesiometer*, and the subject asked to judge whether one or two points of the instrument were touching the skin. The operator was careful to present these two conditions indiscriminately and equally often. The subject was so situated that the arm was always at rest and not otherwise stimulated. The operator was careful when two points were presented to have them touch the skin as nearly as possible at exactly the same time, otherwise the discriminative power would be reduced to that of the time-sense for

impressions, or at least so confused with it that the rest would be too complex and varied. Then, too, care was needed to so shift about over the part to be stimulated that the after-images would be as little as possible confusing. To this end it was necessary to work often quite slowly and to make too many observations at one sitting. Besides, the effect of fatigue was plainly noticeable after a long series of observations and so it was important to notice this fact. The method of finding the exact symmetrical part on the arm consisted in inking afresh the boundary lines of the spot exercised and then, after placing the palms of the hands together symmetrically pressing the arms together. Thus the same sized area and exactly symmetrically placed was obtained. The same plan was used at first so as to obtain the relative ability of the two arms before either had undergone a special exercise.

RESULTS: The following table is a statement of the distance of the points to begin with, and the results of the ability to discriminate, with the date and time of day given. The results are given in mm., and in all cases represent the distance between the two points of the æsthesiometer, which was at least seventy-five per cent. of the judgments were correct. It will only be necessary to glance down the columns to see the great increase in the discriminative ability. As will be seen for C. W. D. the distance of the points to begin with was 21 mm., but the average distance of the last week was only 4 mm. For F. B. D. at the beginning of the work, the points must be separated 33 mm., while the average distance of the last week was only 3 mm.

TABLE I.

DATE.	C. W. D.		F. B. D.	
	A. M.	P. M.	A. M.	P. M.
October 11,	22 mm.	24 mm.	29 mm.	26 mm.
" 12,	19 "	20 "	33 "	15 "
" 13,	17 "	22 "	18 "	18 "
" 16,	17 "	17 "	18 "	16 "
" 17,	18 "	16 "	18 "	15 "
" 18,	15 "	18 "	13 "	10 "
Av. Sensit. 1st week,	13 "	19.5 "	21.5 "	16.6 "
October 20,	22 mm.	18 mm.	10 mm.	15 mm.
" 22,	15 "	12 "	16 "	13 "
" 23,	11 "	15 "	12 "	10 "
" 24,	12 "	12 "	10 "	13 "
" 25,	8 "	7 "	7 "	7 "
" 26,	10 "	11 "	5 "	5 "
Av. Sensit. 2d week,	13 "	12.5 "	10 "	10.5 "

October 27,	10 mm.	8 mm.	9 mm.	8 mm.
" 29,	5 "	8 "	7 "	7 "
" 30,	4 "	6 "	4 "	9 "
" 31,	4 "	8 "	4 "	4 "
November 1,	5 "	2 "	4 "	5 "
" 2,	5 "	4 "	5 "	4 "
Av. Sensit. 3d week,	5.5 "	6 "	5.5 "	6.1 "
<hr/>				
November 3,	5 mm.	6 mm.	3 mm.	3 mm.
" 4,	5 "	5 "	3 "	3 "
" 6,	4 "	4 "	2 "	3 "
" 7,	4 "	3 "	3 "	1 "
" 8,	2 "	4 "	2 "	2 "
" 9,	5 "	3 "	4 "	2 "
Av. Sensit. 4th week,	4.1 "	4.1 "	2.8 "	2.3 "
<hr/>				
Gen. Av. Sensitiveness,	10.1 mm.	10.5 mm.	10 mm.	9 mm.

It was thought at first that there would be developed quite a difference in sensitiveness in both subjects between the morning and the evening sittings, and it can be seen by reference to the table that there is considerable difference in the averages for the first week, C. W. D. showing a greater sensitiveness during the morning sittings, while the opposite was true for F. B. D. It is interesting to note that this variation quickly decreased with practice and that after the first week there is practically no difference, thus following what might be termed a general law in education, viz.: one of the first effects of practice is to eliminate accidental hindrances, to render the work more and more mechanical and so less subject to variations due to unconscious influences. By referring to the general average, it is seen that the completed results for the month retain this preference in a slight degree, but it must be understood that these differences are due to those that arose in the first week.

Perhaps it would be well to call attention here to the absolute difference in sensitiveness, as shown by the table, for the two subjects. A great deal has been said about the comparative sensitiveness of the sexes with regard to touch, though a very few of such statements have been based on the results of accurate experiments. It would be wholly unscientific for me to make any general statement in regard to this question, simply because my experiments have been limited to two subjects. However, for these two the following may be said: In the first place it must be remembered that the part of the arm chosen for C. W. D. (a woman) was absolutely more sensitive than the part of the arm chosen for F. B. D. (a man), because that of the former was nearer the wrist than that of the latter. Hence the difference in sensitiveness at the start betwixt the two subjects should not be construed to mean that one or the

other had the greater power of discrimination, but that difference arose chiefly because of the different positions the parts exercised in the two subjects. By keeping this difference in mind, it is clear that F. B. D. showed in a slight degree the greater sensitiveness of the two. That is, he began with a part of the skin of less normal discriminative ability and succeeded in reaching a slightly higher discriminative ability than C. W. D.

Bilateral Transference of the Effects of Education of Skin. One of the chief purposes of this work, as has been said, was to test the transference of the effects of exercise of the skin on one arm to that of the corresponding part of the other. These tests were made, as exhibited in the table after a month's work, and after it seemed that the approximate limits had been reached with regard to discriminative ability. The method used in making the tests was as follows :

The æsthesiometer was set for first test at 5 mm. whenever both points were pressed against the skin, in order to test what was considered the possible limit for discrimination before any exercise in testing might render the part more sensitive. Out of forty-five observations C. W. D. made but eleven errors, thus showing clearly that even when the points were so near, they could be recognized as two in seventy-five per cent. of the cases. The next day another series of forty-five observations was taken with the points separated by 33 mm., when it was shown that eighty per cent. of them could be judged correctly. The first test made for F. B. D. was likewise for 5 mm., and out of fifty observations eighty per cent. of them were correctly judged. In order to show more clearly the transference of the increased ability to the corresponding part of the opposite arms, the following table is introduced, which shows the ability of both arms before and after the month's exercise.

BEFORE EDUCATING.

C. W. D.				F. B. D.			
Date.	Time.	Left Arm.	Right Arm.	Date.	Time.	Left Arm.	Right Arm.
Oct. 10.	8 A. M.	21 mm.	21 mm.	Oct. 10.	8 A. M.	33 mm.	33 mm.

AFTER EDUCATING.

Date.	Time.	Left Arm.	Right Arm.	Date.	Time.	Left Arm.	Right Arm.
Nov. 10.	8 P. M.	5 mm.	5 mm.	Nov. 11.	8 A. M.	2 mm.	5 mm.

From the result of these tests it is plain that either the education had crossed over, so to speak, or that the general discriminative ability had been wonderfully sharpened. That it was not largely due to the latter was proved in the following

ing way: Tests were made on the neighboring parts of the skin to those educated at the end of the month's work and the discriminative ability found to be much inferior to that of the part exercised and its corresponding part on the other arm. It is no doubt true that the general sensibility was slightly increased, but just how much, it is somewhat difficult to say. Unfortunately no general tests were made before the experiments began, to test whether or not the sensibility had been raised for distant parts of the body, such as one might naturally expect from education of the attention for this special kind of discrimination.

Other evidence, however, unexpectedly came out during the course of the experiments to prove that the education was limited to the locality in question. As was stated above tests were made on the territory surrounding the exercised part and its corresponding part on the other arm, which not only revealed the fact there mentioned, but also a marked difference in the *quality* of the local sensations; more striking in the arms exercised, but distinguishable in the others also. So much so that the subject could locate with a fair degree of accuracy the boundary of the exercised part merely through this acquired quality in the local sign. This difference in quality is best illustrated by the difference in the quality of the sensations, easily recognized, which arise from touching a finger-tip and immediately afterward some point on the back of the hand. There is not only an immediate recognition of a difference in location, but there is a different feeling present in the two cases. By touching the tips of two neighboring fingers, or for that matter one on each hand, the difference in the quality of the *feeling* is much less marked than in the former case. The change in the quality of the sensation, as Wundt¹ remarks, "is gradual as we pass from one point to another on the skin," and especially on the less discriminative parts of the skin is the change slow. Thus it was that this difference in the quality of the sensations was especially noticeable on the arms, for the change was so marked and so rapid near the boundary of the territory exercised and its corresponding part on the other arm that it attracted the attention of both subjects.

Given this change in the quality of the sensations, the question naturally comes, how did it arise? Surely not in a change in the fundamental structure of the end organ, because the time required for the development of this qualitative difference was too short for such changes. It must be that this rapid change was due to functional changes in the end

¹Vorlesungen über Menschen und Thierseele. Vol. I. p. 214.

organ. The greatly increased exercise of the part would naturally tend to a larger development of these organs and hence there would come a quicker response to the stimulus, and especially a greater amount of nervous force would be liberated by a given stimulus. Wundt¹ says "the difference in the structure of the sense organ is the chief cause of the different quality of the sensation. Just as differences in tone and color fundamentally depend on the structure of the end organ in the ear and in the eye, so all qualitative differences which belong to any single organ have their necessary basis in the smaller variations, which may appear in the structure, or in the arrangement of the end organ."

The Rate of the Loss of this Increased Sensibility. Another purpose of this research, as stated above, was to determine as nearly as possible the rate of the loss of this increased sensibility, after the special exercise had ceased. It is clear that an exact curve of this kind can never be found, simply because the tests made to determine the exact state of the sensibility would naturally prevent the loss of the increased power as rapidly as it would occur without the tests. While, in a general way, it was known that the loss was rapid, no definite observations had previously been made, so far as I knew, on this special point. In determining how often the tests should be made in order to approximate the normal curve of such a relapse, there was, therefore, no help to be derived from well defined facts bearing on the question. It was decided, however, that the first test should be made one week after the special exercise had ceased, and let the results of this test aid in directing further experiments. It will be seen by the accompanying table that it was a mistake to wait so long before making the first test, for the decline at the first was very rapid, and decreased in rapidity inversely as the time. Later, tests were made more often, but varied in time, if, perchance thereby, a few points in the curve could be hit upon. But, as will be seen by the table given, the decline of this acquired sensibility was much more rapid than had been anticipated, and hence no definite curve can be given. The results here tabulated, however, give something of a glimpse of the true curve and in two places show that it was nearly reached. This table is of value chiefly as a guide to other workers in this line, though it is not without some value as an approximation to the facts sought.

¹ Op. Cit., p. 217.

TABLE SHOWING THE DECLINE OF THIS ACQUIRED SENSIBILITY.
For F. B. D.

DATE.	SPACE BETWEEN POINTS.	JUDGMENTS.	
		Right.	Wrong.
Nov. 10.	5 mm.	40	10
" 11.	10 "	42	8
" 19.	5 "	25	25
" 22.	10 "	35	15
" 26.	15 "	32	18
Dec. 4.	20 "	32	18

Explanation of Table. On November 11 the tests showed that when the points of the æsthesiometer were separated by a space of 5 mm., eighty per cent. of the judgments were correct; on the same day in the evening with the points at 10 mm. apart, eighty-four per cent. of the judgments were correct. On November 19, with no intervening exercise, it was wholly impossible to distinguish with the æsthesiometer set at 5 mm., and three days later less than seventy-five per cent. of the judgments were correct at 10 mm., and so on for the other days, as indicated.

Observations. Throughout the entire series of experiments, note was taken of all subjective experiences which it was thought might elucidate, or direct in the experiments. The following statements summarize the chief points noted:

1. Some time must elapse between two successive applications of the æsthesiometer, so as to give time for the after-images to die away, otherwise these will fuse with the primary image and render differentiation much more difficult.

2. The discriminative ability is less acute at the beginning and close of the sitting than toward the middle. After a few experiments at the beginning, the sensibility increases with an increased flow of blood to the parts, while toward the close there is a slight dulling caused by fatigue.

3. Under certain conditions, not yet fully determined, much greater pressure was required to render the sensations clear and definite.

4. The same amount of pressure on different parts of the arm gives very different degrees of pain. That is to say, the normal amount of pressure on a certain part giving no pain, would, if exerted on another place, produce a sharp pain. It seems that this difference is not wholly due to difference in the thickness of the skin, but to inherent difference in sensibility to pain.

5. The discriminative ability is finer when the two points of the æsthesiometer are pressed against the arm in a cross direction than when they are pressed lengthwise. (Kotten-

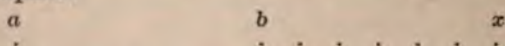
kamp and Ullrich found that this preference for the cross direction, for the upper extremities, over the lengthwise direction, was on the flexor side one-eighth, and on the extensor side one-fourth.)

6. Care was required during each sitting to keep the arm in a natural position, else the stretching of the skin rendered the sensibility abnormally acute.

III. *Experiments on Open and Filled Space for Touch.*

Tests with Active Touch. It is a well known fact that if two equal adjacent spaces are compared by the eye, the one filled and the other empty, the filled space will appear the greater. The purpose of the following series of experiments was to determine whether this would hold good for the sense of touch. That is, whether, of two equal spaces the one offering the greater number of stimulations to the touch organs, as the finger is moved along through the spaces, would be judged the greater or not.

It is, perhaps, well to explain a little further here what is meant by open and filled space for touch. Of course, in strict sense, there can be no empty or open space for touch. But by open space in this connection is meant a smooth homogeneous surface, such as the surface of a bit of well-sized cardboard. By filled space we mean the same sort of a surface punctured from the under side, so as to offer to the sense of touch a series of additional and sharply defined sensations. The question arises here, how should these spaces be situated with regard to each other? Is there any source of error in the method adopted from Professor James, of dividing a line into two parts, marking a puncture at each extremity, and filling one part of this line with punctures and leaving the remainder for the open space, and thus be able to pass the finger over both spaces with one continuous movement? To illustrate, let the space between the dots at *a* and *b* in the accompanying diagram represent the open space, and that included between the dots at *b* and *x* represent the filled space:



This question might be raised: If the tip of the finger be passed from *a* to *x* and the subject be asked to judge of the comparative lengths of the spaces *ab* and *bx*, is it not to be expected from a physiological reason that *ab* would seem shorter, because its length would be judged on the basis of the sensations received from the time the *last* sensitive point of the finger tip left the puncture at *a* until the *first* one

touched at *b*? We should answer yes, and were it not that exactly the same thing occurs in case of the filled space, there would be a permanent source of error in the method, even for relative judgments. That these relative judgments have not been influenced in this way is plainly evidenced by the fact that the apparent length of the filled spaces depends on the *number* of punctures. Then, too, it might be supposed that retardation of the movement due to friction as the finger-tip passed over the roughened filled space would have a tendency to make the filled space seem longer, but that this was not the case is the testimony of all the subjects, because the filled spaces were not so rough as to prevent free movement.

This, too, is proved by the fact that increase in the number of punctures, although increasing the apparent length of the space in most instances, did not increase the roughness of the space, but on the contrary made it more easy for the finger to pass over. With these apparent objections out of the way, let us pass directly to the problem.

Prof. James¹ says, "If one divide a line on paper into equal halves, puncture the extremities and make punctures all along one of the halves; then, with the finger-tip on the opposite side of the paper, follow the line of punctures, the empty half will seem much longer than the punctured half." He does not say how often this was tried, or whether the results obtained were the same, when the finger passed first over the punctured half and then over the open, as when it passed in the opposite order. Neither is there anything said of the absolute lengths of the spaces compared. Thinking that these conditions, if they were introduced into the experiment, might influence the results, I have carried on a series of experiments in the following way to determine the influence of these conditions:

Apparatus. (1) Eighteen cards were made according to Prof. James' method from well-sized stiff card-board and about the same width, with the spaces to be judged similarly placed for each card. (2) The spaces to be judged ranged from 2 cm. to 16 cm., as can be seen in the accompanying table. (3) A number of cards were varied only in the number of punctures in the same space. That is, the punctures in these cards were made closer together. The punctures were all made with the same point, and, on any given card, in the punctured space were equally separated from each other. It seems probable, at any rate, that whatever the results might be, the introduction of these variables would be

¹Psychology, Vol. II. p. 241. (Loeb found the opposite to be true for rough and smooth threads. Pflüger's Archiv, Vol. XLI. p. 121.)

helpful in arriving at some explanation. (4) Almost an equal number of experiments were taken with each of the spaces presented first, for it was thought perhaps that the after-images of the punctures when presented first might appear to lengthen the punctured part.

The first series, that of C. W. D., was made on a wholly naïve subject, and at no time were the cards permitted to be seen or the purpose of the experiment mentioned. The cards were never presented in the same way two successive times and the short ones were distributed so that no possible means of establishing an incidental and illegitimate means of judging was brought in. The other series given in the table were made on men who had thought and read more or less on the point, and could not be said to be ignorant of the purpose of the experiment. Their records, however, show that they judged according to their sensations, and hence they are in the main uniform.

TABLE II. Part 1.
COMPARISON OF OPEN AND FILLED SPACE THROUGH SENSE OF TOUCH.

		a. Open Presented First.				b. Filled Presented First.				
		4	9	4	7	13	9	9	11	6
		2-2	2-2	3½-3	3½-3	3½-3	4-4	4½-4	4½-5	5-5
C. W. D.	a	1 18	0 19	7 10	3 17	3 15	3 16	2 17	0 15	3 17
	b	1 18	0 17	4 14	4 15	2 16	1 13	3 13	0 12	4 12
J. A. B.	a	0 2	0 3	0 2	0 3	0 3	0 3	0 3	0 3	0 1
	b	0 2	0 2	2 0	1 1	0 2	0 0	0 1	1 1	1 0
J. H. L.	a	0 3	0 3	0 3	0 3	0 3	0 3	0 2	0 3	0 3
	b	0 3	0 3	2 0	1 2	0 3	1 1	0 3	2 1	0 2
J. S. L.	a	1 2	0 4	2 3	2 2	2 2	1 3	1 4	0 3	2 0
	b	0 2	0 2	1 1	0 2	0 2	0 2	0 2	0 2	0 1
J. A. H.	a	0 3	0 3	0 3	0 3	0 3	0 2	0 3	0 3	0 3
	b	0 2	0 2	1 1	0 2	1 1	0 2	1 1	0 2	1 1
H. A. A.	a	1 1	0 2	3 0	4 0	1 3	0 3	1 3	2 2	2 3
	b	0 2	0 2	0 2	0 3	0 1	1 0	1 2	0 2	2 1
F. D.	a	2 1	1 1	2 0	2 2	2 2	1 3	0 4	1 3	2 0
	b	0 4	0 2	1 3	1 4	0 4	0 0	1 4	3 1	4 0
Totals.		6 63	1 65	25 42	18 59	11 60	8 51	10 62	9 53	21 44

Explanation of Table. Parts 1 and 2. The numbers 4, 9, 4, 7, etc., at the head and on the right of the double columns indicate the number of punctures made in the number of cm. denoted by the number immediately below in the same column. Thus, in the first double column, the figure 4 at the head and on the right indicates that in the space of 2 cm. of the card there were four punctures; in the second double column the figure 9 indicates that in the number of cm. denoted

TABLE II. Part 2.

COMPARISON OF OPEN AND FILLED SPACE THROUGH SENSE OF TOUCH.

		a. Open Presented First.				b. Filled Presented First.					
		11	13	7	10	19	12	13	14	16	TOTALS.
		5-5	5-6	7-6	11-9	11-9	9-11	12-12	14-13	16-15	Open Filled
W. D.	a	1 11	0 16	6 10	12 1	9 6	7 8	6 5	8 4	17 11	66 62
	b	4 8	2 12	3 11	8 2	7 7	6 7	9 2	7 6	21 2	67 57
A. B.	a	0 3	0 2	1 2	3 0	1 2	0 2	0 2	0 3	1 2	6 18
	b	1 0	0 2	2 0	2 0	1 0	1 0	0 0	0 1	1 0	8 3
H. L.	a	0 3	0 3	0 3	0 3	1 2	0 3	0 3	0 3	0 3	1 27
	b	1 1	1 3	3 0	3 0	3 0	3 1	2 0	3 0	3 0	22 5
A. L.	a	0 2	1 3	4 0	3 0	5 2	1 3	3 1	2 2	3 1	23 14
	b	0 2	0 2	1 1	2 0	2 0	0 2	1 1	1 1	1 1	8 10
A. H.	a	0 3	1 2	1 2	1 2	1 2	0 3	0 1	0 3	0 2	4 20
	b	1 0	0 2	2 0	2 0	2 0	2 0	2 0	2 0	2 0	15 4
A. A.	a	0 2	0 4	2 1	2 1	2 1	1 3	1 3	0 3	0 3	8 21
	b	0 1	0 1	2 0	2 0	2 0	2 0	2 0	2 0	2 0	14 2
D.	a	1 0	1 3	4 1	4 2	3 3	1 4	0 2	1 3	0 3	15 21
	b	1 0	3 1	4 0	4 0	4 1	4 0	2 0	3 1	2 2	31 5
Totals.		10 36	10 56	35 31	48 11	43 26	28 36	29 20	29 30	53 30	

by the figure immediately below there were nine punctures, etc. Thus, in the first two double columns, the cards were exactly alike save in the number of punctures in the filled side, the one having but four, the other nine, punctures in 2 cm.

Results: With C. W. D., out of nineteen judgments between an open space of 2 cm. and the same amount of adjacent space punctured with four holes equally separated, eighteen judgments were made in favor of the punctured space being longer, to one in favor of the open space being longer when the open was presented first, and exactly the same proportion when the punctured was presented first. In the second column the same length of spaces was used (that is, 2 cm. for the open and the same for closed), but instead of only four punctures there were nine in the filled space. Here it will be seen that the filled space was judged to be longer every time for C. W. D., and by referring to the accompanying table, it will be seen that out of a total of sixty-six judgments for this card sixty-five of them were in favor of the filled space being longer. In the next three columns, that is, in the third, fourth and fifth, are tabulated the judgments rendered on three cards, all of which were exactly alike save in the number of punctures in the filled space. It will be seen, by referring to the totals in these columns, that increase in the number of punctures in the same space increased the number of judgments in favor of the filled space being the longer. Thus C. W. D., in the

first column of this set, made seventeen judgments; ten were in favor of the filled space being longer, and seven in favor of the open. In the second column of the sets twenty judgments were made, seventeen in favor of the filled space and three in favor of the open. In the third column of the set eighteen judgments were made, with fifteen for the filled and three for the open. By glancing at the totals at the bottom of these columns, it will be seen that these proportions were more than borne out by the other subjects. That is, the totals show that with the increase in the number of punctures in the same space, the space appeared longer even in the third column of the set. These judgments, it must be remembered, were between two spaces: $3\frac{1}{2}$ cm. of open space and 3 cm. of filled space; so that, even if the number of judgments were equal for the two, it would show a preference for the filled space. But when it is seen that under these conditions, the totals show the ratios of preference to be $1\frac{2}{3}$, $3\frac{1}{3}$ and $5\frac{1}{2}$ for the filled spaces respectively, it makes a clear demonstration, not only that filled space seems longer for active touch, but that the greater the number of separate sensations received in the filled space, the longer it appears to be. Now, it is clear that this increase in the apparent length of the filled space would not indefinitely increase with the increase in the number of punctures, but that there would be a point reached where these would not be separately distinguishable and the number of the sensations would diminish rather than increase, so that we should have a right to expect a decrease in the number of judgments preferring the filled rather than the open as the longer.

This same influence of the number of punctures, or in other words, the number of distinct sensations received in passing over the same amount of space, is seen by comparing the last double column of judgments in Part 1 with the first in Part 2. Here the spaces in each case were the same; that is, 5 cm., but in the former there were but six punctures in the filled space, while in the latter there were eleven. By glancing down these columns, it is plain to see that the filled space in each case was preferred, but much more often relatively in case of the card with eleven punctures. The totals show the ratios of the judgments to be two and one-tenth and three and two-fifths respectively. That is to say, there were two and one-tenth judgments, in the first case, in favor of the filled to one in favor of the open, but in the second there were three and two-fifths to one in favor of the filled. It seems that no further explanation of the results bearing on this point need be made, but that this is seen to be true, within certain limits, in all parts of the table. However, it is clear that as the

spaces to be compared increase in length, the influence of the filled space is less and less marked. That is to say, when the spaces to be compared are more than 10 cm. in length, the illusion does not hold so steadily. For example, the open space is judged longer more times than the filled where each space was 12 cm., although the filled had thirteen punctures. It is evident that, with the longer spaces, the subjects were guessing quite largely, though in a general way the filled spaces were preferred for the longer. The notes I made at the time of making the experiments are valuable here. With no exceptions, the subjects complained of the length of these longest cards, constantly saying they had no sure basis for their judgments, often saying that the judgment was a mere guess. Especially, they complained of the long open space as being so indefinite, because, in passing the finger over it, there was a sort of anxious effort to guide the finger through this blank, so to speak, in order that it would strike clearly the first puncture of the filled space. The hesitancy and caution here introduced would have a tendency to make these longer open spaces appear longer than they otherwise would, and in this manner in a slight degree, at least, vitiate the results. However, as has been said, even, in a general way, the longest open spaces used do not seem so long as equally long filled spaces.

The next question is, what difference in the judgments arose when the open space was presented first as compared with those where the filled came first? Or, in other words, when two equal spaces are to be compared as to their length by touch, one filled and the other open, does it make any difference in the judgments which is given first? This question can be answered by saying that when the spaces to be compared are short, less than 5 cm. each, there is practically no difference, but that above this, as far as we went, there is quite a difference, though varying slightly with different subjects. By examining the double columns of totals on the right of each of the foregoing tables, these statements are made more clear. Take, for example, the first subject again, C. W. D., it will be seen that in Part 1 the whole number of judgments rendered in favor of the open being the longer was twenty-two when the open was presented first, and nineteen when the filled was presented first, and 144 and 130 respectively in favor of the filled as being the longer. Of course, twenty-two and nineteen can not be directly compared unless the whole number of judgments in the two cases were the same. As this was not the case, the ratios of the judgments in the two cases should be compared and these are found to be the same, viz.: one to four and five-tenths in

each case. By following down the columns further in the same part, it will be seen that for some subjects there seems to be quite a difference, even when the spaces were less than 5 cm., but as there were comparatively few experiments made on each of these subjects, the difference ought not to count for too much. But in Part 2, it will be seen that for every subject save one, when the filled space was presented first, there was an increase in the number of judgments preferring the open spaces as the longer. Just why this should be so is not so easy to say. But it is perhaps due to the rapidity with which the sensations die out as the finger is moved through the spaces. Here again, the notes made during the progress of the experiments are helpful in making an explanation. It was observed that the longer the spaces compared, the greater the number of trials that were required for the subjects to come to a decision. Not only this, but each of them remarked that the long ones were very indefinite, and that by the time the finger was passed over both spaces, no definitely clear notion of the length of the first one was left.

Tests with Passive Touch. The problem of getting regular and steady motion of the proper rate for passive touch occupied much of my time before it was finally and satisfactorily solved. The conditions which it was necessary to satisfy were: (1) That the motion should be of uniform rate; (2) that it should be unaffected by any slight friction, and (3) that the card should thus be brought in contact with the finger with sufficient and regular frequency. Happening to be in the shop of the university mechanic one day, I noticed that in the movement of a planer for metals, which was run by a steam-engine, the above conditions could be easily fulfilled. Its movement was horizontal and could be regulated as to speed and frequency of return, while at the same time the apparatus was so massive that the motion would not in the least be influenced by a small amount of friction. All that was required, then, in order to make this serve the purpose, was to arrange a stationary arm rest, and to fasten a rack to the moving bed of the planer to hold the cards. Having made these arrangements, the conditions were then fully satisfied and the same tests could be made with the cards for passive touch that had been made for active touch.

The subjects were seated at right angles to the moving bed of the planer with the arm at rest. By simply extending the finger tip over the end of the arm rest, it was thus brought into contact with the punctures of the moving card. In this case, as in the tests with active touch, the finger-tip was kept in contact with the card only when the motion was in one

etion. To do this, the finger was raised from the card in the backward motion of the planer. The subject thus had opportunity to try as often as he wished before coming to a decision as to which of the two spaces was the longer. In all the tests with passive touch, the cards were presented with the open space always coming first. It was deemed unnecessary to take any tests with the spaces reversed, because of the negative results with this variation in the tests with active touch. The subjects were blindfolded during the tests and directed to make their judgments wholly upon the basis of their sensations.

Under these conditions, as stated, it is very clear that all error in determining the relative lengths of the two spaces, arising from the sensations derived from voluntary movement, was eliminated. That is to say, the various judgments the subjects were based only on the sensations of touch. Cards with punctures, as previously described, were used, because with these there was much less likelihood of introducing an element of pain. As soon as the points became dulled by repeated touchings, new cards were substituted, which kept the spaces constant and their relations unchanged. The following table shows the judgments of the subjects so named, under the above conditions:

	2 $\frac{1}{2}$ -2	2-2 $\frac{1}{2}$	2 $\frac{1}{2}$ -2	2-2 $\frac{1}{2}$	2 $\frac{1}{2}$ -2	2-2 $\frac{1}{2}$	3-2
	0 8	0 8	4 4	0 8	6 2	0 8	5 3
	0 3	0 3	0 3	0 3	0 3	0 3	1 2
	0 3	0 3	0 3	0 3	0 3	0 3	0 3
	0 5	0 5	0 4	0 4	2 2	0 5	0 5
ers.	0 2	0 2	0 2	0 2	0 2	0 2	0 2
als.	0 21	0 21	4 16	0 20	8 12	0 21	6 15
	3 $\frac{1}{2}$ -3	3-3 $\frac{1}{2}$	4-3	4-2 $\frac{1}{2}$	2 $\frac{1}{2}$ -4	4-3 $\frac{1}{2}$	3 $\frac{1}{2}$ -4
	0 9	0 9	3 6	4 5	0 9	2 7	0 9
	0 3	0 3	1 2	0 3	0 3	0 3	0 3
	0 3	0 3	0 3	0 3	0 3	0 3	0 3
	1 4	0 5	3 2	5 0	0 5	2 3	1 4
ers.	0 4	0 4	0 4	1 3	0 4	0 4	1 3
als.	1 23	0 24	7 17	10 14	0 24	4 20	2 22

Explanation of Table. This table is identical in plan with the one given for active touch, save in this one there is no specification of the number of punctures in each filled space. The number was not indicated, because the punctures were made equally far apart in each case.

Results. There were 312 judgments under these conditions, of which 270 were in favor of the filled space. This too, in view of the fact that the open spaces of the fourteen cards used were really longer than the filled. If we consider the

judgments made, when the open spaces were always longer than the filled spaces with which they were compared, we will still find a great preference for the filled space. The number of judgments made from a comparison of the spaces on the eight cards whose open spaces are always longer than their filled spaces, is 178. Of this number the filled spaces were judged to be the longer 138 times, while the various open spaces were judged to be the longer only forty times. It is a waste of space and time to take up each card and show that in every case the totals give a greater number of judgments for the filled space. If the reader will only glance at the results of each column this fact will be manifest.

Tests when the Tactile Surface of the Finger-tip was Limited. It was mentioned previously that there might seem to be a fault in the method thus far used, in that when the entire width of the tip of the finger was brought into contact with the points, the notion of the length of the open space would be formed from the space passed over from the time the *last* sensitive point of the finger left the point at the beginning of the open space, until the *first* sensitive point touched the beginning of the closed space; thus, of course, making the open space appear shorter than it really was. While, as was said, it was thought that this was counter-balanced by the same thing happening with the closed space, to prevent any possible error in the results, the following variation in the method for passive touch was introduced. It is plain that if there be any fault in the method as indicated, it could only be remedied by limiting the amount of the tactile surface of the finger-tip to the space of a single sensory circle. This was accomplished in the following way: The arm-rest previously mentioned was lengthened so as to pass entirely across and above the moving card-rack; directly over this moving rack, a slot was cut in the board forming the arm-rest and into this slot a strip of tin was fitted so as to extend downward below the lower side of the board and approach the surface of the card. Through this tin, a hole was made just so large that when the finger was pressed against it, a portion of its sensitive surface, less than 2 mm. in diameter, protruded and came into contact with the punctures of the card passing along underneath. By this means, not only was the possible error in the results avoided, but all motion of the finger, either real or suggested, was wholly eliminated. All the conditions of this series of tests were the same as those in the previous tests, save the variations above described. It perhaps might be well to say here that punctured cards were used throughout the whole series of tests.

The tests made under these last conditions give the following condensed results: By using cards made and spaced exactly as those used in the tests with passive touch when the finger-tip was not limited, out of 200 judgments, only thirty-three were in favor of the open, while 167 were in favor of the closed being the longer, thus maintaining practically the same proportion of preference for the closed as in the tests with the other methods. These judgments were purposely obtained from subjects who had not served for me in the previous tests, in order to be sure that they be completely naïve.

The observation made by Herr Mellinghoff, as reported by Wundt,¹ to the effect that if the space between two points be divided into two equal parts by a dot, and then one of these parts be again divided into halves by another dot, the half of the whole space thus filled seems shorter for the eye than the open half, does not hold true for touch. To see whether or not this held for touch, I made a series of experiments with cards prepared to meet the above conditions, but found in the tests made thereon that the preference was as marked for the filled space under these conditions as it was in any of the previous tests for passive touch.

The question now arises, what is the meaning, from a psychological point of view, of these results? In the first place, it is plain that a closed space for the sense of touch, just as for that of sight, appears longer than an equal open space. Hence the explanations heretofore given, wholly based on the difference in time the eye used in passing over opened and filled space, must stop short of the real truth. That this illusion holds for touch is, I think, completely demonstrated by my experiments in passive touch, and in these the time used in passing over equal spaces was exactly the same. Hence, differences in this element cannot here enter into the explanation. The fact is, that in the comparison of these two separate trains of sense-perceptions, the one which was the fuller of definite and distinct sensations seemed the longer, though equal time had been consumed in the passage of each train across the focus of consciousness. All have noticed how much longer a road seems when first passed over, than it does after it becomes a familiar and often traveled road. The first time it is passed over, everything appears in consciousness, even to the minor details of the scenery, but as it becomes more familiar, there come to be only a few points, such as half-way places, or some peculiar objects, which are noticed at all, while the

¹ *Physiologische Psychologie*, 4th ed. Vol. II. p. 142.

stretches between are as open spaces. One cannot read Stanley's¹ description of the great African forest without feeling the vastness of it, chiefly because he makes one crowd into a small space of time, images of multitudes of plants and shrubs and trees and insects, and then reminds you that all you have in mind must represent only those things seen within a small radius, for you cannot even see through to the sunlight. Besides, he merely mentions and hastens, leaving the reader with only suggestions from which to bedeck the trees with their varied foliage and festoon them with myriad vines.

In the tests I made with the cards, it was not only found that of two equal spaces, the one open and the other filled, the filled one seemed the longer; but, also, it was found that of two filled spaces of equal extent, the one more completely filled seemed the longer. It must be remembered, however, that "more completely filled space" in this connection means capacity to present to the subject in a given time a greater number of distinct sensuous stimuli. It is clear that if too great a number of distinct stimuli be poured in upon the subject in a given time, they would drop below the threshold of differentiation, and such a train would be classed with those which arise from what we have termed open space. For example, if we run the fingers rapidly over a piece of coarse cloth or a wire screen, instead of a number of distinct sensations, there is but one continuous undifferentiated sensation; or if we glance over the lawn, the sensation is as if there were but one continuous stimulus, though we know it to be produced by the fusion of the stimuli from countless grass-blades.

The conclusion to which our results lead us, then, is that if we have given two trains of sense-perception produced in the mind in equal durations of time, the one which is made up of the greater number of distinct sensations requires, of the mind a greater amount of space in its reference to the external world, provided, of course, that these trains are special at all.

It should be noted that this generalization says nothing of the origin of our notion of space, but only calls attention to the fact as exhibited in adult mental life. As to whether this be an inherent mental tendency or the result of habitual associations, the tests here made have nothing at all to say. There is thus offered abundant opportunity for speculation and hypotheses, but we prefer to run no risk in parting company with the truth and so will be content to await the outcome of a continuation of these investigations.

¹ In *Darkest Africa*, Vol. II. p. 72.

IV. *Illusion for Weights. A Study in Association and Apperception.*

It has long been known that of two bodies of equal weight, but of different size, the smaller one appeared the heavier when lifted. The purpose of the following series of experiments was to determine how the perception for certain weights could be influenced when (1) they were of the same *shape* and *weight*, but different in *size*, and (2) when they were of the same *size* and *weight*, but different in *shape*. The well-known experiments of E. H. Weber¹ and Fechner² will be recalled here. They experimented with weights of the same *shape* and *size*, but of different absolute *weight*. The purpose of these workers was to determine the fineness of discrimination for difference in weight. The work here described is of a very different nature from theirs, nevertheless the results obtained show the need of great care in the preparation of weights for such work as theirs and in so far is supplemental to their work. The purpose of this work, however, was to make a study in association and apperception by making use of one of the strongest associations we make in life, viz., that of two given weights of the same material the larger is the heavier. This association we have firmly fixed because of the often recurring and immediate sequence of the ideas. Indeed, if it could be said of any association, that it is inborn or racially fixed, it could be truly said of this one.

*Tests on School Children.*³ Upon these a study was made concerning the illusions arising from comparing weights of the same shape and weight, but different in size. To make this study, I constructed some weights in the following way: A brass tube, a little over an inch in diameter, was cut into small tubes of the following lengths: No. 1, 1½ in.; 2, 2 in.; 3, 2½ in.; 4, 3 in.; 5, 3½ in.; 6, 4 in.; 7, 4½ in.; 8, 5 in. They were each made to weigh 132 grams by filling them with different substances and were all capped over with carefully fitted bits of brass. Care was taken to distribute the weight throughout the whole of each tube, so that one end

¹Der Tastsinn und das Gemeingefühl, Wagner's Handwörterbuch der Physiologie, 3, p. 543 et seq.

²Elemente der Psychophysik, I. p. 183 et seq.

³I am greatly indebted for the material here used to Mrs. Grace B. Sudborough, Principal of the Teachers' Training School, Omaha, Nebraska, who carried out under my direction this long series of tests on the public school children of Omaha. Her work of making the tests was of the most painstaking and careful kind and deserving of the highest commendation. Also, my thanks are due to Supt. Fitzpatrick, of the city schools of Omaha, for his interest and coöperation in the work.

should not be heavier than the other and thus introduce a disturbing element when they were lifted. There was nothing in the external appearance of any of them which would in any way suggest a difference in weight save their difference in size. It will be seen that the tubes increased in length regularly by half an inch and when set up they formed a series of regularly ascending steps. The design of this arrangement was to determine what effect such a relationship would have in suggestion so as to continue the illusion for the whole series. The suggestion thus made became so strong that, when the subjects began to see that the tubes could be arranged in a regular order, in many instances the judgments were changed and the tubes were made to occupy a place according to size. In some cases, however, this suggestion had the opposite effect; that is, it caused a disarrangement which would probably not otherwise have been made, simply, as the subjects afterward acknowledged, because they thought it improbable that the tubes had been made so as to decrease in weight so regularly. These, of course, were not strictly legitimate judgments, and should be cast out of the results wherever they are known to exist. But as there were very few of such they have not appreciably influenced the final results. In all cases where the subject permitted his judgments to be made from the sensations purely, the arrangement is probably without a variation, in order of their size. So with most of the subjects the illusion was strengthened by attracting their attention to this regular difference in size. Constant care was taken in making the experiments not to suggest to the subjects that they were given a puzzle to work, or that there was anything strange about the experiment. The tubes were put before them and they were simply asked to "arrange them in order of their weight." These words were used because it was thought they were freest from suggestion. The subjects thus began the experiment with the notion that it was a test in fineness of discrimination for weight. The tests were made under the most favorable circumstances possible. No one was permitted to be present but the operator while the tests were being made, in order to prevent, especially in the case of the younger children, any possible confusion or unnaturalness. They were directed to lift each weight in the same way, that is, by taking them between the thumb and finger and lifting them straight up, in order to prevent any physiological basis for a difference in sensations. No more restrictions were placed upon them, however, than were necessary to insure the introduction of no error. After the weights had been arranged in the order, as they thought, of their weight, they were

asked to make a comparison of the weights in relative terms, of the first and the last of the series. These comparisons have all been entered in the tables.

Before the work began, the children were divided into three classes according to their general intelligence. The brightest were grouped together to form class I, the good to form class II, and the dullest were put in class III. This classification was made by their teachers, and was unknown to the children. They were simply marked in the tables as belonging to a certain class. Also, their age, sex, nativity and physical development were noted, as well as the nativity and occupation of the parents. All this but the latter will be seen by a glance at any one of the tables given.

The following tables show the arrangements of the weights and their comparisons for the groupings with reference to grade of intelligence, sex and age.

CLASS I. BOYS.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	3
8	5	4	2	1	3	6	3	7	3
1	1	3	2	4	5	6	7	8	3
1	1	2	3	4	5	6	7	8	4
1	1	2	2	4	5	6	7	8	2
1	1	3	2	4	5	6	7	8	2
1	1	2	3	4	5	6	7	8	3
1	1	2	3	4	5	6	7	8	2
1	1	2	3	4	5	6	7	8	2
1	1	2	3	4	5	6	7	8	3
1	1	2	3	4	5	6	7	8	2
1	1	2	3	4	5	6	7	8	3
1	1	2	3	4	5	6	7	8	5
1	1	2	3	4	5	6	7	8	6
1	1	2	3	4	5	6	7	8	4
1	1	2	2	4	6	5	7	8	2
1	1	2	3	4	5	6	7	8	3
1	1	2	3	4	5	6	7	8	5
1	1	2	3	4	5	6	7	8	2½
1	1	2	3	4	5	6	7	8	2
3	2	2	1	4	5	6	7	8	3
1	2	2	3	4	5	6	7	8	1½
1	1	2	3	4	5	6	7	8	2

CLASS I. GIRLS.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight first and last of series.
6	1	2	3	4	5	6	7	8	3
7	1	2	3	4	5	6	7	8	2
7	1	2	3	4	7	5	6	8	2
7	1	2	3	4	5	8	6	7	4
7	1	3	2	4	5	6	7	8	5
7	1	2	3	4	5	6	7	8	2
7	1	2	3	4	5	6	7	8	6
8	1	2	3	5	4	6	7	8	3
8	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	3
8	3	1	2	4	6	5	7	8	3
9	1	2	3	4	5	6	7	8	3
9	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	3
9	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	2
10	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	3
10	2	1	3	4	5	6	7	8	3
10	1	3	2	5	6	4	7	8	2
10	1	3	5	2	4	7	6	8	2
11	3	2	1	4	5	6	7	8	2
11	1	5	2	3	4	6	7	8	3
11	1	2	3	4	5	6	7	8	2
11	1	2	3	4	5	6	7	8	9
11	1	2	3	4	5	6	7	8	4
11	1	2	3	4	5	6	7	8	1
12	1	2	3	5	4	7	6	8	2
12	1	3	2	4	5	6	7	8	2
12	1	2	3	4	5	6	7	8	2

CLASS II. BOYS.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	1	2	3	4	5	6	7	8	3
7	1	2	4	3	8	7	6	5	2
7	1	2	3	4	5	7	6	8	2
7	1	2	3	4	5	6	8	7	5
7	1	2	3	4	5	6	7	8	8
7	2	1	3	4	5	6	7	8	4
8	1	2	3	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	2
9	1	3	2	4	5	6	7	8	5
9	1	2	3	4	5	6	7	8	6
9	1	2	3	4	5	6	7	8	6
9	1	3	4	2	5	6	7	8	3
9	1	2	3	4	5	6	7	8	2
9	1	2	3	5	4	6	7	8	3
9	1	2	3	4	5	6	7	8	4
9	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	2
10	1	3	2	4	5	6	7	8	2
10	1	2	3	4	5	6	7	8	3

I IV V

II. V

CLASS II. Boys.—Continued.

III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
3	5	7	6	8	4	1½
2	4	5	6	8	7	2
7	3	4	5	6	8	4
6	4	3	5	8	7	3
3	4	5	6	7	8	2½
5	3	4	6	7	8	3
3	4	5	6	7	8	3
3	4	5	6	7	8	1½
3	4	5	6	7	8	3
3	4	5	6	7	8	3
3	4	5	6	7	8	3
3	4	5	6	7	8	2
3	4	5	6	7	8	2
3	2	4	5	8	7	3
3	5	4	6	7	8	3
2	4	5	6	7	8	2
3	6	4	5	7	8	1½
4	3	5	6	7	8	3
3	4	5	6	7	8	8
3	4	5	6	7	8	4
3	4	5	7	6	8	2
3	4	6	5	8	7	3

CLASS II. GIRLS.

III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
	4	5	6	7	8	0
2	4	5	6	7	8	2
1	4	5	6	7	8	3
3	4	5	6	7	8	2
3	4	5	6	7	8	4
3	4	5	6	7	8	3
3	4	4	6	7	8	2
2	5	4	6	7	8	3
3	4	5	6	7	8	8
3	4	5	6	7	8	3
2	4	6	5	7	8	3
3	4	5	6	8	7	2
3	4	6	5	7	8	2
3	4	8	6	5	7	2
3	4	5	6	7	8	3
2	4	4	6	7	8	3
3	4	5	6	7	8	4
3	5	5	6	7	8	5
3	4	5	6	8	7	2
3	4	6	4	7	8	2
3	5	5	6	7	8	6
3	4	6	5	8	7	2
3	3	4	7	6	8	3
4	2	5	6	7	8	4
5	4	5	7	6	8	3
3	4	5	6	7	8	2
3	4	5	6	7	8	3
3	4	5	6	7	8	3
3	4	5	6	7	8	6
3	4	5	6	7	8	8
3	4	5	6	7	8	3
4	3	6	5	7	8	3

Explanation of the Tables. The Roman numerals at the head of the columns represent the arrangement of the order of eight weights. All the weights which were judged to be the heaviest were recorded in the columns marked I; those weights which were judged to be the next lightest, in the columns marked II, and so on to those judged to be the lightest, which was thus recorded in column VIII. The Arabic numerals entered beneath in these columns represent the weights which were judged to belong in this order of weight. Thus, the figures 1, 2, 3, 4, 5, 6, 7, 8, represent the weights in order of their size: 1 represents the weight $1\frac{1}{2}$ inches in length; 2 represents the one 2 inches; 3, the one $2\frac{1}{2}$ inches, and so on up to 8, which represents the longest one, 5 inches in length. So, when the figures correspond with the Roman numerals at the head of the columns, it means that their weight was judged to be in the inverse order of their size. The smallest was judged to be the heaviest, and the largest the lightest.

In the columns marked "comparative weight of the first and last of the series," the figures are entered which represent the number of times the weight entered in column I was judged to be heavier than that entered in column VIII. The figures representing any disarrangements of the weights in order of their size are printed in bold-faced type, in order that they may appear more distinct in the tables.

The Arrangement of the Weights. A single glance at the tables is sufficient to see that more than half of the children arranged the weights in the *exact* order of their size. By actual count, it will be found that ninety-two out of 173 arranged them so, and that of all the others, not *one* reversed them, or even made an approach to a reversal, notwithstanding it was supposed that with the younger children this would be the case. Then, the degree of the disarrangement is of much importance. What is meant by degree of disarrangement in this connection can be best explained by taking a concrete example. In the table marked Boys, Class I, the third boy has made the arrangement, 5, 4, 2, 1, 3, 6, 8, 7; here, in this case, weight 5 was judged to be the heaviest, and so on in the order they occur. Now, it is plain that his arrangement is much more a departure from the normal than it would have been had he placed them so that the weights would in no case be arranged more than one place removed from the column in which it would be normally placed. Instead of weight 5 being put in either column IV or VI, which would be a displacement of the *first* degree, it is put in column I, which makes it a displacement of the *fourth*

degree from the normal. The degree of displacement, then, can always be found by finding the difference between the number of the weight and the number at the head of the column in which it is placed. It is easy to see that, of all the displacements made, by far the greatest number are only of the first degree. In fact, out of 287, the whole number of displacements, 218 were of the first order, fifty-one were of the second order, thirteen of the third order, fourteen of the fourth, and one of the fifth. This fact shows conclusively that, without an exception, each child arranged them under the influence of the illusion that their weight, in a general way, was inversely as their size. This point will be touched again in the study of the results, with special reference to the age of the children.

Comparison of the Weight of the First and Last of the Series. In order to test the amount of the illusion produced, after the weights had all been arranged as the subject thought in the order of their weight, he was asked to judge in relative terms between the one he had placed as the heaviest and the one he had judged to be the lightest. By referring to the table, it can be seen that the illusion is quite large. The judgments of the comparative weights of the two vary from one and one-third to ten. That is to say, the weights in column I were judged to be as many times heavier than the ones in column VIII as the figures indicate in the column marked "comparative weight." The average estimate of 172 children is that the weights placed in column I, which was the smallest weight in all but twenty-seven cases, was three times heavier than those placed in column VIII, which was the largest in all but twenty cases. Before these tests were made on the children, some preliminary tests had been made on adults, and it was seen that these comparative judgments were likely to be larger when all of the weights were presented for arrangement, than if only two, the shortest and the longest, were given. That is, the same weights were judged to be nearer equal when they were presented without the intervening ones. To make a thorough test of this interesting difference, 178 children of the same ages, and as near as possible of the same intelligence as those to whom all were presented, were given only the two extremes, asked to arrange them in order of their weight, and, after this was done, asked to state, in relative terms, their notion of their weights. These last children not only did not see the intervening weights, but did not know that there were any. The tables for these are not given simply because in all but two cases the shortest is put the heavier, and the average of the

comparisons is 2.4. That is to say, the smallest one in direct comparison with the largest one, uninfluenced by the suggestion of the intervening ones, was judged to be only 2.4 times the heavier. It is quite clear, then, that we have in the case of the judgments, when all the weights were presented, a summation effect of the illusion which, therefore, did not depend directly on the difference in the size of the two weights compared, but on the suggestion caused by the previous judgments in arranging the whole series. That is to say, the subject, after having decided that weight one was heavier than two, and two heavier than three, and three heavier than four, and so on through to eight, would come to compare the weights one and eight with the notion built up that there was of necessity much difference between them. This illusion, thus built up unconsciously, is measured approximately by the difference between these two averages, 3 and 2.4, which is .6. This means that an illusion of seventy-nine grams was built up through this series, if the largest one be taken as the standard. For, since the weights all weighed 132 grams each, the difference between these averages would give an addition of seventy-nine grams to the smallest. If the smallest one can be taken as the standard, then the largest one would be estimated seventy-nine grams too light. It seems more natural to interpret this illusion in the latter way, because the subjects all arranged the weights, beginning with the heaviest, and thus necessarily making it the standard, frequently coming back to it, indeed, as a standard as they progressed in the arrangement of the series. It is a legitimate question for the reader to ask for an assurance of confidence in this difference between the averages, or better, to ask for the probable errors of these mean results. These probable errors have been calculated according to the rules given by Jevons.¹ The error thus computed for the comparisons, when all the weights were presented, is .06; that for the comparisons when only two weights were presented is .05. The meaning of this is that if any number of judgments be made under the same conditions, one-half of them will fall between 3.06 and 2.94 in the case of the judgments when all of the weights were presented, and between 2.45 and 2.35 in the case of the judgments when only the extremes were presented. We have thus arrived at an approximate measure of the degree of credibility of the average, which indicates the region for the truth to most likely hit upon. The truth in this case being that if any number of judgments be made under the same conditions,

¹ See *The Principles of Science*, W. S. Jevons. p. 387.

one-half of them would fall within the limits 2.94 and 3.06 when all the weights were presented, and within the limits 2.35 and 2.45 when only the extremes were presented. Since, therefore, there is a great gap between the approaching extremes of these two sets of judgments, we are warranted in asserting with a high degree of certainty that the significant difference between the two averages is not an accidental one, but represents a difference in the judgments based on some permanent difference in the psychological basis for the judgments. This difference, as we have said, is due to the suggestion which came from the intervening judgments when all the weights were presented.

It was thought at first that there might be some relation between the size of this illusion and the least perceptible difference between weights of this heft; for if the illusion which is measured by seventy-nine grams be divided by seven, the number of comparisons made in arranging the weights, the quotient would not be very far removed from the least discernible difference between weights of this heft, were the hefts of those compared of equal ratios. But the distribution of the errors shows that there is at least no discernible connection.

The question now arises, how can this illusion be distributed throughout the series? This can be answered partially by noting the position in the series where the disarrangements have been made. It is evident that wherever the apparent difference in weight between the weights was greatest, there would naturally occur less disarrangement from their order as to size, and *vice versa*. Then, it must be borne in mind, that, as the weights increase in size, the relative difference between adjacent weights, when placed in order of their size, decreases. Thus, the relative difference between the sizes of one and two is one-third, while between seven and eight it is only one-ninth. From this relation alone we would have a right to expect fewer disarrangements near the beginning of the series than we would near the end. But since there are more disarrangements near the beginning, as can be seen from a study of the tables, the relatively greater difference in size of the first ones was more than counter-balanced by the suggestion which came when it was seen that the arrangement was probably in the order of their size. It may be well to call attention here to what will be discussed more at length further on, and that is the fact that records on forty-eight adults show the reverse to be true. That is, as we might expect from the difference in the relative sizes as the series progressed, more disarrangements were made as this relative difference decreased.

Intelligence and Apperception. In the description of the method, it was stated that before any experiments were made on the children, that their teachers divided them, on paper, into three classes, with reference to their general intelligence: class I including the brightest ones; class II, those considered good, and class III, the dullest. This classification, of course, was wholly unknown to the children, as has been said, and so in no way would affect their tests. The purpose of this was two-fold: (1) To compare the strength of the associations of the three classes, and note the influence of these on the judgments, and (2) to determine if there be any difference in the suggestibility of the three classes.

1. The strength of the associations that have been made by the subjects between size and weight is found by a study of the arrangement of the weights. In the first place, no subject hesitated in deciding that there were differences in weight, and that of quite a marked degree. Indeed, the illusion is so strong that it is impossible to rid one's mind of it, even after one sees their weight is absolutely equal, providing that he perceives either through sight or touch that there is a difference in their size.

This last fact is a very striking illustration of the strength of a long continued and practically an unbroken series of associations. Throughout our whole experience, difference in weight is associated with difference in size, and especially so when the weights to be judged are apparently of the same material, as in this case. When the weights were placed before the subject, perhaps the first observation made was that they differed in size and that some were much larger than others. The next idea which arose, especially since the test of weight sensibility was involved, was to the effect that the largest was the heaviest. These ideas perhaps all arose in the mind of the subject before he had touched a weight, so that when he began to lift them, he would unconsciously put forth effort in lifting them in proportion to their size, as he had always been accustomed to do. But as the weights were of equal heft, they of course would respond the more readily to the lift, as they were the larger, and thus arose a basis for the illusion. One would naturally suppose that after the weights had been handled awhile, that the illusion would fade away, but, on the contrary, those subjects who took much time in the arrangement of them very rarely failed to arrange them in the order of their size, judging them to be lighter as they grew larger. Indeed, after having made them and worked with them for weeks, fully conscious of the illusion all the time, I could not rid myself of the idea that there was a striking difference in their weight, when I

depended alone on what seemed to be my sensations. Some of the adult subjects, upon whom I myself experimented, after being told, refused to believe that they were of the same weight, until they saw them weighed. And even then could scarcely prevent themselves from discrediting the balances.

I have nowhere found a better illustration of the power of apperception, or how our sensations are modified and transformed by the ideas already in the mind, than is brought out in these tests. The stimulus in the case of each weight was exactly the same, but the mental result for each weight was different. That is to say, were it not for the imposition of the associations previously formed, the weights would have all appeared equal in their heft, because the same amount of muscular effort would have sufficed in each case to lift the weights.

In Steinthal's¹ story of the party in the railway carriage, all of whom were strangers to each other, it is related that one member succeeded in telling the occupation of each of the others by their spontaneous answers to the same question. Here, as in the weights, the same stimulus was applied to each person, but unlike the results in the case of the weights, the responses were very different. This brings out very clearly the fact that from the same stimulus different results arise for different people, only in so far as the associations with the same special stimulus have differed. In the case of the weights, it has been, as has been said, a universal, almost an inborn association, that of two or more weights of the same material, the heft increases with the increase in size, and so not only was the stimulus the same for each person, but the sensations received were moulded and shaped under the influence of like associations, in such a way that we get the same general results. So, while the doctrine of apperception calls especial attention to the transforming influences, brought about by the elements which the mind itself furnishes, the association processes give the key to the influences thus brought to play upon the sensations.

2. If the foregoing analysis be true, then we would expect that the more intelligent the child, the more likely he would be to arrange the weights inversely as their order in size. For it would follow that those who have been the most discriminative, not only for differences in weight, but also for size and likeness of the material, of which the bodies to be compared were composed, have the strongest and, therefore, the most dominating connection between the two. Coming to a direct study of the tables for each of the three

¹ See James' quotation in his *Psychology*, Vol. II. p. 108.

classes, the truth of these statements is fully borne out. The following table will assist in making this clear:

Number.	DEGREES OF DISARRANGEMENT FROM THE ORDER OF SIZE					No. of Disar- rangements.	Av. No. of Dis- arrangements.
	1st.	2nd.	3rd.	4th.	5th.		
I. { Girls.	30	31	9	1		41	1.3
I. { Boys.	22	11	4	1	1	17	.7
II. { Girls.	30	43	8	1		52	1.7
II. { Boys.	41	56	10	4	2	72	1.7
III. { Girls.	23	31	14	3	1	50	2.1
III. { Boys.	28	45	6	3	1	55	1.9
Totals.	174	217	51	13	5	1	287

Explanation of the Table. Here are gathered together all of the displacements from the order of size and exhibited for the three classes of intelligence. Each class is divided with reference to sex. In class I there were thirty girls and twenty-two boys; in class II, thirty girls and forty-one boys; in class III, twenty-three girls and twenty-eight boys. In the whole set of tests there were only five grades of displacement. It will be seen that nearly all of the displacements are of the first degree. That is, they were only one place removed from the place they would occupy in the series were they all arranged in order of their size. As the table indicates, there were 217 displacements of the first degree, fifty-one of the second, thirteen of the third, five of the fourth and one of the fifth. In the next column will be found the number of displacements, and in the next the average number of displacements for each child for each class. It will not only be noticed that of all the displacements for each class, there are relatively more displacements of the first degree in class I than in class II, and more in class II than in class III, but especially it will be seen that the average number of displacements per child increases as the grade of intelligence decreases, so that it happens that the absolute number of displacements for fifty-two of the brightest children is fifty-eight, while fifty-one of the dullest make 105 displacements, making the average in the latter case double that of the former.

In accordance with this result, one would have a right to expect a still smaller number of displacements when the tests were made on adults. And this is exactly what happens. The number of displacements for forty-eight adults is only forty-four, making the average but .9 of one displacement. (See the table of records on adults page 358.)

Difference with Regard to Sex. Here, again, we might expect to find that differences in the strength of the associations would assert themselves. Since boys have to do in

their experience with size and weight relations much more than do girls, we would expect their associations to be stronger between these two contiguous ideas. This is what we find to be true, though in no very striking degree. However, by observing the preceding table, it will be seen that the average number of displacements is less for the boys than for the girls in classes I and III, and equal to them in class II. By finding the average number of displacements for all of the boys and all of the girls when the whole series of weights was given, it is found that this average for the boys is 1.5, while it is 1.7 for the girls. Though this special difference is not at all marked, it is worth noticing. The chief difference between the judgments of the boys and girls, however, is shown in the difference between the comparisons of the first and last of the series when all the weights were presented and when only two were presented. The average for the girls when all were presented, is slightly larger than the same for the boys, as is also their probable error, showing that while the girls have a distinct notion that there is a difference in weight, they have not a distinct notion of what this supposable difference is. But when it comes to a comparison of the averages of their estimates when only two were presented, there is a much wider difference. When only the two weights were presented, the boys judged the small one to be twice as heavy as the large one, while the girls made an average estimate that it was 2.4 times as heavy. This brings out plainly—since the probable errors of these means are quite small—that the boys were more influenced by the suggestion than the girls when all the weights were presented, and so in line with the fact brought out previously in the paper, that those with the stronger associations of weight and size relations were the most suggestible, as shown by the size of their estimates of their comparative weights.

Difference with Regard to Age. The 173 children upon whom tests were made for the whole series of weights, were about equally distributed for the various ages from seven to fourteen inclusive. It was expected that there would appear quite a difference between the disarrangements of the weights for the two extremes of age, as well as in the comparative judgments of the two weights. But this is not what happened in either case; the difference in each case is too slight to point to any real difference between the ideas of the two for weight and size relationship. However, since only about twenty of each age were tested, no definite statement can be made with reference to what would appear if more were tested. This lack of difference between the records of

children seven and fourteen years of age indicates that the relationship of the ideas in question is about as definite, other things being equal, at seven as at fourteen. But this result, with respect to age, will only be of value as an indication of what can now be better determined. It seems unnecessary to give the special table here, upon which these statements are based, for they can be verified from the general tables previously given.

Just here it might be mentioned that in the whole number of children tested, no less than thirteen nationalities are represented. The fathers of forty-eight per cent. were born in the United States, twelve per cent. in Germany, eight per cent. in Sweden, eight per cent. in Denmark, five per cent. in England, four per cent. in Canada, three per cent. in Bohemia, two per cent. in Norway, and the remaining ten per cent. distributed about equally in Austria, Ireland, France, Russia, India and The Netherlands.

Tests on Adults. The tests recorded in the following table were taken before the work was done with the school children, and from this work the method was partially developed and completed. It may be of interest and value to say that the records here recorded were made on subjects of various occupations, including a number of specialists in psychology, pedagogy and mathematics, as well as druggists, mechanics, grocers, and those generally whose occupation would lead them to be discriminative with reference to weight and size. By glancing at the accompanying table it can readily be seen that there are fewer disarrangements for adults than for children. In the whole number of adults, which is forty-eight, there are but forty-four displacements; thus making an average of .9 of one displacement for each subject. But especially ought it to be noticed that all of these displacements are made by thirteen subjects, and that thirty-five of them, or more than seventy-two per cent., made no displacement at all from the order of their size. It should be borne in mind that throughout this whole research with the children as well as with the adults, *one* judgment, not in accordance with the order of the size, means in nearly all cases *two* displacements. Suppose weight four was judged to be heavier than three and so arranged; there would of necessity be two displacements, according to the method used. If it were possible to make sure of the number of judgments used in all the displacements, it would be better to compare on the basis of these. But this is not always possible and so the other method has been used. If there be any preference for either class in the method used, it has been to make the difference

less marked between them, by an exaggeration of the judgments of the most intelligent, not in accordance with the order of size. So, therefore, if such a method could be used it would only accentuate more decidedly the correctness of the results obtained by our study of the records.

No.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first, last of series
1	1	2	3	5	6	4	3	7	
2	1	2	3	4	5	6	7	8	
3	1	2	3	4	5	7	3	6	
4	1	2	3	4	3	7	6	5	
5	1	2	3	4	5	6	7	8	
6	1	2	3	4	5	6	7	8	
7	1	2	5	4	3	3	7	6	
8	1	2	3	4	5	6	7	8	
9	1	2	3	4	5	6	7	8	
10	2	1	3	4	6	5	3	7	
11	1	2	3	4	5	6	7	8	
12	1	2	3	4	5	6	7	8	
13	1	2	3	4	5	6	7	8	
14	1	3	2	4	5	6	3	7	
15	1	2	4	3	5	6	7	8	
16	1	2	3	4	5	6	7	8	
17	1	2	3	4	5	6	7	8	
18	1	2	3	4	5	6	7	8	2½
19	1	2	3	4	5	6	7	8	2
20	1	2	4	3	5	7	6	8	2
21	1	2	3	4	5	6	7	8	1½
22	1	2	3	4	5	6	7	8	3
23	1	2	3	4	5	6	7	8	4
24	1	2	3	4	5	6	7	8	4
25	1	2	3	4	5	6	3	7	
26	1	2	3	4	5	6	7	8	3
27	1	2	3	4	5	6	7	8	3
28	1	2	3	4	5	6	7	8	3
29	1	2	3	4	5	6	3	7	1½
30	1	2	3	4	5	6	7	8	2
31	1	2	3	4	5	6	7	8	1½
32	1	2	3	4	5	6	7	8	4
33	1	2	3	4	5	6	7	8	3
34	1	2	3	4	5	6	7	8	3
35	1	2	3	4	5	6	7	8	3
36	1	2	3	4	5	6	7	8	3
37	1	3	2	4	5	6	7	8	3
38	1	2	3	4	5	6	7	8	1½
39	1	2	4	6	3	5	7	8	3
40	1	2	3	4	5	6	7	8	8
41	1	2	3	4	5	6	7	8	4
42	1	2	3	6	5	4	7	8	3
43	1	2	3	4	5	6	7	8	8
44	1	4	2	3	6	5	7	8	2
45	1	2	3	4	5	7	6	8	2
46	1	2	3	4	5	6	7	8	3
47	1	2	3	4	5	6	7	8	3
48	1	2	3	4	5	6	7	8	2

Results on Adults. It will not be necessary to explain at length what these tests indicate, for they are in line with the results obtained on the school children previously described. The method was the same in the two cases, except for the first seventeen adults who were not asked at first to make a statement in comparative terms of the weight of the first and last of their series. At this point in the work, the idea occurred, for it had become evident that the illusion was quite marked and that by this means I could get a measure of it. With the rest of the subjects, all of whom were males but two, the comparison is given. Let us first look at the arrangement of the weights. A glance is sufficient to show that there are much fewer displacements in this table than in those with the children. True it is that the twenty-two boys in class I have made a smaller average of displacements, but the probability is that if a greater number of these had been tested, their average would have been larger. Another difference between the displacements for the children and the adults has been mentioned previously; this is the difference of position in the series where the displacements occur. The following table will make this clear. Speaking generally, it is seen that the children make more displacements at the beginning of the series than toward the last, while just the opposite is true for the adults. This latter, as has been pointed out, is

TABLE SHOWING THE DISTRIBUTION OF DISPLACEMENTS.

	No.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	
I	{ Girls,	30	3	7	7	5	7	6	4	1
	{ Boys,	22	2	4	5	1	2	1	1	1
II	{ Girls,	30	5	7	8	6	9	7	6	4
	{ Boys,	41	3	10	10	12	10	8	11	8
III	{ Girls,	23	5	9	9	5	6	6	7	2
	{ Boys,	28	9	11	9	9	7	4	4	2
	Adults,	48	1	4	8	7	7	10	8	8

what one would expect from the gradually diminishing ratio of the sizes of adjacent weights. But this difference is not very striking and more records are necessary before any conclusions can be drawn from them. By far the most interesting difference that came out of the records of the children and those for adults is the greater difference which adults make

in the comparisons of the two weights when all were presented and when only the first and last were presented. The averages of the estimates when all the weights were presented is two and six-tenths, while that when only two were given is one and five-tenths. The probable errors of these means are very small, and thus render the result in a high degree credible.¹

In brief, the results on adults show all the effects of more firmly fixed associations and hence a greater degree of suggestibility in the direction of these associations, as well as the greater influence of these in dominating and transforming the sensations directly received. That is to say, we are unable in this case to ascribe any definite value to the actual muscular sensations, simply because the preconceived notions in the mind, determined and fixed by all of our previous experience, render us unable to determine the truth in such conditions. This is a good illustration of what might be called unconscious dogmatism.

2. *On the Influence of Shape on the Judgment of a Series of Weights of Equal Weight and Size.* A series of tests was made on seventeen adults with weights of the same weight and size, but of different shape, to determine if this suggestion of difference in weight would prevail according to apparent difference in size. Eight weights were made of sheet lead, all of the same area and weight, but varied in shape from a circle to an irregular cornered figure. The table below shows that the weights which appear smaller, that is, are of a compact form, were judged to be heavier than those not so compact. While the preference, of course, is not nearly so uniform as it was for the brass weights, the averages of the numbers representing the order in which they were placed, show a decided preference. It will be seen, by reference to the table, that those weights which are judged to be the lightest are those whose form makes them appear the largest of the series. It should be noted here that when the weights were taken into the hands rather than grasped by the fingers, there was furnished a basis for a difference in the sensations of weight due to the fact that those more compact in form, for example the circle, exerted the same pressure on more closely related

¹Since my work was completed, my attention has been called to some experiments made by Charpentier, who used two brass balls of different size, but of the same weight. He found the same illusion, the larger seeming the lighter. His balls weighed 266 grams each, but the larger was judged to be sixty-six grams lighter than the smaller. *Elements de la Sensation de Poids. Archives de Physiologie. Série 5, Tome 3, p. 126.*

and fewer pressure spots, than those whose form was more irregular. The subjects were carefully observed on this point and very few of them permitted the introduction of this disturbing physiological element. Even when the weights were taken into the hands, the decision was almost invariably rendered after a balancing, of the two compared, by grasping them between the fingers. The avoidance of this physiological cause for illusion went so far with one subject (L) that he tied strings to the weights and then lifted them by the strings. Nevertheless, his record corresponds very closely to the general result.

No experiments were purposely made, either with the brass weights or the lead weights when the subjects were blindfolded, because it was thought that such a method would of necessity introduce much more strongly this probably disturbing physiological element, which was otherwise almost wholly eliminated, even when the subjects were not directed how to lift the weights, but left to use their own method, as was the case with the adults. It was, of course, wholly useless and unnecessary with either series of weights to take a series of experiments, when the elements of size and form were both eliminated, and the weight for each remained the same.

							
1	5	3	2	7	8	6	4
4	1	2	5	6	3	8	7
1	3	2	4	5	8	7	6
2	4	1	3	5	6	8	7
3	4	2	1	6	5	8	7
7	5	3	1	2	4	6	8
3	6	1	4	2	7	5	8
2	1	4	5	3	7	8	6
2	1	5	3	4	6	8	7
4	2	5	3	1	2	8	6
1	2	3	4	6	5	7	8
1	4	3	2	6	7	8	5
1	2	3	5	4	6	7	8
5	6	1	4	3	2	7	8
3	2	1	6	4	8	5	7
1	2	3	5	4	7	8	6
2	1	5	3	7	8	6	4
43	47	47	60	75	99	100	112
2.52	2.76	2.76	3.52	4.41	5.82	5.88	6.58

Explanation of Table. The figures drawn at the head of the columns roughly show the shape of the weights used. The comparative sizes must not be inferred from the drawings, as they are not exactly made. The figures in the

columns represent the order of their weight as the subjects thought. For example, the first row of figures is the order which A judged them as to weight; the second is B's order, etc. It will be noticed that the weights are arranged in the table in the order they were judged on the average. But it must not be inferred from this arrangement that they were presented to the subjects in this or any other order. They were placed before the subject in a hap-hazard way and the subject left to separate and arrange them according to his own method.

Conclusions.

1. The more intelligent the children, other things being equal, the stronger are the associations between the ideas of size and weight of a given material.

2. The stronger this associative element becomes, the more likely it is to dominate and pervert the true sensations, when the conditions are such that these associations do not hold.

3. The elements the mind furnishes, keeping these conditions, have far more influence in determining the judgment than the sensations directly received.

4. Illusions are easily built up when suggested along the lines of firmly fixed associations.

5. Consequently the brightest children are more suggestible under these conditions than the dullest ones.

6. The method used in this research furnishes a means of measuring suggested illusions of this type.

7. Adults have stronger associations between the ideas in question than children, and, despite the fact that they have a higher degree of sensibility for difference in weight, their sensations are more transformed and influenced by the element which the mind itself furnishes than are those of children.

8. Facts which vary, within limits, from our established habits of apperception are simply not taken account of at all; or, if on some occasion the conditions force us to see how our minds have become insulated against the reception of different relations, we do so with a wholly new feeling of personal fallibility.

9. The pedagogical significance of the facts emphasized in this research is of the utmost importance. It has to do with one of the most fundamental laws which regulate our mental life. The foregoing tests as a whole show how strong and dominating an association between ideas may become when they are practically unseparated and immediate in their sequence. "That which the law of gravitation is to astronomy," says John Stuart Mill, "that which the elemen-

tary properties of the tissues are to physiology, the law of the association of ideas is to psychology."

V. *Minor Observations.*

Reversal of Certain Cutaneous Sensations of Motion. The well-known "waterfall" illusion for vision, or as it is more scientifically called, the antirheoscopic illusion, suggested that perhaps the same illusion might occur in case of the skin, if practically the same conditions were complied with; that is, if the skin were stimulated rapidly and regularly in a continuous direction comparable to the stimulus given to the retina by the steadily falling water, or the moving stripes on the antirheoscope. To do this a frame was made sufficiently large to allow the arm to be placed in it. At each end of the frame a roller was placed and over these a belt of plaited velvet was fastened. One of the rollers was furnished with a crank, so that by turning this the skin could be stimulated with the moving folds of the velvet regularly and continuously in whatever direction desired. The folds in the velvet belt were about three-fourths of an inch in depth and one and one-fourth inches apart. Experience proved that these folds need to be made carefully and directly across the belt with no irregularities in them at all, otherwise the stimulation becomes too complex, and the resulting after sensation mixed and indistinct. Care needs to be taken, too, that as far as possible, the velvet must be kept of the same temperature as the arm, for if not, it will absorb the heat of the arm so readily as to swamp all other sensations in that of temperature. The folds of the velvet need to be pressed sufficiently hard against the skin so as to make the sensations of each fold clear cut and distinct. Having as nearly as possible satisfied these conditions, I tried the apparatus first on two women, both of whom were wholly ignorant of the purpose of the experiment, and knew nothing of the illusion for vision. I simply asked each one to give attention to the sensations caused by the moving velvet, and after it was stopped to notice carefully any sensations that might then come out different to those perceived while the belt was in motion. The result was that each one immediately said, "I feel the motion as if the belt were moving backward." Of course these trials were made so that the results of the experiment on one were unknown to the other. Afterward, I tried it upon myself and Dr. Sanford, and though each of us was somewhat skeptical as to the nature of the results, something like backward motion was experienced. It was tried for the lower arm lengthwise and crosswise, and in both directions for the

palm of the hand and for the forehead. For myself, the clearest results were obtained on the sole of the bared foot.

This sensation for reversed motion is not so clear and distinct for touch as it is in the case of vision, as would naturally be supposed, for our attention is so much less frequently directed toward sensations for touch than for sight, that we are unable to concentrate it so carefully and easily.

Although this experiment shows that the illusion in question can be produced in touch as well as in vision, no completely satisfactory explanation can be given. The following suggestions, however, may have something of truth in them and I give them merely for what they are worth.

Stimulation of any part of the body immediately causes a greater flow of blood to that part and especially do the smaller blood vessels become congested when the skin is stimulated with friction, as in the case of the foregoing experiment. Then as the folds of the velvet are swept over the skin there is a tendency for the blood to be forced along in the capillaries in the direction of the moving belt and when the actual motion stopped, to flow back again to restore the equilibrium in the vascular pressure. The rush of the blood to the face in blushing, which is very noticeable and the direction of the wave quite marked, will illustrate in a magnified degree what takes place in the skin under the conditions of the experiment. This view is in harmony with the fact that the sensation of reversal for touch as for sight is immediate, quickly dies out and does not return again as other after-images.

I am aware that the view here taken seems much more reasonable for touch than for sight, and yet the changes in circulation caused by the hastened metabolic processes in the eye brought about by such stimulation may suffice to re-stimulate the retinal elements of vision in a reverse order to that of the direct stimulus and thus furnish a basis for the illusion.

After-Images of Touch and Summation of Stimuli. Dr. Alfred Goldscheider¹ has called attention to the fact that if one touches the skin with a moderately sharp pointed instrument, the first sensation arising therefrom is a pricking sensation which soon dies away, but after this sensation has disappeared, a second sensation arises which retains something of the pricking sensation, but has lost the quality of touch which was prominent in the first. He found by further

¹ A. Goldscheider. *Ueber die Summation von Hautreizen*. Du Bois-Reymond's Archiv, 1891, p. 161.

experiment that if the primary sensation approached very near to a painful sensation, the secondary sensation became distinctly painful, while if the primary itself became painful, the secondary sensation was less distinct. It was found, however, that if the skin be stimulated with a single electric stimulus, the secondary sensation did not come out. It was found that, in order to arouse this secondary sensation, it required a number of electrical stimuli made on the same spot. A series of four stimulations gave a clear secondary sensation only on condition that certain intervals between the stimuli be retained. That is to say, the sensation was most marked with intervals between the stimulations of from .03 to .06 seconds. If the intervals were varied above or below these limits, the secondary sensation became less and less distinct until it gradually disappeared.

For the purpose of testing these results, I first made some small pencils of cork, gave one each to a number of subjects all in the same room, and all students of psychology, and asked them to touch their hands with these points and note carefully their sensations, allowing several seconds to elapse between the separate touches. I did not tell them what to expect, but simply to describe carefully their sensations; Goldscheider's work was known, however, to a part of them at least. The following are a few of the results obtained, and are practically the same as those on a number of other subjects: —

1(A). A more or less ticklish sensation, much like that of a mosquito bite that has nearly disappeared, at first came out. Some time after, when I supposed that all sensation had disappeared, a sharp, somewhat painful, sensation flashed out and disappeared nearly as suddenly. But after quite a period of time, in fact after writing the above description, it appeared again, but with less force each time, until it completely died away.

2(B). At first, a sense of contact and then a sense of pressure. These were both accompanied by a sensation of temperature. On touching my hand again somewhat more vigorously, I find that the demarkation between the sensations of touch and pain, is much more definite than I thought it to be. I noticed no after-sensations at all. [In later experiments, however, this subject had distinct after-sensations.]

3(C). After touching my hand with the cork, in addition to the feeling of contact, in about one-half of the cases, there came a distinct secondary sensation of a more or less painful character, and diffused over a greater area than the primary sensation. In the cases where the primary sensation was

somewhat painful, the after-sensations did not come out so clearly.

4(D). After touching the back of my hand the first time, as an after-effect there arose a slight drawing sensation; the second trial, I struck my hand a little too hard so that the first sensation was slightly painful. Later a sharp, fine sensation, somewhat extended, but chiefly located about an inch nearer the wrist, and somewhat toward the little finger side, came as an after-effect. This was very distinct, both in quality and place, from the slight pain resulting immediately from the stroke. In the third trial the pressure was slight, and the sensation of a sticking character and seemingly not so fleeting as in the other cases; it faded slowly, but not regularly, and seemed to recur several times. In the fourth trial, the touch was again very light, and the after-image was clear and somewhat irradiated. I noticed that the after-images of former touches returned and rendered this last sensation somewhat vague.

From the above results, it is clear that after-images of touch are easily recognized, and that these are of a more or less painful character, even though the primary sensation itself contain no element of pain. My results, of which only samples are given, are in accord with those obtained by Goldscheider for touch. I did not repeat his experiments with electrical stimulation to obtain the summation effects, but I found that this after-image could be much sharpened and enforced by the effects of tickle. After experimenting some time on this after-image effect, it seemed that the clearness and vigor of its effect were due largely to the plethoric condition of the part stimulated. That is to say, when the hand, for example, was cold or had been resting passively, the after-image from a given touch was much less marked than when the hand was full of blood and the attention directed toward it. It occurred to me that the tickle sensations might serve to reinforce and bring out more distinctly this after-effect. Accordingly on many naïve subjects, I first made experiments to determine the normal strength of the after-image before tickle, and then in a short time stimulated the surface thoroughly by tickling. The result, in most cases, was a distinctly marked increase in the clearness and vigor of the after-image, especially in its painful quality. No difference was noticeable, however, in the quality or quantity of the primary sensation. It would seem, then, that increase in sensitiveness for direct touch could not be assigned as the whole cause. Furthermore, I came to see that the rush of blood to the spot pressed, after the cork point had been removed, coincided in time exactly with the recurrence of the after-sen-

sation, and in my own case, and even with other subjects, I could determine precisely when the after-sensation would flash out, by simply watching the rush of blood to the spot touched. Now, it is clear that this rush of blood is the chief, and so far as I can distinguish, the only direct stimulating agent for the production of this after-sensation. Just how this is done is an entirely different thing. It may be that the temporary lack of blood in the capillaries at the point touched, caused by the pressure of the cork point, was sufficient to permit an amount of metabolism of the part, great enough, so that when the blood returned and the opposite process was started, the toxic agents released acted directly on the delicate adjacent nerve tendrils, and the result was the after-sensation. This view seems the more probable when it is remembered that the after-sensation contains invariably a painful element and none of touch or contact. It may be, and probably is, true that also the minute tissues of the part are slightly ruptured, and the return of the blood to these would thus restimulate the adjacent nerves more sharply than the original stimulus, and hence the feeling of pain. Whether this view be correct or not, the fact still remains that the return of the blood to the point stimulated, is simultaneous with the after-sensation. Goldscheider's theory, that this summation stimulus is probably due to the hindrance and the reinforcing of the stimulus in the cellular tissues, which are spread out along the nervous tract and in connection with it, seems to me to go needlessly into difficulties.

Dermographism. "Dermographism, or skin writing, is said to be dependent on two causes: an irritable nervous system and some toxic agent. Both of these conditions become causes which may act either on the peripheral vaso-motor nerves, or on the vaso-motor centre, and so influence directly the circulation." The foregoing is about what the physicians say of this. But it is very easy to obtain upon perfectly healthy subjects. In order to test this, after having found it true for myself, I asked a class of ten men to write, with moderately blunt pieces of cork, on their lower arms the word *blood*, and then slightly rub the arm. This was done, and the word in each case came out in flaming red letters after a pause of a few seconds. In some cases, there was left a welt where the cork passed over the skin, after the increase of the blood had died away, although the writing with the cork was done very lightly and gave no sensations of scratching.

The studies here described were made during the present academic year at Clark University, under the immediate direction of President Hall, to whom I wish here publicly to express my grateful acknowledgments for the many suggestions received during the progress of the work. Likewise to Assistant Prof. Sanford, who has been ever ready to render any assistance, my thanks are due. To all those also who have so generously lent their services as subjects for experimentation, I acknowledge my indebtedness, and especially to Mrs. Dresslar, without whose coöperation some of the work would have been impossible.

ON THE DIFFERENCE SENSIBILITY FOR THE VALUATION
OF SPACE DISTANCES WITH THE HELP OF ARM
MOVEMENTS.¹

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The experiments which form the basis of this paper, were commenced in the institute for experimental psychology here under Professor Wundt, in the summer semester of 1892, and continued during the following two semesters. The object was to determine the fineness of the space-sense as shown by the difference sensibility for movements of the arm in vertical directions upwards and downwards, at different heights, standards and rates of movement. The influence of the leaving of the end points determined or undetermined is also considered. The experiments consider otherwise different asymmetrical influences of the body on the valuations and certain phenomena which result from the loading of the moved members with weights. Either the two arms together or only one were moved as far as possible without bending of the elbow and hand-joints. There are two chief parts to the experiments. The first comprises such as were made by drawing simple curves upon a vertically standing table or on the wall with a soft lead pencil. These are described as the pencil experiments. They are one-handed or two-handed, according as the movements belonging to a single experiment were made with only one arm or with the two arms. In both cases the direction of the arms was forwards, parallel to the median and at right angles to the side plane of the body. The second part comprises such one-handed experiments as were made by what is here described as the angle method. For this purpose a vertically standing upwards and downwards movable table was used. On one side of this, the side adjacent to the arm, a half circle was described, whose radius was 68 cm. and at every one-fourth degree of it a hole was bored through. By this means movements of different extents in angles could be marked off by the placing of plugs in the holes. The observer sat on a chair, and the centre of the circle could be made to correspond to the centre of the arm's motion by shifting of the table up and down.

¹ Translation in part of the original as written in German, spring and summer of 1893. See Wundt, *Grundz.*, 4th Ed. Vol. I. p. 429. *Revue Philosoph.* Dec. 1893, p. 663. To all of those who acted as observers, I am indebted for their patience, namely, Geh. Prof. Wundt, Dr. Külpe, Dr. Kirschmann, Dr. Cohn, Mr. Kiesow, Mr. Rostovsky, Mr. Child, Mr. Rogers and Mr. Hicks, as well as to the first mentioned for the conveniences of his laboratory.

I.

EXPERIMENTS BY MEANS OF THE PENCIL METHOD.

The method of relative valuation and the method of relative difference sensibility.

When one investigates the difference sensibility for motion sensations, he can either leave the extent of the normal distances in any series or group of experiments determined, or he can allow the observer to fix the extent himself. In the experiments of Fullerton and Cattell¹ both methods are used. In the latter case the psychophysical methods of the average error, the mean valuation and the right and wrong cases can be used. Where, however, the normal extents, as also the compared extents, are fixed, the method of just perceptible change, the method of mean valuation and the method of right and wrong cases can be used. The fixing of the normal and compared extents brings about groups of other sensations, which complicate the judging of the pure motion sensations. These are contact, pressure and resistance sensations. Hence it is desirable to have the movements made without contact, pressure or resistance through the fixing of the extents. If, also, only the normal extents are fixed, the pressure contact and resistance at the end can make these of no use as standards for the compared extents. If one investigates the pure motion sensations, then one must, if possible, make the movements without fixing the end points. This can only take place if one makes use of shadows, photography, mirrors, or some other such optical apparatus. My first group of experiments made by the pencil method give impure motion sensations in so far as contact sensations are felt with these. Their calculation, however, is similar to such as can be made without contact. In all such movements, where the making of the standard curves equal to one another in any single series is dependent on the observer, there exists a variation of the individual from the average normal length. The influence of this variation upon the average differences between normal and compared extents where one uses the method of average error, is considered in another place.² Between the smallest and largest normal extents in any single series, one can further reckon out whether the individual differences upon increase of their respective normal extents increase or decrease. If we call such of the individual differences between the normal and compared extents, where the latter are greater than the former, plus; where they are smaller, minus; and where they are equal to, equal,—then the individual differences in any series can either be all plus and equal, all minus and equal, or also there can be all plus, minus and equal differences. The difference sensibility where the normal extents increased, would increase or decrease respectively with increase and decrease in the individual differences in proportion to the increase in their normal extents. If they are all minus, or minus and equal, then the difference sensibility upon increase in the normal extents would respectively increase or decrease in proportion to the increase in their normal extents inversely with the increase or decrease of their respective differences. Where these are all either plus, minus and equal, one must consider the cases where they are plus and equal and where they are minus and equal, each by itself, and from this twofold result reckon out the mean difference sensibility. The method by which the relative

¹ "The Perception of Small Differences." Fullerton and Cattell. Philadelphia, 1892.

² Pages 375, 379, par. 12, 381, 391d.

difference sensibility with the variations in the normal curves, where all the individual differences are plus, minus or both plus and minus, is reckoned, I call the method of relative difference sensibility. The reckoning of the difference sensibility by this method is very similar to the reckoning used in the method of relative valuation. If we let the individual differences in a series be all plus, all minus, or both plus and minus, then the valuation (judging) will increase where the compared extents increase with increase in their normal extents. This will be shown by the absolute increase of the individual differences with increase in their respective normal extents. Inversely, the valuation will decrease where the compared extents decrease with the increase in their normal distances. This will be shown by the absolute decrease in the individual differences with increase in their respective normal distances. The method by which this valuation within the variations of the normal extents in a series is reckoned out, I call the method of relative valuation. The estimation of this valuation is similar to the estimation of the difference sensibility by the method of relative sensibility, where the individual differences are either all plus or all minus. As it is the simplest, I give at first the reckoning out of the method of relative valuation.

Description of the Reckoning Out of the Method of Relative Valuation.

We let m_1, m_2, m_3 , etc. . . . m_x be the simple digits 1, 2, 3, etc. . . . x , of which m_1 represents the greatest normal extent, m_2 the one in the same series which is just smaller than m_1 , and so on up to $m_x = x$, which represents the smallest normal extent. We let further n_x, n_{x-1}, n_{x-2} , etc. . . . n_3, n_2, n_1 be the same digits $x, x-1, x-2$. . . 3, 2, 1, of which, however, $n_x = x$ represents the smallest difference between any individual normal extent and its compared extent in the same series, $n_{x-1} = x-1$, that which is somewhat greater than n_x up to $n_1 = 1$, which represents the greatest difference between any normal extent and its compared extent in the series. We add, then, each m value to its respective n value, so that the m value, which represents any individual normal extent, is added to its respective n value, which represents the simple difference between this normal extent and its compared extent. r, o, s , etc. . . . g are the sums of such individual values as $m_1 + n_x$, etc. . . . $m_x + n_1$, according as the valuation takes place. In every series there exists x such values. d, e , etc. . . . f are the variations of these values from $(m_n + 1) = (n_1 + 1)$. q represents the average of these variations. If m_1, m_2 , etc. . . . m_x are added respectively to n_x, n_{x-1} , etc. . . . n_1 , then will d, e , etc. . . . f each be like null and the average variation q will also be like null. If they are added together inversely, namely m_1 to n_1, m_2 to n_2 , etc. . . . m_x to n_x , then the average variation will be Q . This Q gives us the

greatest possible average variations $\frac{q}{Q} \times 100$ gives us a value which shows whether the differences between the normal and compared extents increase or decrease with the increase in their normal extents, i. e., whether the valuation increases or decreases

with increase in the normal extents. If $\frac{q}{Q} \times 100$ is equal to 100, i. e., $q = Q$, then in comparison with the valuation of the smallest compared extent, a gradual increase of the compared extent with reference to its normal extent would be shown with increase of

this last in the same series. If $\frac{q}{Q} \times 100$ is equal to 0, i. e., $q = 0$, then a similar gradual decrease would be shown. If $\frac{q}{Q} \times 100$ is equal to 50, then there would be shown on the average for the series neither such a decrease nor such an increase. If the value of $\frac{q}{Q} \times 100$ lay between 0 and 50, then there would be more or less of a decrease; if between 50 and 100, more or less of an increase.

Description of the Reckoning Out of the Method of Relative Difference Sensibility.

If all the individual differences between the normal and compared extents are plus, then the difference sensibility, where the value obtained by the method of relative valuation is 0, increases with increase in the normal extents; where 100, decreases; where 50, it neither increases nor decreases on the average; where it is between 0 and 50, it more or less increases, and where between 50 and 100, it more or less decreases. If the individual differences are both plus and minus, or equal and minus, then it is necessary to reckon out the value obtained by the method of relative valuation for the different normal extents which show plus or plus and equal differences between the normal and compared extents, and likewise for the different normal extents which show minus or minus and equal differences. The reckoning out of this latter will, however, be different from the reckoning in the case of the method of relative valuation only in so far as one allows $n_x = x$ to represent the difference, which is nearest the value null, $n_x - 1 = x - 1$, that which is somewhat smaller, and $n_1 = 1$, the minus difference, which is most

unequal to the simple difference null. The values of q , Q , and $\frac{q}{Q} \times 100$ are now reckoned out, and also the mean of the two values, namely, that obtained from the normal extents which give plus differences and that obtained from those which give minus. If this value is null, then the difference sensibility gradually increases with increase in the normal extents; if it is 100 it gradually decreases; if it is 50, then it neither increases nor decreases on the average; if it is between 0 and 50 it increases more or less, and if between 50 and 100 it decreases more or less.

It is here to be remarked that when we speak of the normal extents in any single series, from the standpoint of difference sensibility, we have to do with an absolute increase or decrease in the series and not with a relative. Only when we compare this increase and decrease in one series with the same in another can we speak of a relative difference sensibility. If one wished to estimate the relative difference sensibility for each series alone, then instead of using the individual differences to reckon out the n values, he must use these divided by their respective individual differences. This, however, has little value. In the same way, we can only speak about an absolute increase or decrease in the valuation, unless we use, in reckoning out the n values in the method of relative valuation, instead of the individual differences, these divided by their respective normal extents. This has also, however, insufficient value. We can, however, speak about a relative valuation or a relative difference sensibility in connection with the values obtained by these methods, in so far as in the case of the first method one takes the valuation where the normal extent is smallest, as normal valuation for the series, and in the case of the other, the difference sensibility

as shown, where the individual difference is null, or nearest above or below null, as the normal difference sensibility in each series. The values obtained by these methods are dependent on this normal valuation and on this normal difference sensibility, and to some extent show a relative valuation and a relative difference sensibility. From these considerations we obtain four methods which show connections between the individual series and the increase in the normal extents, as well as the increase or decrease connected with this in the increase and decrease in the valuation and in the difference sensibility, namely, the two which we have above completely described, the method of relative valuation and the method of relative difference sensibility, and two others which correspond to these, and which give values for the increase and decrease in the relative valuation and the relative difference sensibility in the individual series, instead of the absolute valuation and the absolute difference sensibility, in the individual normal extents. These two latter methods we do not consider further.

The reckoning out of the method of relative valuation and of the method of relative difference sensibility is accompanied by certain difficulties which can be overcome only through carefully choosing the number of experiments and the digits. In the first place, if there is more than a single normal extent, of the same value, one must take the mean of these and also of their respective differences and reckon out the results in the ordinary way with this reduced number of experiments in the series. The same can also be done where there is more than a single simple difference in the same series of the same value. In the second place the average value "q" might be the seat of an error in that the individual parts of $V(m_x + 1)$ work contrary to one another. A table (Part II) with an instance where this occurs is given below, where 2.40 is $q = Q$.

PART I.

PART II.

$N = \text{sum}$	$V(m_x + 1)$	N L	D	M	N	M+N	$V(m_x + 1)$
r	d	20.50	— .50	3	1	4	2
o	e	21.50	—1.50	1	2	3	3
s	etc.	21.25	—2.00	2	3	5	1
	etc.	18.25	—3.75	4	4	8	2
g	f	17.50	—4.00	5	5	10	4
$r + \text{etc.} + g$		$5 \mid d + e + \text{etc.} + f$		$5 \mid 12$			
$m_x + 1$		q				2.40	

But it is easy to see that it should be smaller. This error is not frequent and it would serve no present purpose to reckon it out, in so far as no wholly exact value is sought, but only a relative one.

The method of relative valuation and the method of relative dif-

ference sensibility will be used as an aid in the reckoning out of the difference sensibility in the experiments made by means of the pencil method, and is represented by the symbols M. R. V. and M. D. S.

A.

THE ONE-HANDED MOVEMENTS.

1. *A General Consideration of the First Experiments.*

I have reckoned out these experiments all together, without taking into consideration the length of the curves. The results show that out of a total number of experiments of 1008, $1\frac{1}{2}$ per cent. were overvalued, $\frac{1}{2}$ per cent. were valued correctly, and $18\frac{1}{2}$ per cent. were undervalued. Each of the sixty-six series was at first treated separately. The average differences of the experiments, where over, under and equal valuations took place, along with the average variation of these, were reckoned out for all the experiments. These were then grouped into combinations of six and seven series in a group. These groups were then calculated all together. The average of the whole of the averages of the normal and compared curves, of their differences, of their variations, of the experiments of the sixty-six series, was then reckoned out. In each of the sixty-six series there were overvalued experiments, and in fifty-one of them there were experiments where undervaluations took place. The number of experiments, where correct valuations took place, was so small that we shall not take them into consideration here. The lengths of the normal curves varied between 10 and 60 cm. The average of all the normal curves, where an overvaluation took place, is 22.31 cm. The same, where an undervaluation took place, is 29.62 cm. The average of the differences of the former is 5.48 cm., of the latter, 3.53 cm. When both the normal and compared curves are added together, the average obtained in the first place was 25.96 cm., and in the second, 26.93 cm. In this way one obtains an average difference in valuation of .97 cm. The average of all of the average normal curves of each series, without consideration of the over and undervaluation, is 23.59 cm. The arithmetical average of all the average differences was 2.75 cm. overvaluation, an average difference sensibility of about $\frac{1}{10}$. It is seen, then, that both according to the number of experiments, and also to the average differences, a significant overvaluation had taken place. A review of the series shows that nineteen series had an average normal curve of between 10 and 15 cm., twenty of between 20 and 30 cm., fifteen of between 30 and 40 cm., four of between 40 and 50 cm. and two of between 50 and 60 cm. The average of the average variations of fifty-one series, where an overvaluation took place, was 2.37 cm. For thirty-seven series, where an undervaluation took place, it was 1.00 cm. As this was small in such a general consideration, I have not taken it into account.

It is desirable now to find out what sort of influences the upwards and downwards direction of the motions, the changes in the normal lengths and in the height of the starting points, as well as special differences in the observers and in the methods of experimenting, could show. In the case of the first fifteen series the height of the starting point was not taken, and we do not consider these further. Instead of a lead pencil being used, also a small brush with lamp black mixture was used. Of the remaining fifty-one series, thirty were made in the upwards direction¹ and twenty-one in the

¹ See table II. p. 407.

downwards.¹ The accompanying tables¹ show the heights of the starting points from the lowest, which is marked with minus, to the highest, which is marked with plus. The horizontal starting point, which could be only roughly estimated, is marked with zero.

In considering this first group of experiments it is important to remark that they were all made more with respect to the convenience of the observer than to any previously determined plan of experimenting. The beats of the metronome, which regulated the rate of movement as well as the length of the curves, show also to some extent the disposition of the observer apart from the exactness of his judging. Very quick time and very slow time was disagreeable. A look at the diagrams² shows also that the average lengths of the normal curves for the series varied more under the horizontal point in the case of the upwards movements, but in the case of the downwards movements more over the horizontal point. Not only also are the limits for the average normal lengths greater in these directions, but the number of series is also greater in the case of upwards movements under the horizontal point and in the case of downwards movements above the same. Apart from series 20 of the upwards movements and series 17 of the downwards, a certain attempt was made to make all the normal curves in each series as much as possible equal to one another. There was nothing binding in this, however, as it was thought that it could possibly disturb the power of valuing correctly between the normal and compared curves. Still a certain indirect valuation in this respect was present in each series. Each new normal curve was more or less of an attempt to reproduce the former. In so far, such a valuation was implicated. The variation of the normal curves gives a value for this. In the case of the upwards motions, the changes in the variations follow the changes in the lengths of the average normal lengths, with few exceptions. In the case of the downwards motions this is also very often the case. This shows that the difference sensibility is dependent on the length of the normal curves. In many cases also, the normal curves were made of the same length as the just previously described compared curves. In such cases the compared curves were made smaller than their normal curves. These two pairs of curves appear, then, to form a separate basis of valuation. The above described average values of all the series can be considered as a sum of such valuations, of which the average differences show the exactness of the valuation. As the average overvaluation of the experiments of all the 66 series, where overvaluation took place, is 5.48 cm., the corresponding average undervaluation 3.53 cm., and the average overvaluation in all the experiments was 2.75 cm., we see that the difference sensibility would amount to about $\frac{1}{20}$. $+23.59$ cm. is the average normal length of all the experiments; $\frac{2.75}{23.59 \times 2} = \frac{1}{20} =$ difference sensibility. In general, the curves show no very exact changes caused by changes in the lengths of the normal curves and in the starting points. The larger normal curves show in the case of the upwards movements, where the starting points were, -30 and -60 cm. larger average differences than do the smaller normal curves. In the case of the downwards movements, the average differences show the opposite results, so that the smaller normal curves are more overvalued than the larger. It is easy to see that on account of the changes in the average normal lengths and in their variations, the influence of the changes in the starting points in these experiments can not be

¹See table I. p. 406.

²See original.

very exactly determined. Where also the variations of the average normal curves and the changes of the starting points do not vary much, the variations of the average differences are too great to give exact results. In order to find out the influence of the starting points, I have made the two-handed experiments, which are given below (B), and I have also made other additional one-handed experiments to determine the influence of the extent of the normal curves. I give in addition certain remarks on the individual series. By the method of relative valuation above described, I have reckoned out in the individual series the tendencies towards under and overvaluation. From this it appears that in the case of upwards motions, where the starting points are very low or very high, an undervaluation takes place more and more with the increase in the length of the normal curves. Under and at the horizontal point, however, more of an overvaluation takes place. These overvaluations and undervaluations are not very much dependent either on the changes in the average normal lengths nor on the changes in the variations of the individual normal curves from their average normal lengths. In the case of the downwards motions with low starting points, a distinct undervaluation of a similar kind takes place, and is also present where the starting points are taken quite high. Only when the starting points are very high is an overvaluation met with.

2. *Consideration of the Individual Series.*

a. Series 11 and 12 of the upwards motions and series 8 of the downwards were made while the observer was seated. They show no special deviations from the other series. Among the later experiments are some which show more particularly differences between the standing and sitting posture.

b. With and without the beating of the metronome. Series 16, 24, 26 and 5 of the upwards motions were carried out with no accompanying beating of the metronome. It is to be remarked that the variation of the normal curves in each case is very small. The angle method experiments to be described later will show the more important influence of the rate of movement upon the relative valuation. In general it appears that the observer without the beating of the metronome, chooses the most convenient rate of movement for the starting point and length of movement. The metronome's beating has less influence on the variation of the normal lengths with such as are accustomed to see the regular motions of machines, or to make regular motions themselves. In these cases the two groups of sensations, the normal and the compared, are more or less localized in the organs of motion. The motions are automatically made. Where this is not the case the metronome exercises a greater and more distinct influence upon the variation in the normal curves. Along with this the two groups of motion sensations can be considered more as sensations of the willing impulse, which are localized in the glottis and in the breathing apparatus. As the metronome tends to make these motions also automatic, however, these sensations are not so important (as materials in investigating) as in either case where one makes movements of unusual length and height.

c. Influence of the lengths of the radii. The irregularity of many of the average differences which are dependent neither upon the differences in the variations of the normal curves, nor upon the differences in the heights, can be very well ascribed to the bending

of the arm. It is desirable, therefore, to find out what relation the radii of the normal curves bear to the radii of the compared ones. The curves were more or less bent towards the extremities. The measuring of the radii, then, was confined to the normal arcs in the middle of the curves. A large wooden compass was used to measure these directly. Where the radii of the normal curves are smaller than those of the compared, the differences are marked with plus, where larger with minus. In the case of the upwards motions, it appears that the normal radii are smaller where the starting points are very high and very low than where the points lie between these extremes. It is meant that the variations in the lengths of the normal radii and the average differences between those of the normal and compared, show the value of this measuring. Where one variation is relatively small, so is the other, where large, likewise. From this it follows that there existed regular fluctuations in the bending of the arm. The bending of the arm alternates more or less in each series, and the average differences of the radii show, then, how the valuations of the movements are influenced through this.

If we consider now more particularly series 2, 5, 6, 7 and 9 of the upwards movements, which have starting points very nearly alike, we find that by increasing the height of the starting point as well as by lengthening the average lengths of the normal curves, the lengths of the average radii of these increase. If we consider, however, only the changes of the lengths of the average differences of the radii in connection with the changes in the average differences of the curves, then we find that they stand in a directly inverse relation to one another. The exception in series 6 can be very easily ascribed to the greatness of the variation.

In series 1, 5, 6 and 11 of the downwards motions, the lengths of the average normal radii increase with the heightening of the starting points. The first three series were made under the horizontal point and can be considered by themselves. The average differences of the normal and compared curves stand in a directly inverse relation to the average differences of their radii. Apart from the starting point the increase in the average lengths of the curves is accompanied by a decrease in the average differences of the normal and compared curves, and by an increase in the average differences of the radii themselves.

In the case of both upwards and downwards movements, then, it results that an increase in the average differences of the normal and compared curves is accompanied by a decrease in the lengths of the differences of their radii. The greater normal curves are, however, more overvalued in the former case than in the latter.

d. What relation do the curves bear to their corresponding angles and perpendiculars? I have drawn diagrams², which give the average values for the above described series. For series 2, 5, 6, 7 and 9 of the upwards motions, the average of the heights of the starting points was —52.10 cm. The average of the average normal curves is 26.67 cm.; of the compared 34.10 cm.; of the average radii of the first 56.72 cm., of the last 61.96 cm. In the case of the downwards movements, the average of the heights of the starting points for series 1, 5 and 6 was —42.87 cm. The average of the average normal curves is 16.96 cm.; of the average compared 17.65 cm.; of the radii of the first 54.19 cm., of the last 56.63 cm. From these values I have reckoned out the corresponding angles and the perpendiculars to the horizontal plane lying between the extremities of the curves. Dr. J. Loeb¹ has shown

¹Archiv für die ge. Phy. d. M. u. d. Th. von Pfüger. Bd. 46, I. S. 1; Bd. 41, S. 107.

²See original.

that where the starting points lie under the horizontal plane, motions made at right angles to this plane show great deviations between the normal and compared distances, and this just so much the more that the starting points are distant from the horizontal plane. If the observer had described such straight lines without changing the angles of movement, the above perpendiculars would show my results to agree with those of Loeb's. My experiments show, however, that in order to make lines of equal greatness, the angles at the shoulder should be taken as the standard and not the perpendiculars. In both cases, whether one makes movements with the arm held straight out in the form of curves or with the elbow joint bent in the form of straight lines, it is to be assumed that the mass of sensations are so nearly equal to one another that they do not need separate consideration. There exists a probability for this in so far as the contact and holding of the elbow joint when the arm is fully stretched out, compensate for the other kinds of stimuli which arise through the bending of the elbow joints and the muscles. How far the sensations due to the moving muscles and to the so-called will-impulse are to be taken as the basis for the valuation, will be explained later. The chief point to be noted here is that the angle at the shoulder joint (or joints) is to be taken as the basis for the valuation of the distance moved. The above described changes in the perpendiculars bear a similar relation to their corresponding angles at the shoulder in the case of the upwards movements to what 8.12 cm. and 26.42 cm. bear to $26^{\circ} 56'$ and $31^{\circ} 34'$, and in the case of downwards movements to what 9.25 cm. and 5.72 cm. bear to $15^{\circ} 53'$ and $17^{\circ} 48'$. These two accompanying diagrams¹ give us examples as to how these perpendiculars stand to their curves or angles at the shoulder. This relation changes with the starting points. In the horizontal plane both stand in a very close relation to one another. High above and low below, the greatest deviation between the two occurs. Loeb used the two arms in his experiments. The normal arm had always the horizontal plane as starting point, while the starting point for the other arm was varied.

3. *The Later One-Handed Experiments of the Winter Semester, 1892-3.*

As above mentioned I have made further experiments with special respect to the changes in the average differences which arise through changes in the lengths of the normal curves and in the heights of the starting points. The latter I have considered more especially in the two-handed experiments to be described later. The experiments we consider here, consisting of ninety-nine series, were all one-handed. In twenty-nine series the starting points were changed. In all the ninety-nine series an attempt was made as much as possible to keep the variation of the normal curves for each series small. The number of experiments for each series was ten. The observers were Mr. Child, "C.," and the experimenter himself, "S." The former was seated, while the latter remained standing and acted as his own experimenter. The horizontal plane for the former was 102.5 cm. above the floor, and for the latter 130 cm. above the same. The number of series made by the first was nine in the upwards direction and eleven in the downwards; by the last, forty-five in the former and thirty-four in the latter. The beating of the metronome was, with the exception of eleven series, by observer S., where it made 180 beats in the minute, and five others where it made 120, kept at sixty beats to the minute.

¹ See the original.

(1.) *Observer C.*—The series by observer C. were made, with two exceptions, in order to find out the changes in the average differences caused by changes in the lengths of the average normal curves. In general the average differences were not nearly as large as in the earlier experiments. The variation in the normal curves follow in their relative values the changes in the lengths of the average normal curves. Where the latter decrease, so do the former. With the same decrease the average differences in the case of the downwards motions also decrease, and inversely in each individual series the increase in the individual differences is accompanied by an increase in the lengths of the normal curves.

Upwards movements. The accompanying tables¹ show the two groups of series of upwards motions by observer C. Series 4, group II., had, besides the starting point in the horizontal plane for the normal curves, a different one for the compared. It is, then, left out of consideration here. The question presents itself here, "How does the relative and absolute difference sensibility decrease or increase with the increase in the lengths of the normal curves?" The variations of the normal curves and the differences of the normal and compared curves can only be considered as absolute spatial changes, which serve as a basis for an absolute difference sensibility. With the increase in the normal curves they also increase. Hence the absolute difference sensibility decreases. The M. R. V. shows that where the individual differences in each series are considered, each increase in the normal curves, where these are greatest, is accompanied by a decrease in the individual differences; where they are smallest by an increase, however. The average differences show that with increase in the average normal curves the average differences increase in group II., and inversely in group I. decrease. Were the average differences not reckoned out, one could estimate the difference sensibility to a certain extent by the method of relative difference sensibility. The figures obtained by this method show that in group I. the difference sensibility increases with the increase in the individual normal curves, and this just in proportion as the individual normal curves decrease. In group II., however, it decreases, and just in proportion as the normal curves increase.

Downwards movements. Two groups of experiments were also made by observer C in the downwards direction. The M. R. V. shows a constant increase of the absolute values of the individual differences in the series with the decrease in the normal curves, and this the more the average normal curves increase. The average differences in the two groups are themselves different. In series 1-4, group II., they are greater than in series 1-8, group I. The difference sensibility is then inversely greater with the latter. In both groups the increase in the lengths of the average normal curves appears to cause neither an increase nor a decrease in their average differences. They do not allow of a judgment in regard to the absolute difference sensibility. In both groups the normal variations decrease also with decrease in the average normal curves. As above mentioned there is ground here for concluding that the absolute difference sensibility decreases with the decrease in the lengths of the average normal curves.

Changes in the starting points. The two remaining series of downwards movements of observer C. show with increase of the starting points a greater decrease of the compared curves in relation to their normal. The absolute difference sensibility was great-

¹ See the original.

er above than below, because the differences in both cases were greater. This agrees with what was said in regard to the other downwards movements.

(2.) *Observer S.*—Upwards movements. The six groups of upwards movements of observer S. were made under different conditions. In all of these, apart from group V., the variations were greater than where the average normal curves were greater, and smaller where these were smaller. Hence the difference sensibility decreases with increase of the normal curves. The lengths of the averages of these vary between 5.95 cm. and 53.95 cm. The average differences in group I. show no regular increase or decrease, with the increase in the lengths of the average normal curves. The average of the last three series is greater than that of the first three. This shows a finer difference sensibility for the greater curves than for the smaller. The dividing of the average of the compared curves by the same of the normal ones shows the difference sensibility from 5.95 cm. to 23.10 cm. to gradually increase. At 30.15 cm. it is again less and at 44.70 cm. is about the same as at 23.10 cm. The M. D. S. shows that the difference sensibility, as shown within the limits of a group, increases on the average with increase in the individual normal curves.

Group II. of observer S. was different to group I. only in so far as the metronome beat 180 times in the minute instead of sixty. By this the rate of the movement was considerably increased. In general an overvaluation took place, and this is greater with the larger normal curves and smaller with the smaller. The variations of the normal curves are also greater with the greater average normal curves and smaller with the smaller. Series 3 forms an exception to this. The normal variation of the same is greater than the normal variation in series 2. From the standpoint of the average differences and of the average variations of the normal curves, there appears a greater absolute difference sensibility when the normal curves are smaller than when they are larger. The M. R. V. shows in general in the individual series a decrease in the individual differences with increase in the individual normal curves. This decrease is somewhat larger when the normal curves are greater than when they are smaller. In series 2, 4 and 5, the M. D. S. shows that the difference sensibility in the average decreases with increase in the individual normal curves, while in series 1 and 3 it decreases.

Group III. was made in such a way that in each series four curves of different normal lengths were made after one another in such a way that the second was greater than the first, the third greater than the second, and the fourth greater than the third. Each time it was attempted in the following sixteen series to repeat these four experiments with similar normal lengths. For the first twelve of these, the metronome beat sixty times in the minute, and for the last five (group V.) 120 times to the minute. In groups III. and V. the variations are relatively greater than in the other groups of these experiments. They follow, however, in size the lengths of the average normal curves. With the larger ones they are larger and with the smaller they are smaller. We obtain, then, a greater absolute difference sensibility in the case of the smaller ones than in the case of the larger ones. The average differences themselves are smaller with the larger curves than with the smaller curves. An undervaluation takes place with the former and an overvaluation with the latter. This overvaluation is greater than the undervaluation. From the standpoint of the average differences the difference sensibility is less in the case of the larger normal curves than is the

case of the smaller. The M. D. S. shows in the individual series that the individual differences where the average normal curves are large, decrease with the decrease in the individual normal curves, and where the average normal curves are small rather increase. This agrees with the results obtained from the average differences.

Group IV., where the metronome beat 120 times in the minute, shows in many ways results similar to those in group III. The average differences vary with one another somewhat more, and in series 2 the variation of the normal curves is greater than would be expected from group III. In series 3 it is smaller.

Group VI. was made so that the normal curves in series 1, 2 and 5 were made without rubbing the papers. The compared curves were described in the ordinary way, with the lead pencil. The M. R. V. shows that the increase of the individual normal curves in the case of the larger normal curves is accompanied by a decrease in their differences; in the case of the smaller by an increase. The variations of the normal curves show in series 5 a small variation, in series 1 and 2 a large one. From the standpoint of the variations the difference sensibility decreases with increase in the average normal curves. The average differences in series 1 and 2 give a mean which is smaller than the average difference in series 5. From the standpoint of the average distances, then, it is to be concluded that with increase in the average normal curves, the absolute and relative difference sensibility increases. This agrees with the results from the method of relative valuation. In general these series show no great deviation from group I. The rubbing in the case of the normal curves appears to exercise no great influence upon the valuation. Series 3 and 4 were made so that the arm was fatigued through lifting of a weight of five kg. It shows, however, nothing new.

Group V. was carried out from the standpoint of the variations of the compared curves. The normal curves were made in such a way that the eyes could see only and alone the making of the normal curves. By this it was thought to keep the pure stimulation or the normal curves more constant and to make the average differences follow more the real differences in the length of the motions or of the space valuations. The average normal curves varied in the 8 series between 6.17 cm. and 30.30 cm. Their variations give no regular increase or decrease with the same in the normal curves. The average of the larger average normal curves is greater than the average of the smaller. From this standpoint, then, the difference sensibility is greater in the case of the latter. It is, however, much smaller than in the other series. From 12.95 cm. to 15.79 cm. the average differences rise. From 20.65 cm. to 30.30 cm., and from 12.95 cm. to 6.17 cm., they decrease. In general the larger normal curves show much larger average differences than the smaller. The M. R. V. shows in general a small increase in the individual differences of each of the series with increase in the individual normal curves. In three series it is just the reverse. In the other five, however, it is so much to the contrary that an average increase takes place. The relative difference sensibility obtained from the average differences is greater in the case of the larger normal curves than in the case of the smaller. An exception to this occurs in series 6, where it is greatest. We find by the M. D. S. that the difference sensibility increases, in series 3, 4 and 7, with increase in the individual normal curves. In series 1, 2, 5, 6 and 8, however, it decreases.

Downwards movements. I consider now the downwards movements of observer S., by which changes in the difference sensibility caused

by changes in the lengths of the normal curves are sought. Only two of the twelve series had a more rapid rate of movement, namely, 120 beats of the metronome in the minute. In the case of the others it beat sixty times. The average normal curves varied between 2.14 cm. and 23.25 cm. Their variations increase gradually with increase in their lengths. From this standpoint, then, the absolute sensibility decreases gradually with the increase in the lengths of the average normal curves. The average differences, on the contrary, also gradually decrease with increase in the normal curves. On the whole they are very small. The greatest difference sensibility is at 11.50 cm. and at 4.40 cm., where the average differences are respectively $-.05$ cm. and $+.05$ cm. From these normal lengths it decreases with the longer and shorter normal curves. At 23-25 cm. the difference sensibility is greater than at 2.60 cm. (the average of series 5-9). Within the limits of the individual series the M. R. V. shows that a decrease in the lengths of the individual normal curves is accompanied by an increase in the size of the average differences, and this in proportion as the average normal curves increase. Series 11 and 12, which were made with the quicker rate of movement, show an increase in their average differences, and in their average normal variations above motions of the same size which were made with a slower rate. One series of the downwards movements, in which the normal curves were made without rubbing of the surface, gave an average normal length of 36.40 cm., with a variation of 4.35 cm., and an average difference of 12.77 cm. The contact of the hand with the table through the lead pencil appears, in the case of the downwards movements, to exercise a great influence. The M. R. V. shows an increase in the individual differences with decrease in the individual normal lengths.

Still another series was completed after fatiguing of the arm with a five kg. weight. The average normal length was 19.04 cm., their variation 2.47 cm., and the average difference 4.18 cm. The fatiguing brought out an overvaluation in comparison with series 3. In these downwards movements fatiguing causes the difference sensibility to decrease, both from the standpoint of the variation of the normal curves and from the standpoint of the average differences. The M. R. V. shows that an increase in the lengths of the average normal curves is accompanied more by a decrease in the average differences than by an increase.

The series so far described were made so that the starting points of the compared curves were the end points of the normal. Six series were now made in which the starting points for both normal and compared were the same. The lengths of the average curves varied between 8.12 cm. and 58.82. The variations and the normal curves themselves vary more in the case of the series considered above. They show no increase with the increase in the length of the average normal curves. The average differences are not very much different from the average differences of the other series. The greatest difference sensibility is at 17.90 cm. Over and under this it varies. It is least where the normal lengths are very great. The average differences vary between 0.00 cm. and -2.42 cm. In only one series is there an overvaluation. At 41.60 cm. the average difference is $+1.90$ cm., at 58.82 cm. it is 2.42 cm., and apart from these an undervaluation of from 0.00 cm. to 1.00 cm. is always present. The M. R. V., apart from series 1, where the average normal curve was 58.82 cm., shows an increase of the individual differences with increase in the individual normal curves.

Experiments with changes in the heights of the starting points. So far, we have considered these later results from the standpoint

of the lengths of the normal curves. As above mentioned, I have also carried out some such series to determine how far changes in the average differences were due to changes in the length of the normal curves. In the downwards direction thirteen series were made in two groups. In group I. the average normal curves varied between 13.75 cm. and 15.70 cm. Their starting points varied between 60 and 15 cm. under the horizontal plane. The average differences are for the lower of these, smaller, and for the higher, greater. The variations of the normal curves are greater in the first than in the last. The variations of the normal curves, then, show a smaller absolute difference sensibility at the lower points, while from the standpoint of the average differences a greater at these points. The M. D. S. shows more of a decrease of the difference sensibility with the increase in the lengths of the individual normal curves. The M. R. V. shows an approximate equal increase of the individual differences with increase in the lengths of the normal curves more and more as the starting points are raised.

Group II. consists of nine series, of which two are duplicates. The starting points vary between -45 cm. and +60. The variations of the normal curves all fall under 1.50 cm., and are somewhat greater above than below the horizontal point. The average normal lengths vary between 7.40 cm. and 12.95 cm. The larger of the same are the upper ones. The average differences are greater under than above the horizontal plane. In the former case there is an average overvaluation, in the latter an average undervaluation. None of the average differences amount to more than +1 cm. or to less than -1 cm. The absolute average undervaluation in the higher series is greater than the absolute average overvaluation in the lower ones. The M. R. V. shows in the former series an increase of the individual differences with the increase in the individual normal curves, while in the latter series a decrease is shown. In these latter series, we find that the increase in the average differences agrees with the increase in the individual differences, as shown in the separate series. The M. R. V. shows in the horizontal plane a decrease in the difference sensibility to be accompanied by an increase in the individual normal curves. With the heightening and lowering of the starting points this decrease becomes less, so that an increase takes place. On the average, also, there is an increase. Groups I. and II. are different from one another in so far as the first shows, on the whole, a larger overvaluation than the second.

Two groups of series of these kinds of movements were made in the downwards direction. In group I. the lengths of the normal curves varied between 8.22 cm. and 11.65 cm. Their variations are greater at the higher starting points, namely, 0.00, +15 and 30 cm. than at the lower, namely, -45, -30 and -15 cm., and also than at the highest, namely, +60 and +45 cm. The M. R. V. shows that the increase in the lengths of the individual normal curves at the lower points is accompanied by an increase in the lengths of the individual differences, but at the higher points by a decrease. The average differences varied between -1.00 cm. and +1.75. The average differences at points -45 cm., -30, -15 and at 0.00 are +1.17, -.55, -.87 and -.02 cm. At the points +15, +30 and +45, they increase. They are +1.36, +1.50 and +1.75 cm. At +60 it is -.9 cm. The variations of the lengths of the normal curves appear to have no influence on these average differences. We can assume, then, from these experiments that the difference sensibility is smaller above the horizontal plane than below it. Apart from the lowest point, -45, it is greater under. In group II. the normal

curves are longer. They vary between 16.72 cm. and 21.42 cm. The starting points were +15, +45 and +60. The variations are much the same as in group I. The average differences decrease with the increase in the lengths of the average normal curves and in the heights of the starting points. Along with this the difference sensibility decreases.

This finishes the results obtained from the one-handed experiments by means of the pencil method. The results are:— 1. The results obtained by the pencil method show that in valuing space distances, the angles at the shoulder joint should be taken as the measure of the sensibility in the making of arm movements.

2. The larger normal curves, when the movements are upwards, are more overvalued than are the smaller, where they are downwards more undervalued. This varies, however, with the observer and with the rate of movement.

3. The results obtained by the M. R. V. show that the individual compared curves increase on the whole with increase in their normal curves in both upwards and downwards movements. When the normal curves are small they rather decrease, however. This relation between the normal and compared curves is dependent also on the length of the normal curves, as also on the heights of the starting points.

4. The variations of the normal curves show that the absolute difference sensibility is greater where the normal curves are smaller than where they are larger, because the variations are greater in the latter case than in the former.

5. The upwards movements at points above the horizontal plane are overvalued, and under it they are undervalued. The reverse holds where the movements are downwards. This over and undervaluation varies more in the former than in the latter movements. Observer C., who was seated during the experiments, overvalued upwards movements above the horizontal plane and undervalued downwards movements under the same plane.

6. Very rapid rates of movement caused greater average differences in the larger and smaller normal curves than medium rates of movement.

7. It is impossible to overcome great differences in the valuations which depend upon the individual observers, because the shoulder joints in the holding of the arm out and moving of it allow the curves to vary too much on account of the radii and centres of movement.

B.

THE TWO-HANDED MOVEMENTS.

a. *Influence of the Starting Points.*

We consider, first, the differences in the individual series, which are due to changes in the heights of the starting points, in the lengths of the normal curves. In such psychological curves as these, where one investigates the ability to make two movements equal to one another, we have to deal with two groups of sensations rather than with two single sensations, as in the case of tone investigations, for example. For we can say that the two motions are equal to one

another when the two groups of sensations are equal. I give, first, the results of the changes in the heights of the starting points in influencing the valuation of these two groups. The heights of these remain for the normal curves in the same series always the same, while those of compared curves change with each series. In the upwards direction five groups were made, two by observer Kp., one with the right hand for the normal movements and one with the left hand for the same; two by O. (observer) S., and one by O. K. Each group had six or eight series and each of these is the average of ten experiments. In the diagrams 1, 2, 3, 4, both the upwards and downwards groups of movements are described.¹ Loeb (l. c.) has shown by his method that in upwards movements, where normal and compared movements are made simultaneously (simultaneous movements), the compared decrease more and more in comparison to the normal. They are undervalued. In the five groups considered now apart from changes in the lengths of the normal curves, there is only partly such a decrease. In groups c and h of O. S., such a decrease is most distinctly present. In group h the decrease from starting point (S. P.) -4 cm. to +6 is somewhat more than 2 cm. From S. P. +6 cm. up to S. P. +16, it is somewhat more than 1 cm. From starting point +16 up to +26 it increases a little, and then from +26 to +36 decreases nearly 2 cm. It is to be said here that in all these two-handed experiments the starting points of the normal hands were taken for observer Kp. at -40, for K. at -6, for R. at -10, and for S. at -4. In group c of the last there is an irregular increase from S. P. -24 up to S. P. +26, where a small increase at point +26, and then again a decrease up to +55, takes place. In the two groups, a and g, of O. Kp., the decrease is only partly to be met with. In group a, the line (Diag. 2)¹ sinks from S. P. -40, nearly 2 cm., down to -30. From -30 to -20 it rises 3.5 cm., and from -20 to -10, 1.50 cm. From -10 to 0.00 it decreases 3 cm., and at +10, .2 more. From +10 up to +20 a great deviation takes place and then it rises 2 cm. up to +30 cm. In this group, then, the increase and decrease in the differences vary. At first there is a decrease, then an increase, again a decrease and then again an increase. This variation is not dependent on the lengths of the normal curves, for the average of these does not vary sufficiently. Group b, of O. R. (Diag. 2)¹, shows an increase in the first three series and a decrease in the last three. This variation, with the increase in the heights of the starting points, agrees with group a of O. Kp., but agrees only partly with Loeb's results. In the other group, g, of O. Kp., there is an alteration between an increase and decrease, and *vice versa*, with each new starting point, and the results at the last starting point do not vary much from those at the first. The large variation in the lengths of the normal curves makes it possible that these last served as a means in making the valuations correct. We will later become acquainted with the influence which the lengths of the normal curves exert on the average differences. On the whole it is to be said that with upwards movements there is only partly an increase in the lengths of the compared curves in comparison with those of the normal where the starting points rise. It is worthy of remark that in group g, a, b and c, an inversely correct valuation takes place at the horizontal plane. From this point the average differences increase and decrease. Six groups of downwards motions were carried out, two by O. K., two by O. R. and two by O. S. In each case one was made with the right hand, for the normal curves, and one

¹ See original.

with the left for the same. In group d, of O. K., an increase in the average differences occurs from S. P. -6 up to $+34$. From here up to $+64$, a decrease takes place. Inversely, in group l of O. K., there is a decrease from -6 to $+14$ and an increase from $+14$ to $+54$. Loeb found that by his method an overvaluation took place with increase in heights of the starting points where the motion was downwards. He says, however, that these movements are not so correctly made as those in the opposite directions. In this group l of O. K., it is possible that the difference in the lengths of the normal curves from S. P. -6 to $+14$, makes a sufficient difference in the compared curves to explain the decrease. In group d there is also a difference in the normal curves in connection with the decrease in the starting points. I think, however, that this is more dependent on the heights of the starting points than on the differences in the lengths of the normal curves. We will later take the opportunity of treating this further. Group e of O. R. shows a regular increase from starting point -10 up to $+30$, which is possibly dependent upon changes in the lengths of the normal curves. Group m, by the same, shows five changes between increase and decrease from points -10 cm. up to $+50$. From $+10$ to $+30$ we find, with the exception of a decrease from 1.50 cm., between $+10$ and $+20$, a rather regular increase. From $+30$ to $+40$ it is possible that the difference between the lengths of the normal curves causes the decrease. There remain two groups of O. S. In group f we meet with a regular increase from -4 up to $+26$. From $+26$ up to $+46$ a decrease takes place, which is accompanied by a shortening of the normal curves. Between $+46$ and $+56$ an increase again is present. In group n a decrease, at first from -4 to $+6$, is seen, and after this a distinct increase from $+6$ up to $+16$. Between $+16$ and $+26$ a decrease occurs, and from $+26$ up to $+56$ the increase is clear enough. Probably the difference, 3.50 cm., in the lengths of the normal curves accounts somewhat for the small increase between points $+26$ and $+36$.

Apart, then, from the consideration of the differences caused by changes in the lengths and variations in the normal curves and by the time relations of the same, we can only agree partly with Dr. Loeb's results. Movements in both directions show variations from the increase and decrease in the differences which his method gives. These are greatest where the starting points for the compared movements are taken very high or very low. They also depend upon the observers.

b. Influences of the Lengths of the Normal Curves.

I give now some remarks upon the differences which are caused by the changes in the lengths of the normal curves. I treat the series each by itself, because otherwise the heights of the starting points would exercise too much influence. The individual series show the effect of the changes in the lengths of the normal curves without the variations which arise through these latter. Preliminarily we can say that changes in the lengths of the normal curves do influence the compared ones.

To show this I have used the M. R. V. The average of all the values obtained by this method for the upwards movements was 41.32 , for the downwards 41.8 . In the former, therefore, the individual compared curves decrease in comparison with their normal ones, with increase in these latter. In the latter they rather increase than decrease. This increase is, however, rather small. One might say that in this case the normal and compared curves increase and

decrease relatively to one another, with increase in the lengths of the normal curves. This signifies that if a normal curve has a certain value, each increase of 1 cm. in the normal curves is accompanied by an increase of a little more than 1 cm. in the corresponding compared curves where the movements are downwards. Where these are upwards, the accompanying increase will be a little less than 1 cm. These increases and decreases, as determined by the method of relative valuation, vary according to the observer and the heights of the starting points. In group m of O. R., for example, this decrease rather increases with the elevating of the starting points. The average normal curves themselves do not vary much. In group e the average of the values shows an average increase, while in group m an average decrease. This agrees with the average differences. Again, in group l of O. K., a decrease obtained by this method shows itself from points -6 to +14, and from +14 to +54 an increase. This agrees with the decrease in the average differences from -6 to +14, and with the increase from +14 to +54. At starting point +14 the very high number, 96, is worthy of notice. In group d the values obtained by the M. R. V., from points +46 on, increase and decrease with the same of the average differences. Group n of O. S. shows at S. P. +36, and at +46, higher values, which accompany the increase in the average differences, and group f a high value, 80, corresponding to the greater increase in the average differences. The upwards movements of group g of O. Kp., apart from S. P. 0.00, show an agreement between the alterations in the average differences and in the values obtained by the M. R. V., so that an increase in the one accompanies that in another; so also with a decrease. From starting points -40 to -10, group a of O. Kp. agrees with the above, as also from S. P. 0.00 to +40. In group b of O. R. the increase and decrease obtained by this method follow the same in the average differences. In group h the values obtained by this method appear to show the opposite results to that of the increase and decrease in the average differences. From points +6 to +36 these values do not vary much, however, from such as are given by the differences when a relatively correct valuation takes place. In group c the values obtained by this method agree with the increase accompanying the elevating of the starting points of the average compared curves, in so far as both show an increase from -24 to -14 and under, from 0.00 to -56 a decrease. If we put these results now together, we find that the decrease and increase of the compared curves in each series, in comparison with their normal ones, agree sufficiently nearly with the decrease and increase in the average differences. From this it is to be assumed that the compared curves, in comparison with their normal ones in the separate series, vary according to the heights of the starting points and the direction of the motions. This presupposes the same for the differences between the average differences. The values obtained by the M. R. V. show in the downwards movements more of an increase than a decrease of the individual compared curves in comparison with their normal curves. This agrees with the average increase of the average compared curves over the average normal curves. On the contrary, a similar decrease is shown by the corresponding values of the upwards movements, and this agrees with the average decrease of the average compared curves over their normal curves. These values vary, also, as we have shown above, according to the heights of their starting points.

In both cases we find, however, that this decrease obtained by the M. R. V., in the case of the upwards movements and increase

in the case of the downwards, is not altogether complete. Were this so, then the values obtained by this method for the decrease would be 4.00, and for the increase, 100. They are only relative values. In both cases the average values show deviations from the above. It is only the variations of these values which are large, according to the differences in the starting points, lengths of the normal curves and direction of the movements. It will still be our duty now to again consider the individual series and groups of series of the different observers in so far as all three changes mentioned above work together.

The Combined Effect of Changes in Heights of the Starting Points in the Lengths of the Normal Curves and in the Direction of Movement.

The average differences show in general, with few exceptions, that at and above the horizontal point, the upwards movements exhibit a decrease of the average compared curves in comparison with their normal ones with the raising of the starting points, while the downwards ones show an increase. This is not very continuous in these experiments on account of the variations in the average normal curves and in the observers. After this general conclusion in regard to differences due to changes in the direction of movement, I consider now the combined effect of changes in the lengths of the normal curves and in the heights of the starting points.

(a.) In the downwards movements. The two groups of experiments of O. K. show that where the right hand makes the normal curve and the heights of the starting points are gradually increased from -4 to $+34$, and the left hand the normal curve with increase in the points from $+14$ to $+54$, the average compared curves in comparison with their normal curves more and more increase. In these groups the variations in the average normal lengths are small enough to leave them out of account. In group e of O. R., on the contrary, one can only compare the values from points $+20$ to $+40$ with one another. The variations of the average normal curves within these limits are not great. They show a distinct increase in the average differences with the increase in the average normal curves. In group m of O. R. the decrease in the average differences accompanying the raising of the points is dependent on this changing of the starting points. This occurs very exactly between points 0.00 and $+30$, and the variations in the average normal lengths between these points are very small. In the right handed group of O. S. there is a distinct increase in the average differences between the points -4 and $+26$, dependent on the changing between these points. From $+26$ to $+46$, the normal lengths vary too much to compare them with one another, while from points $+46$ to $+56$, there is again an increase in the average differences. In the left hand movements (where the left hand makes the normal curve) of O. S. there can only be considered an increase from points $+36$ up to $+56$. From the points $+6$ to $+26$ there is only a very small increase, and the variation of the average normal lengths between these points and between $+36$ and $+56$ is too great in order to compare the average differences between $+6$ and $+56$ with one another. In the downwards movements, then, apart from the left hand group of O. R., we find an increase in the average differences with the increase in the average normal lengths, where the variations in the average normal lengths in the groups are not large.

(b.) In the upwards movements. In group a of O. Kp. there is a decrease in the average differences between points -10 and $+16$ distinctly due to the changes in the starting points, because the

average normal lengths vary between these points very little. On the contrary between points -30 and -10 there is an increase. Between these points also, the average normal lengths vary very little. From these groups it is to be assumed that lowering of the starting points under the horizontal plane makes the average differences increase, but above the same, decrease. In group *g* of O. K. there is under the horizontal plane, -40 and -30 , an increase, but above the same, between $+10$ and $+20$, a decrease. Apart from these groups the variations in the average normal curves are too great in order to compare the average normal differences with one another. In group *h* of O. S. there occurs between points $+6$ and $+26$ a distinct decrease in the average differences, which is dependent upon the raising of the starting points, because the variation of the average normal lengths is not great. Apart from these, there is in general a decrease, which, however, can be ascribed to the variation in the average normal lengths and not to that of the starting points. Group *e* of O. S. is in every way similar to group *h*. Group *b* of O. R. shows the average differences to decrease between $+10$ and $+20$ and between $+30$ and $+40$. Otherwise the variations in the average normal curves are too great in order to determine the influence of the starting points. In the upwards movements we have found that the average differences decrease with raising of the starting points. It would be possible, where this variation in the average normal lengths, above spoken of, is large in the groups, to reckon out a curve which would show the influence of the changes in the lengths of the average normal curves. In so far, however, as we have already found that, where these variations are not too great, in the upwards movements a decrease, of the compared curves in comparison with the normal ones, in the downwards ones, on the other hand, an increase, occurs, there is no purpose to be fulfilled. My experiments show, nevertheless, that the size of this increase and decrease is very much dependent on the observer. This can arise from the fact that the starting points of the normal curves bore different relations to the horizontal plane for the different observers. The normal starting points for observers Kp., K., R. and S. were respectively -40 , -6 , -10 and -4 .

c. What kind of symmetrical results are given? There occurs a general tendency for the different compared curves to be made greater in comparison with their normal curves where the right hand is the normal than where the left hand is the same. This tendency varies with the observer and also with the height of the movement. The average of the average differences of the different series is reckoned out for each group. This average of a left hand group minus that of a right hand group gives the individual differences, which show the tendency above described. In groups *d* and *l* of observer K. this is very distinct. Here the left hand average is -3.05 cm., that of the right $+3.23$. The difference is 6.28 cm.; also in groups *c* and *m* of observer R. and in groups *d* and *l* of observer S., this tendency is clear. In the former two the average of the left hand motions is $-.50$ cm. and that of the right hand $+3.93$ cm., a difference of 4.13 cm. In the last two series the average of the left hand motions is $-.19$, that of the right hand $+4.11$, a difference of 4.30 cm. In the upwards movements in groups *a* and *g* of observer Kp., this tendency is not so distinct. The average of the left hand movements is -2.63 , that of the right hand -1.21 cm., a difference of 1.42 . In general, however, it is present especially at the beginning and end of the group. It is possible that the large variations in the lengths of the normal curves will explain this somewhat. In group *a* the limits between the largest and smallest

normal curves are roughly 13 and 22 cm. In group g they are 22 and 37 cm. In groups c and h of observer S. the general tendency is also present. The average of the average differences of the left hand movements is -1.17 , that of the right $+0.515$ cm., a difference of 1.68 cm. In group b of observer R., we find no variation from the tendency in so far as the compared curves are less in extent than the same in the downwards group, which were right handed. The average of the former is $+2.39$ cm., that of group b $+3.93$ cm., and the difference is 1.54 cm. We have no corresponding group of the left hand upwards movements in order to compare it with group b.

The above mentioned figures of the M. R. V. show that from the standpoint of the relation of an increase in the individual normal curves, when one considers the series separately, to the making of their compared curves, a distinct asymmetrical phenomenon is present. Where the left hand makes the normal movements the compared curves decrease with increase in their normal curves more than where the right hand makes them. The M. R. V. yields values for the upwards movements of O. Kp., where they are left handed, of 36.5, and where they are right handed, 38.4. For O. S. the corresponding values are 38.4 and 45. Where the movements are in the downwards direction, the corresponding values for O. K. are 55 and 50. For O. R. the corresponding values are 42.7 and 59.5, and for O. S. 47.5 and 56.3. The single exception to this asymmetrical difference consists in O. K.'s case, where it is of the opposite nature.

Another asymmetrical difference is found in the averages of the average normal curves for the right and left hand groups along with the variations of these. While there was no strictness as to the making of the average normal curves in the series for the groups equal for the right and left handed experiments, still the variations in these show certain degrees of difference sensibility. The averages of the average normal curves in the downwards movements are for the observers R. K. and S., for the left hand, 15.00 cm., 27.17 and 12.42, and for the right hand, 11.14, 21.23 and 11.14 cm. respectively. It results, then, that the average of the average normal curves of each left hand group is greater than the same of each corresponding right hand one. Further, the averages of the average differences as we have mentioned above are similar to these. With each observer the average of the average differences of all the series of a right hand group is 4 to 6 cm. smaller than the same in a right hand group. This is so significant that an average undervaluation takes place where the groups are left handed, while an average overvaluation takes place in the right handed ones. The M. R. V. has shown that the compared curves in comparison with their normal curves decrease with increase in these last.

These three asymmetrical tendencies work together. The variations of the normal curves when a whole group comes into consideration give the decrease obtained by the M. R. V. an importance. If, *e. g.*, each increase in the normal curves of 1 cm. produced a difference of only $+0.04$ cm. or $+0.05$ in the difference between a normal and a compared curve, this could produce in 75 to 100 such, as occurs in a single group, a difference of 2.00 cm. to 3.00 cm. In some of the groups the average of the variations is 2 cm., and in all it is not less than 1.00. The differences between the limits of the largest and smallest normal curves of the groups themselves lie between 2.53 and 11.00 cm. There is here plenty of room for the decrease, as shown by the M. R. V. This decrease explains to some extent the undervaluation in the left hand groups and the overvaluation in the right hand. In the two downwards

groups of O. R. the averages decrease with raising of the starting points in the left hand movements. In right hand ones they increase. It can then scarcely be doubted that this difference between the average differences in the left and right hand groups is dependent on the corresponding results obtained by the decrease and increase in the values obtained by the M. R. V. With the other observers this difference is not so easy to explain in this way. We have referred to this above when we considered the influence of the changes in the starting points and in the normal curves upon the difference sensibility. It results, then, that in the downwards motions these three asymmetrical phenomena, namely, the average overvaluation of the right hand groups and undervaluation of the left hand, the greater extent of the average normal curves of the left hand than that of those of right hand, and the smaller values obtained by the M. R. V. in the left hand movements than in the right hand ones, accompany one another. Where the movements are in the upwards direction, the average of the average normal curves of the left hand group of O. Kp. is smaller than the same average of the right hand group, while by O. S. it is inverse. Here the average of the average normal curves of the left hand group is greater than the same of the right hand group, as we have found above with the downwards movements. In these upwards movements the difference between the average differences of the right and left hand groups is not at all so great as in the downwards ones. M. R. V. shows that a much greater decrease in the individual compared curves in comparison with their normal ones takes place in the right handed upwards movements than in the left handed.

d. The variations. I have so far considered the simple average difference without respect to the average variation, because the variations of the normal curves produce an effect upon the average variations of the different series of the groups. The average of all the average variations of the normal curves of the different series of O. Kp. is 1.991 cm. and of the compared curves 1.643 cm. The difference between the two is .348 cm. In the series of all the other observers the corresponding averages are smaller for the normal curves than for the compared. We call O. Kp.'s difference +.348 cm. The corresponding difference with O. K., is -.510 cm. The average of all the variations of his normal curves is 1.47 cm. and of his compared ones 1.98 cm. The corresponding averages of O. R. are 1.263 cm., and 1.47 producing a difference of -.206 cm. The corresponding averages of O. S. are 1.08 cm. and 1.25 cm. and the difference is -.17 cm. It appears now that the compared curves do not vary very much from the normal. What effect now do these variations of the normal and compared curves produce on the average variations of the series? Or of what importance are the latter? The average variation spoken of here was reckoned out as follows: The individual difference of the ten experiments give us +, - and 0 values. All the plus subtracted from all the minus give us an average difference, which can be +, - or 0. This average difference is now subtracted from the individual differences. The values so obtained are half plus and half minus when added together, and the average obtained by dividing by ten is the average variation of the series. The average of all these for O. Kp. is 1.84 cm.; for O. K., 2.04 cm.; for O. R., 1.363 cm. and for O. S., 1.38 cm. The average of the normal curves lies between 10 and 30 cm. In any case, then, these average variations are not very large. I have not reckoned out the differences between the average variations and the average differences in order to obtain from this the raw values for the sen-

sibility, because these variations are possibly too much influenced through the variations in the normal curves. Still I think that by further investigation these variations in connection with the average differences would give better values for the sensibility than the simple average differences. From the investigation, we have so far obtained no exact sensibility values for the different starting points. I think that the sensibility is very variable with the different observers,¹ so that one cannot obtain exact values for the difference sensibility, as has already been done in the other fields of psychology.

e. Influence of the metronome. In all these experiments the metronome beat sixty times in the minute. The duration of each motion could, then, equal one second. Where the two motions of each experiment were made together at the same time, they were made with each beat of the metronome. In case the compared motion followed the normal one, they were made so that the former followed the latter on the next beat, by O. K. and O. S., or the fourth beat, as by O. Kp. I find the great differences between the results of the variations of the normal and compared curves, which exist in the figures of O. Kp. and the other observers, to arise in this difference in the beating of the metronome. As above shown the average of all the variations of the normal curves is greater than the same of the compared ones by +.24 cm. In the results of the other observers it is smaller by +.29 cm. If the sensibility for space valuations from the standpoint of the different variations is very good, then the variations of the normal and compared curves would be equal. If the movements are made at the same time or immediately after one another, the variations of the normal and compared curves do not change. If the latter, however, accompany the fourth beat of the metronome, then their variations will be smaller because they are more influenced by the metronome, and the normal curves would also serve to give a better sensibility. The above described figures show this.

f. Influence of the weights. I have made forty-one series of experiments in order to estimate the influence of the weights upon the valuations of the motions. Twenty-three were made by O. S., twelve by O. R. and six by O. Kp. In the first twenty-three a weight of 1.31 kg. was fastened to the hand, which made the compared motions, and the starting points were varied, as in the above described experiments. Four groups of these are described on the diagrams X, R, j and K. The weight remained constant in these. In group x there is no great deviation from the ordinary group c as the normal lengths increase. The great deviation in group R is dependent on the time relations. In group R the compared motions were made at the same time as the normal ones. In group x they followed them. Group K agrees in general with group h. The curve for k stands only a little lower. Where the motions were downwards, group j gives a curve higher than group n. The results of groups R and j in comparison with group c agree with the results, which show the differences in the lengths of the normal curves. The groups 1, 2, 3, 4, 5 and 6, which are described to the left on diagram 2, were made by O. Kp. and O. R. with the left hand as the normal, and with weights of 0.5 kg., 1.31 kg. and 3 kgs. They were in the upwards direction. It is worthy of remark that all these groups of O. Kp. appear higher on the diagrams than his group a, which is to be taken into comparison with it. In groups 1 and 2 one observes an increase in the compared curves with the increase in the weight.

¹ "Der Muskelsinn der Blinden." Paul Hochelsen, Berlin.

In group 3 this is not continued. Groups 4, 5 and 6 of O. R. also show an increase in the lengths of the compared curves with the increase in the size of the weights, if one takes into consideration the increase in the lengths of the normal curves. These 3 groups appear lower (on the diagram) than group b of O. R. The groups 7, 8 and 9, diagram 3 of the downwards movements of O. R., with starting points of -2 cm., show an unsteady increase of the average differences, if the increase in the averages of the normal curves is considered. This increase is more doubtful in the groups where the points are -10 cm. In groups x and R of O. S., in groups j, 7, 8 and 9 of O. S. and O. R., respectively, and in groups 1, 2, 3, 4, 5 and 6 of O. Kp. and O. R., respectively, the average differences are greater where the motions were successive than those of the simultaneously made movements.

Results of the Pencil Method, Two-Hand Motions.

1. The results obtained by the M. R. V. show that where the normal and compared motions are considered in each series separately, an increase in the former is accompanied by a comparative decrease in the latter if the direction is upwards, if it is downwards by an increase. In both cases the starting points are above the horizontal plane, and the above results are more distinct the higher these points are raised.

2. With the raising of the starting points of the compared curves above the horizontal plane, the average differences increase in downwards movements, and decrease in upwards according to the observer and heights of the normal curves.

3. The unsteadiness as shown by comparing the average normal curves with one another by the different observers, makes it impossible to obtain the proper values for estimating the absolute or relative difference sensibility of these two-handed movements.

4. The movements where the right hand is the normal, exhibit an average overvaluation, and inversely where the left hand is the normal.

5. Where the left hand is the normal the average normal curves with equal effort of the will are greater than where the right hand is the normal.

6. The values obtained by the M. R. V. are, for left handed movements, less than those for right-handed.

The experiments with the weights show:—

7. Firstly, the average differences where a weight is used are larger than where the movements are made without such, in the case of downwards motions, and in the case of upwards motions smaller, apart from groups 1, 2 and 3 of O. K.

8. Secondly, the average differences of the successive movements are larger than those of the simultaneous.

9. Thirdly, the greater the weights are, the more the average differences decrease in upwards movements, and in downwards the reverse.

These experiments with the weights are, however, not sufficiently numerous, for the different observers, in order to lay much value on them.

II.

EXPERIMENTS BY THE ANGLE METHOD, USING THE METHOD OF JUST PERCEPTIBLE CHANGE.

General Description.

These experiments were made with Dr. Külpe, K.; Mr. Child, O.; Dr. Cohn, Ch.; Mr. Rogers, R., and the investigator himself, S., as observers. Eight series were obtained from O. K., four from O. C.,

four from O. Ch., two from O. R., and one from S. Some of the experiments of O. R. were intended to determine differences between valuations of active and passive movements. Those of the other observers were active motions with the horizontal plane as starting points as well as end points with motions whose starting points were above and below this. The rates were fixed by the beating of a metronome forty and one hundred and twenty times in the minute. The results obtained, where observer K. made the valuations, were in all the above directions and at all the different rates. The diagram¹ given shows the investigated extents of movement and the directions of the same. A is the centre of motion for the arm, BA is the horizontal plane, in which B is the starting point for downwards movements under this plane, and for upwards above it, and inversely the end point for upwards movements under, and for downwards movements above this. Downwards movements were made under and above this plane with normal distances of 15° , 30° and 60° , and with both rates. A few series of these motions were also made with 10° as normal extent under the 0 point (horizontal plane). The experiments of O. C. include the above described motions which lie under the 0 point, those of O. Ch. such as lie above it. I consider the experiments in the following order: those of each observer separately, those under the 0 point and afterwards those above it; at first the downwards experiments under the 0 point, then the upwards; the upwards above the same next, and lastly the downwards above it.

The two chief rates yield eight groups¹ of series for O. K., four for O. C. and four for O. Ch. In the tables the symbols² represent the following values: " r " is equal to the normal stimulus, r_0 is the mean of the just greater than and the valuation of the extent above the same and just equal to it. " r_n " is equal to the mean of the stimulus just smaller than the normal, and the valuation of the extent below the normal just equal to it. R is the mean of r_0 and r_n . Δr_0 is equal to $r_0 - r$, Δr_n is equal to $r - r_n$. Δr is equal to the mean of Δr_0 and Δr_n , while Δ is equal to $R - r$. Vr_0 is the variation of the parts of r_0 , and Vr_n that of those of r_n . Zr_0 is the number of the parts of r_0 , Zr of those of r_n . R shows the relative over

and undervaluation. $\frac{\Delta r}{r}$ shows the approximate relative sensibility.

a. Under the Horizontal Plane.

Observer K. Group I. of O. K. consists of four series of downwards motions of extents of 10° , 15° and 30° under the horizontal plane, with a rate of forty beats of the metronome in the minute, and of one series of 60° at a rate of 120 beats a minute. We consider the first four series first. The variations in these vary between .37° and 1.00°. At 10° and 60° they are greater, while at 15° and 30° they are nearly alike. The valuating is, then, steadier at 15° and 30° than at 10° and 60° . The thresholds for difference sensibility, as shown by the values for Δr , which are .42°, .70° and .59°, increase from 15° to 30° and then decrease. This shows that the absolute difference sensibility does not increase regularly. The relative difference sensibility increases with the increase in the normal lengths, more between 10° and 15° and 30° and 60° than between 15° and 30° . At 15° , 30° and 60° the valuation is nearly correct. At 10° it is +.50°; at 15° , +.04°; and at

¹ See original.

² See Wundt's *Gründz. d. Phys. Psych.*

30° and 60°, —.05 and —.03°. The variations of all the series considered here of 10° normal extent can only be explained by the fact that the changes in the gradations of the compared extents was 1° instead of 4°. Still another series of 60° normal extent at the rate of twenty beats a minute shows an approximately correct valuation, namely, an undervaluation of .09°. The variations are greater than those at the more rapid rate. The threshold is .40°. The difference sensibility obtained by $\frac{\Delta r}{r}$ is finer than that of those of 60° at the more rapid rate.

Group II., of observer K., shows valuations of downwards movements at 10°, 15°, 30° and 60° under the horizontal plane at a rate of 120 beats in the minute. The variations of these vary for 10° and 30° between .68° and 1.25°, while for 15° and 60° they are respectively .30° and .63°. The individual valuations of these series are not so numerous as are those of the first. At 10° and 60° there is a large undervaluation, respectively .62° and .81°. At 15° one finds an undervaluation, of .12°, and at 30° an overvaluation of .19°. The thresholds, and along with them the absolute difference sensibility, increase with the increase in the normal extent. They are respectively for 15°, 30° and 60°, .41°, 1.05° and 1.31°. For 10° it is .87°. The relative difference sensibility is finest for 60°, less at 15°, still less at 30°, and still less again at 10°. From the standpoint of both thresholds and over and undervaluations, as also of the relative difference sensibility, the slower rate of motion is best in making downwards movements.

Group III. of O. K. was made in the upwards direction towards the horizontal plane at angles of 15°, 30° and 60°, and at rates of sixty beats of the metronome in the minute. The variations are, at 30° and 60°, more equal to one another than at 15°. Therefore, the steadiness is greater at 10° than at 30° and 60°. For angles of 10° and 60° overvaluations take place, and for 30° there is an undervaluation. The thresholds for 15°, 30° and 60° are respectively .34°, .48° and .65°. These signify an increase in the absolute difference sensibility with increase in the normal extents. The relative difference sensibility increases with the increase in the lengths of the normal angles (or curves, as they are seen on the table). The very small thresholds of this group are worthy of remark.

Group IV. of O. K. consists of upwards movements towards the horizontal plane at angles of 15°, 30° and 60°, and at a rate of 120 beats of the metronome in the minute. On the whole, undervaluations are manifest. These valuations at 15° and 60°, taking that at 30° as a standard, are the inverse of those in group III. In the last the angles at 30° are more overvalued than at 30° and 60°, while in group IV. the reverse is true. The beating of the metronome is the cause of this, which we shall consider later. At 30° the valuation is nearly correct, namely, —.07°. At 15° and 60° they are —.06° and —.13°. The thresholds are for 15°, 30° and 60°, respectively, .94°, .72° and 1.25°. They are also much greater than at rates of forty beats in the minute. Hence the absolute difference sensibility increases regularly with the increase in the normal angles. On the other hand, the relative difference sensibility also increases with the same increase. The variation is at 30° smallest, at 60° larger, and at 15° greatest. They vary between 0.00° and .78°. We find now that the time relations have more influence upon the valuations than the directions. In the case of O. C. we will find that the direction exercises the greater influences.

Observer C. Group I. consists of downwards motions at 10°, 15°, 30° and 60° under the horizontal plane at a rate of forty beats of

the metronome in the minute. Vr_o and Vr_n vary between $.50^\circ$ and $.90^\circ$. At 10° and 30° they are smallest, and at 15° and 60° largest. From the standpoint of the variations, it is to be assumed that the difference sensibility is smaller at 15° and 60° than at 10° and 30° . The threshold values (Δr) are for 10° , 15° , 30° and 60° , respectively, $.87^\circ$, $.76^\circ$, $.87^\circ$ and 1.16° . From 15° to 60° , then, the absolute difference sensibility decreases. At 10° it is the same as at 30° . R and Δ show an approximately equal valuation from the standpoint of the average valuation. At 10° there is a correct valuation. From 15° on the small undervaluation decreases up to 60° , where there is an

overvaluation. $\frac{\Delta r}{r}$ shows the relative difference sensibility with increase in the normal angles to increase distinctly.

Group II. consists of downwards movements at 10° , 15° , 30° and 60° under the horizontal plane at a rate of 120 beats of the metronome in the minute. The variations in the values are smallest, namely, $.50^\circ$ and $.76^\circ$ at 10° and 30° , and largest at 15° and 60° , namely, 1.03° and 1.15° . At 10° there is a combined overvaluation of $.25^\circ$, which increases to $.50^\circ$ at 15° , and at 30° again to $.315^\circ$. The thresholds of 10° and 30° agree again with one another in this group. They are smaller than at 15° and 60° . At the first they are $.75^\circ$ and 1.09° , and at the last 1.25° and 1.37° . The absolute difference

sensibility is, then, smaller at the first than at the last. $\frac{\Delta r}{r}$ shows that the relative difference sensibility decreases from 10° to 15° and increases from 15° to 60° . The difference between the relative sensibilities at 15° and 30° is greater than that between 30° and 60° . It results, then, that a slow rate of movement is better in valuing downwards movements than a more rapid rate.

Group III. consists of downwards movements at angles of 15° , 30° and 60° below, but towards the horizontal plane, at a rate of forty beats of the metronome in the minute. The number of individual valuations is, in the experiments that follow, less than formerly. At 15° there is an overvaluation of $.26^\circ$, at 30° and 60° undervaluations of $.21^\circ$ and $.13^\circ$. The thresholds increase gradually from 15° on. They are respectively $.92^\circ$, 1.29° and 1.94° somewhat greater than in the downwards movements. The absolute difference sensibility

decreases, then, with the increase in the normal extents. $\frac{\Delta r}{r}$ shows that the relative difference sensibility rather decreases between 15° and 30° and increases between 30° and 60° . The variations remain more constant in these groups than in the downwards movements.

Group IV. consists of downwards movements at angles of 15° , 30° and 60° , as in group III., at a rate of 120 beats in the minute. At 15° and 60° there are undervaluations of respectively $.32^\circ$ and $.04^\circ$. At 30° an overvaluation of $.34^\circ$ is shown. The variations are also greater at 15° and 60° than at 30° . On this account there is a greater absolute difference sensibility at 30° than at 15° and 60° from the standpoint of the variations. The thresholds increase in size from 15° on. They are $.62^\circ$, 1.34° and 2.34° respectively for 15° , 30° and 60° . Groups III. and IV. show that the downwards movements are made more exactly from the standpoint of these than are

the upwards. The relative difference sensibility obtained from $\frac{\Delta r}{r}$ does not agree with that obtained from the thresholds. At 60° it is finest, at 15° finer, and at 30° and 60° it is smallest.

One series was also made in the downwards direction by O. R. at 15° and at a rate of forty beats of the metronome in the minute.

The valuation was very nearly correct, namely, an overvaluation of $.04^\circ$. The accompanying variations were also small, $.16^\circ$ and $.22^\circ$. The threshold agreed with that of the upwards movements. It was 1.52° . The small variations of the series of O. R. show that they are very valuable, although the thresholds are large.

I add here a series by O. S., which was made in the downwards direction below the horizontal point at angles of 30° and a rate of forty beats of the metronome a minute. An overvaluation of $.37^\circ$ took place, while the variations were $.75^\circ$ and $.74^\circ$. The threshold and the relative difference sensibility agree with the experiments of O. C. and O. K. There is more of an overvaluation in these experiments than in the others.

b. Above the Horizontal Plane.

Observer K. The groups V., VI., VII. and VIII. of O. K. follow. They were made in the upwards and downwards directions above the horizontal plane at angles of 15° , 30° and 60° and at rates of forty and one hundred and twenty beats of the metronome in the minute.

Group V. consists of upwards movements at angles of 15° , 30° and 60° at rates of forty beats in the minute. The variations increase with the increase in the normal angles. Δ shows at 15° an overvaluation of $.12^\circ$ at 30° , an undervaluation of $.16^\circ$, and at 60° one of $.26^\circ$. The thresholds gradually increase with the increase in the normal angles. They are $.37^\circ$, $.59^\circ$ and 1.12° . Hence the absolute difference sensibility decreases with the increase in the normal

angle. $\frac{\Delta r}{r}$ shows a greater increase of the relative difference sensibility between 15° and 30° than between 30° and 60° .

Group VI. was similarly made to group V., apart from its more rapid rate of movement, which was 120 beats instead of forty in the minute. The variations increase with the increase in the normal angles. The valuations are the reverse of those in group V., in so far as the compared movements in comparison with the normal ones increase with the increase in these last. There is an undervaluation of $.07^\circ$ at 15° , one of $.15^\circ$ at 30° and of $.56^\circ$ at 60° . At 30° it is smaller than at 15° and 60° . The thresholds are: $.56^\circ$, $.40^\circ$ and 1.19° , respectively, at 15° , 30° and 60° . At 30° it is somewhat smaller than at 15° and 60° . The absolute difference sensibility is smaller at the very large angles, greater at the medium ones and somewhat less at 60° than at 30° . The above mentioned differences between the valuation in series V. and VI. can only be explained through the change in the rate of movement.

Group VII. of O. K. consists of downwards movements from above towards the horizontal plane at angles of 15° , 30° and 60° and at rates of forty beats of the metronome in the minute. The variations increase here also with the increase in the normal angles. They vary between $.16^\circ$ and $.75^\circ$. The valuations are related to one another similarly as in group V. At 15° there is an overvaluation valuation. The compared movements decrease, then, in comparison of $.65^\circ$ cm., at 30° an undervaluation of $.10^\circ$ and at 60° a correct with their normal ones with increase in these last. The thresholds are: $.37^\circ$, $.53^\circ$ and $.75^\circ$ for 15° , 30° and 60° respectively. Hence the absolute difference sensibility decreases with increase in the normal angles. The relative difference sensibility increases more between 15° and 30° than between 30° and 60° .

Group VIII. of O. K. was different to group VII. in the rate of the metronome, which was 120 beats instead of forty, as in the latter. The variations increase here also with the increase in the normal angles. The valuations agree with those in group VI. At 15° and 30° , there are undervaluations of $.04^\circ$ and 1.19° , and at 60° an overvaluation of $.27^\circ$. In connection with the increase of compared angles as compared with that of normal ones, groups V. and VI. agree with groups VII. and VIII. This agreement corresponds with the variations in the rate of movement. The thresholds show here also an increase in the absolute difference sensibility with increase in the normal angles. They are respectively $.40^\circ$, $.56^\circ$ and 1.09° for angles 15° , 30° and 60° . The relative difference sensibility increases gradually with the increase in the normal angles. Groups VII. and VIII. show that the valuation is more exact at the rates of forty beats of the metronome than at the rates of 120, and this from three standpoints, namely, that of the valuation as determined by Δ , that of the absolute difference sensibility, and that of the relative difference sensibility determined by $\frac{\Delta r}{r}$.

Observer Ch. Groups I., II., III. and IV. of O. Ch. were carried out similarly to groups VI., VII., VIII. and IX. of O. K. Group I. consists of upwards movements, etc. The variations are in this group especially large compared with the same in the groups so far considered. They vary between 1.02° and 2.66° , and are much larger at 60° than at 15° and 30° . The valuations are undervaluations. At 15° and 30° , they are $.43^\circ$ and $.09^\circ$, the latter an approximately correct valuation. The compared angles at 15° and 60° are more undervalued in comparison with their normal angles than at 30° . The threshold at 30° is smaller than at 60° , and at 60° smaller than at 15° . The absolute difference sensibility follows this order also. The thresholds are for 15° , 30° and 60° , respectively, 1.14° , $.76^\circ$ and $.85^\circ$. The relative difference sensibility increases more between 15° and 30° than between 30° and 60° .

Group II. of O. Ch. varies from group I. only in so far as the rate is 120 instead of forty beats per minute. The variations vary between $.44^\circ$ and $.75^\circ$, and are somewhat greater at 15° and 30° than at 60° . The valuations are at 15° and 60° overvaluations, namely, $.26^\circ$ and 1.35° , respectively. At 30° there is an undervaluation of $.18^\circ$. The relation of changes of the compared angles to their normal ones is the reverse of what they were in group I., where the normal angles vary. The thresholds are greater than in all the groups considered above. They are for 15° , 30° and 60° , respectively, 1.48° , 1.71° and 3.15° . Hence the absolute difference sensibility decreases with the increase in the normal angles. The relative difference sensibility increases much more between 15° and 30° than between 30° and 60° .

Group III. of O. Ch. consists of downwards movements at angles of 15° , 30° and 60° at a rate of forty beats of the metronome in the minute. The variations follow in their size those of the normal series. At 30° there is an overvaluation of $.06^\circ$. At 15° and 60° there is an undervaluation of $.07^\circ$, and an overvaluation of 1.25° respectively. The valuations at 15° and 30° are nearly correct valuations. The thresholds do not vary relatively as do the normal angles. At 15° , 30° and 60° they are respectively 1.00° , $.93^\circ$ and 1.47° , while the absolute difference sensibility is smaller at 60° than at 15° . This last is somewhat less than at 30° . On the contrary, the relative difference sensibility is finer at 60° than at 30° , and finer at 30° than at 15° . The difference between these two is, however, nineteen times greater than that between the first two.

Group IV. has normal angles of 15° , 30° and 60° . The movements were quite similar to those in the above group apart from a change in the rate of forty beats of the metronome to that of 120. Δ shows an overvaluation of .21° and .37° at 15° and 60° , and an undervaluation of .19° at 30° . Δr gives thresholds of 1.20°, 1.00° and 3.00° at 15° , 30° and 60° , and $\frac{\Delta r}{r}$ shows a higher relative difference sensibility at 30° than at 15° and 60° . The relative difference sensibility is finer at 30° in group III. than in group IV.

Valuations of Passive as Compared with Active Movements.

With O. R. passive movements, as well as active, for the purpose of comparing them with the active, were made in the upwards direction under but towards the horizontal plane. The normal angles were 15° and the rates forty (group I.) and 120 (group II.) beats of the metronome in the minute. The variations of both groups are not large. In group I. they vary between .32° and .44°. In group II. between .29° and .69° Δ shows that the valuations on the whole are finer with the passive than with the active movements. In the first there was an overvaluation of only .05° for group I. and of .03° for group II., while for the last there were undervaluations of .23° and .36°. The thresholds in group II. are nearly equal for the active and passive movements, namely, 1.48° and 1.41°. Those in group I. are not nearly so much like one another, since the values are 1.40° and 1.85°. The absolute difference sensibility is finer at the slower rate for the active than for the passive series, and for the quicker rate than for the opposite. The constant size of the thresholds with O. R. are probably dependent on the want of practice, or on his want of sensibility for motion sensations. The relative difference sensibility yields like results to those of the absolute. While now we have so far described the differences between active and passive motions, yet we can now leave them aside, as they are so limited. They serve only to show influences of the rate of movement.

The Relations of Changes in the Rate of Movement.

So far we have considered these experiments on the whole without respect to changes in the rates, which arise within the groups themselves. All the possible rates to be found are as follows: 15° in 1.50 sec. = 1° in .10 sec., 15° in .50 sec. = 1° in .033 sec., 30° in 1.50 sec. = 1° in .05 sec., 30° in .50 sec. = 1° in .016 sec., 60° in 1.50 sec. = 1° in .025 sec., 60° in .50 sec. = 1° in .008 sec., 60° in 3.00 sec. = 1° in .05 sec. If the metronome beats forty times in the minute, each interval is equal to 1.50 sec.; if 120, .50 sec., and if twenty times, 3.00 sec. Applying these figures to the groups we have described, we find that in each series the rate increases at 15° , 30° and 60° in a geometrical progression of one-half. One group of motions with 15° , 30° and 60° as normal angles and a rate of forty beats in the minute, implies relative rates of for 15° , 30° and 60° , 1° in .100 sec., 1° in .05 sec. and 1° in .025. Now these increase in the relation of a geometrical progression of one-half to one another. Where the metronome beats 120 times in the minute, the corresponding relative rates are 1° in .03 sec., 1° in .016 sec., 1° in .008 sec. These stand to one another in the relation of a geometrical progression of one-half. I presuppose here that if the metronome beats forty times a minute, the movements last the length of time between the two beats. If this is not exactly so, still the variations from this at

15°, 30° and 60° is nearly alike in each case and very small, and moreover these relative rates show the exact duration of the movements.

On the Relation between Stimuli and Difference Sensibility.

The sizes of the normal angles themselves bear to one another the relation of a geometrical progression of one-half, and the rates also bear the same relation to one another. This is true, also, although the duration of movements for the different normal angles remains the same. If we consider the size of these angles, with the rates of movement as stimuli, then we have in each group, considered above, three stimuli, which bear to one another the relations of parts of a geometrical progression. Now, Weber's law says that in order to let the rate of movement increase in an arithmetical progression, the stimuli must increase in a geometrical progression. Of the eight groups of O. K. there are only four where this law holds approximately, namely, in groups III., IV., VII. and VIII. In all the groups the direction is towards the horizontal plane; III. and IV. upwards, from below, towards the horizontal plane; VII. and VIII. downwards from above, towards the same. As measure of the

sensibility I have taken $\frac{\Delta r}{r}$. When one reckons out this sensibility value to the tens of thousands, it is sufficiently approximate without reckoning out $\frac{\Delta r_0 + \Delta r_x}{2}$. The values obtained by this last vary only five to ten thousand parts from those obtained by the $\frac{\Delta r}{r}$. In group III., O. K., the difference sensibility for 15°, 30° and 60° respectively, is represented by 226, 160 and 108. The differences are, therefore, sufficiently approximate to an arithmetical mean of 60. In group IV. the arithmetical mean is about 57, in group VII. about 60, in group VIII. about 65. In these four groups Weber's law appears to hold. In the other four groups of O. K. the changes in sensibility do not follow any distinct law. In group I. the sensibility at 60° is nearly three times finer than at 15°, while the difference between that at 30° and 60° is nearly four times greater than that of the same between 15° and 30°. This difference might easily come about through habitual movements in this direction and practice. In group II. the sensibility is in general not great. That it is greater at 15° than at 30° can be explained through habit. In group V. the small sensibility at 60° prevents Weber's law from being applicable to the results. These motions at 60° are uncusomary. In group VI. the difference sensibility is finer at 30° than at 15° and 60°. I see no explanation of the variations in the sensibility in this group. In the ordinary movements which one makes, the direction is towards the horizontal plane. Goldscheider¹ has shown that sensations of locality exist by which one can recognize the place where the arm is held. In the ordinary movements towards the horizontal plane, these sensations of locality are better developed. The more regular sensibility for different distances in motion towards this plane can be influenced through this. Only in group III. of O. C. is there any approximate application of Weber's law. The difference between the values for sensibility at 15° and 30° is 183, between those at 30° and 60°, 107, an approximate arithmetical mean of 145. In group IV. the sensibility is throughout not fine. That at 15° appears, however, to be finer than that of the same

¹ "Ueber den Muskelsinn." Du Bois-Reymond's Archiv, 1889 and Appendix.

observer for other directions and rates of movement. The very small sensibility which O. C. shows in comparison with O. K. throughout is worthy of attention. The differences between the sensibilities at 15° and 30° and 30° and 60° , in groups I. and II. of O. C., are peculiar, in so far as in both cases the difference between the first two is much finer than that between the last two. Between 30° and 60° , in group I., the sensibility increases 2.29 times more than between 15° and 30° , and in group II. 3.48, times. With O. Ch. Weber's law does not hold at all. In group I. the difference between 15° and 30° is 4.5 greater than that between 30° and 60° , and in group II. nine times. It appears in groups I. and II. of O. C. and O. Ch., that while Weber's law does not hold, a law of another kind holds. This appears to be dependent on the rate of movement in so far as the tripling of the rate of movement doubles the relation of the differences in the sensibility of either of the observers. In the case of the first observer 9 is twice as great as 4.5, and in that of the last, 3.48 is 14 times as great as 2.26. This doubling of the relations of the differences to one another is accompanied by a triple increase in the rates of movement for both observers. For 15° , 30° and 60° the rates are respectively—in group I., 1° in .10 sec., 1° in .05 sec. and 1° in .025 sec., and in group II., 1° in .033 sec., 1° in .016 sec. and 1° in .008 sec. The groups of O. K., where the deviations from Weber's law are found, namely, I., II., IV. and VI., correspond to these groups. The differences in these groups agree, however, only partly with the results of O. C. and O. Ch. In group III. 4 is twice as great as 2 in group I. (the relative values for the relation of the differences). Group VI., however, yields no value to be compared with 4 in group V. Series III. and IV. of O. Ch. allow of no application of Weber's law which the corresponding groups of O. K. show. The very fine difference sensibility at 30° , in comparison with that at 15° and 60° , explains this partly. It appears, then, that movements towards the horizontal plane, as, e. g., III., IV., VII. and VIII. of O. R., III. and IV. of O. C., and III. and IV. of O. Ch., with exceptions, follow Weber's law, while such as have a direction of movement away from the horizontal plane, as, e. g., groups I., II., V. and VI. of O. K., I. and II. of O. C. and O. Ch., give greater differences of difference sensibility between 15° and 30° and 30° and 60° than Weber's law presupposes. The peculiarities in the different kinds of movements, then, are dependent as well on the normal lengths as on the rates of movement, and these latter are probably influenced by the direction of movement.

The Relations of the Valuation to One Another.

The table given on page 402 shows the values for Δ , for the different directions and groups of O. K., O. C. and O. Ch. I. to IV. show it for motions which were made under the horizontal plane, and V. to VIII. for such as were above the same, a and b represent rates of respectively forty and 120 beats of the metronome in the minute. U and D show whether the movements were up or down. The symbols minus and plus, when they are enclosed in brackets, show whether there was an over or undervaluation. When the plus and minus stand outside of these, they show the relative valuations of the three normal angles, 15° , 30° and 60° , to one another. The values for the three normal angles are given from left to right for angles of 15° , 30° and 60° in each diagram. I., III., V. and VII. are upwards movements, II., IV., VI. and VIII. downwards.

TABLE III.

I.				II.			
	15°	30°	60°		15°	30°	60°
I. C.	a +(+.29)	—(-.31)	+(-.17)	II. C.)	a —(-.05)	—(-.04)	+(-.10)
	b —(-.32)	+(+.34)	—(-.04)		b +(+.50)	+(+.34)	—(-.11)
III. E.	a +(+.02)	—(-.27)	+(+.27)	IV. E.	a +(+.04)	—(-.05)	+(-.06)
	b —(-.06)	+(-.07)	—(-.11)		b —(-.11)	+(+.19)	—(-.01)
Ch.	a —(-.43)	+(-.09)	—(-.43)	VI. Ch.	a —(-.07)	+(+.06)	+(+.15)
	b +(+.26)	—(-.15)	+(+.35)		b +(+.31)	—(-.19)	+(+.05)
V. E.	a +(+.12)	—(-.16)	—(-.26)	VII. E.	a +(+.05)	—(-.11)	+(-.00)
	b —(-.07)	+(+.15)	+(+.56)		b +(+.04)	—(-.19)	+(+.07)

It results that the best valuation for downward movements was at the rate of forty beats a minute, namely, in groups a III., a IV., a VI. and a VII. There is almost a correct valuation by each observer for all the angles. A single exception to this is at 60°, by Q. Ch. The valuations of the other movements vary between an overvaluation of .56° and an undervaluation of .31°. In only b III. and a V. is there an undervaluation for all three normal angles. In all the other groups there are more or less overvaluations as well as undervaluations. In no group are there undervaluations alone. Apart from groups b III. and a V. it is, then, to be assumed that if movements had been made at the angles lying between the angles used as normals, with similar rates of movement (forty or 120 beats), correct valuations would have resulted. One finds, further, from the tables, with few exceptions, an overvaluation where the metronome beats forty times a minute, and inversely an undervaluation where it beats 120 a minute for normal angles of 15° and 30°. At 30° the reverse holds true. From this it is further to be assumed that if one investigated the angles between the normal angles with both rates of movement, several such normal angles between 15° and 60° would be found for which correct valuations would be given.

That at the two rates of movement for the same normal angles and movements, the relative valuations are the inverse to one another, is shown in the tables by the plus and minus symbols marked outside of the brackets. The relations of the over and undervaluations and time of the changes in the valuations are not the same in all the groups. In the first groups, namely, for motions under the horizontal plane, there is for angles 15° and 60°, and at the slow rates of movement, a relative overvaluation, and at 30° a relative undervaluation, at the quicker rates of movement for 15° and 60° inversely a relative undervaluation, and at 30° a relative overvaluation. Group II. shows some exceptions to this. The relative over- and undervaluation signify nothing else than that in one case the angles are more highly valued at 15° and 60° than at 30°; in the other case, less. Group II. shows that the smaller angles at the slower rate are less valued than the larger ones. When the rate is quicker the reverse holds. When the motions were made above the horizontal plane, the relations of the valuation change somewhat with the groups. Still the rates of movement show

their peculiar inverse changes in the valuations. In groups V. and VI. the angles are less valued than at 30° , where the slow rate of movement is used. Where the quick rate is used the reverse is true. In group VII., at the slow rate, the smaller angles are higher valued than are the larger, and at the quicker rate the opposite is true. Group VIII. shows that the angles at 15° and 60° are higher valued at the slow rate of movement than those at 30° , and at the quicker rate less valued at 15° and 60° than at 30° .

The Steadiness of the Motions.

The downwards movements appear on the whole to be valued with less certainty than the upwards. The average of all the variations of the groups of the latter for O. C. is $.62^\circ$, and the same of the downwards $.795^\circ$. The average of all the former of O. K. under the horizontal plane is $.4009^\circ$, the same of the latter $.684^\circ$. Above the horizontal plane the corresponding figures are $.35^\circ$ and $.40^\circ$. The groups of O. Ch. give an opposite result in so far as the average of the variations of the upwards motions is 1.14° and that of the downwards $.53^\circ$. It is worthy of remark that the upwards movements of this observer were less exact where the rate was slow than where it was quick, as the variations of the former ranged between 1.02° and 2.66° , and those of the latter between $.44^\circ$ and $.75^\circ$. In so far as the average of the variations was $.46^\circ$ at the slower rate and $.59$ at the quicker of the downwards movements, the reverse holds true in regard to them. This observer could only with difficulty execute the larger normal angles at the quicker rate for experimental purposes, and his motions appear more as reflexes than as regulated movements.

TABLE IV.

U.					D.				
		15°	30°	60°			15°	30°	60°
I. C.	a	613 +(+)	430 —(—)	323 +(—)	II. C.	a	512 —(—)	291 —(—)	194 +(+)
	b	416 —(—)	446 +(+)	393 —(—)		b	833 +(+)	365 +(+)	229 —(—)
		226 +(+)	160 —(—)	108 +(+)	IV. K.	a	280 +(+)	233 —(—)	99 +(—)
III. K.	a	226 +(+)	160 —(—)	108 +(+)		b	280 —(—)	351 +(+)	219 —(—)
	b	283 —(—)	240 +(—)	208 —(—)					
		760 —(—)	253 +(—)	141 —(—)	VI. Ch.	a	666 —(—)	145 +(+)	291 +(+)
V. Ch.	a	760 —(—)	253 +(—)	141 —(—)		b	806 +(+)	333 —(—)	500 +(+)
	b	986 +(+)	570 —(—)	525 +(+)					
		246 +(+)	196 —(—)	186 —(—)	VIII. K.	a	246 +(+)	176 —(—)	125 +(0.00)
VII. K.	a	246 +(+)	196 —(—)	186 —(—)		b	266 +(—)	186 —(—)	136 +(+)
	b	373 —(—)	733 +(+)	198 +(+)					

In tables similar to the above I have put in the values for the relative difference sensibility. In groups II., III., VI., VIII. and V., and for the most part also in groups I. and VII., it appears that the

movements are executed with finer difference sensibility at the slow rate than at the quick. In so far as the relatively smaller rate in any group accompanies the greater angles, it would be desirable to complete these groups so that all the normal angles would have the same rate of movement.

I have completed two other groups below the horizontal plane—one upwards, group II., and one downwards group I.,—in which the rate was kept relatively the same for 15° , 30° and 60° , namely 1° in .033 seconds. For this purpose the metronome beat at 15° 120 times a minute, at 30° sixty times and at 60° thirty times. The O. Hicks was at first somewhat exercised, and afterwards four series of upwards and four series of downwards movements in the order of experimentation were varied alternately with one another. Those for the purpose of exercise were in the downwards direction, and consisted in eight series at respectively 15° , 30° and 60° . These eight series I have reckoned out with the last four similar series, because both the four series and the combined twelve series show similar deviations from the four series of upwards movements.

A very distinct difference between the results of groups I. and II. is present, in so far as Δ shows that a general overvaluation of the upwards movements and undervaluation of the downwards take place. From this and from the other observers it is to be assumed that the direction of the valuation is dependent on the observer.

In group I. Δr and $\frac{\Delta r}{r}$ show that increasing the size of the normal angles causes the absolute difference sensibility to decrease and the relative to increase. Δ shows also that this increase also causes the valuation to be more correct. The variations varied between $.28^\circ$ and $.54^\circ$ and gradually increased with the increase in the normal angles. The difference between the relative difference sensibility, as shown by $\frac{\Delta r}{r}$ at 15° and 30° , is 1.5 times that shown at 30° and 60° .

Group II. of O. H. is the reverse of group I. in so far as Δr and $\frac{\Delta r}{r}$ show that increasing the angles decreases the absolute difference sensibility, but increases the relative. This holds for angles of 15° and 60° . At 30° , however, the absolute difference sensibility, as shown by $\frac{\Delta r}{r}$, is finer than at 60° . The values of Δ do not vary much from one another. The variations Vr_o and Vr_a vary between $.12^\circ$ and $.62^\circ$ and increase with the increase in the normal lengths. The relative difference sensibility is much smaller at 15° than at 30° and 60° . Both series I. and II. show, then, that the increase in the rates with increase in the normal angles produces no great influence upon the increases in the relative sensibility as shown by $\frac{\Delta r}{r}$.

Sense of Locality.

I have called attention to sensations of locality for the arm. O. C. has observed this further in so far as in his upwards movements the starting points instead of the end points were varied. He recognized the changing of the angles in a series by the change in the height to which he must raise or lower his arm in order to reach the starting points. One series of group VII. of O. K. was executed in which the starting point instead of the end point was varied. The valuation and the threshold values are about the same as in

the regularly executed series. From this it appears that the difference in the valuation is not large. It appears, nevertheless, that here an apparently fruitful field for investigation on the sense of locality¹ by the method of just perceptible change, is open. If a height is taken as normal starting point and the thresholds above and below determined, values of the difference sensibility in the sense of locality would be obtained. The values of the average of the heights of the starting points in my first experiments could also be very easily used here, in so far as they show the average variations of the individual valuations of the heights of any assumed starting point.

The results of these angle method experiments by the method of just perceptible change are:

1. The absolute difference sensibility as determined by Δr for 15° , 30° , 60° is greater at the slower rate of forty beats of the metronome a minute than at the quicker rate of 120 beats.

2. This absolute difference sensibility varies with the observers and with the size of the normal angles.

3. At 30° and 60° it is with few exceptions smaller than at 15° .

4. The relative difference sensibility as shown by $\frac{\Delta r}{r}$ is much finer at the larger normal angles. For motions which are directed towards the horizontal plane, Weber's law holds approximately. For such as are directed away from it, the increase in the relative difference sensibility with increase in the normal angles is much greater than shown by Weber's law.

5. By proper use of different rates it is possible for different observers to execute correctly, according to the values given by Δ , movements at all normal lengths.

6. The absolute difference sensibility as shown by Δr is finer at quicker rates of movement than that at slower, where the motions are upwards towards the horizontal plane.

7. The difference between the thresholds for motions above and under the horizontal plane is not large for the same observer.

8. Where changes in the size of the normal angles and in the rates take place, the changes in the first produce the greatest influence.

9. The steadiness in the valuations is dependent on the observer. In the case of most observers, valuations of downwards movements are less steady than are those of upwards.

¹ Goldscheider above cited.

TABLE I.—Downwards One-Handed Movements.

N. S.	N. E.	H. S. ³	H. V.	M. R. V.	N. L.	V. N. L.	M. D.	V. D.	R. L.	V. R. L.	M. D. L.	V. D. R.	O.
1	9	-53.95	1.80	30	15.97	2.15	1.05	2.95	46.81	2.86	4.50		S.
2	12	-50.00	50.—	32	10.02	.86	3.48						S.
3	14	-47.00	2.00		11.30	1.45	.307						S.
4	15	-46.00	84.—		16.95	3.48	4.03						K.
5	4	-42.50	2.00	0.00	12.93	2.33	5.87	.68	55.00	8.50	-2.50	13.50	S.
6	17	-32.65	1.68	31½	21.98	1.40	-4.83	2.36	60.77	11.75	6.03	14.56	S.
7	10	-09.00	3.94		18.67	3.68	1.47						K.
8 ¹	15	-00.00	+14		10.41	1.43	2.28						K.
9	14	+14.50	3.00		36.06	2.95	-.41						K.
10	5	+23.46	3.94		13.00	1.70	4.40						K.
11	71	+41.76	1.82	30	28.45	3.09	.77	3.38	67.50	6.54	-6.18	12.25	S.
12	9	+46.67	3.26	25	37.61	3.42	3.00						S.
13	8	+49.98	4.00	00	34.47	5.02	-1.63						S.
14	8	+56.07	4.81	46	16.62	2.22	2.28						W.
15	24	+58.59	1.20		24.82	2.67	8.85						K.
16	48	+63.00	4.00		59.98	9.74	3.45						S.
17	30	+63.50	4.01		27.97	11.67	2.86						K.
18	23	+70.00	2.00—		9.98	.91	.57						S.
19	12	+70.37	.85	59	12.41	1.40	4.14						S.
20	15	+72.00	+72	85½	6.75	.70	1.21						K.
21 ²	10	+50.00	-22		19.42	3.07	1.82						S.

¹Observer sat.²Made by shadow method.

N. S., number of the series. N. E., number of experiments in each series. H. S., average height of starting point for each series. H. V., average variation of same. M. R. V., as described above. N. L., average length of normal curves of each series. V. N. L., average variation of same. M. D., average difference between normal and compared curves of each series. V. D., average variation of same. R. L., average length of radii of normal curves. V. R. L., average variation of same. M. D. R., average difference between radii of normal and compared curves. V. D. R., average variation of same. O., observer.

TABLE II.—*Continued*

N. S.	N. E.	H. S. ¹	H. V.	M. R. V.	N. L.	V. N. L.	M. D.	V. D.	R. L.	V. R. L.	M. R. L.	V. D. R.	O.
1	9	-59.28	1.15	90	21.25	1.88	6.77	4.35	47.33	?	7.90	8.55	S.
2	18	-58.73	.91	52	19.83	4.35	8.29						S.
3	15	-55.00	38.72	38½	13.95	2.05	6.66						S.
4	15	-54.97	2.33	33	16.93	1.73	3.30						S.
5	12	-54.67	1.24		19.54	2.38	2.54	2.18	50.91	3.83	9.75	4.79	S.
6	10	-53.70	1.80	48	16.50	1.55	4.50	2.10	59.10	12.28	10.50	16.44	S.
7	15	-51.17	1.83	45½	35.45	4.68	9.80	3.46	57.66	5.39	2.83	6.20	S.
8	15	-48.34	1.37	39	14.15	1.92	3.91						S.
9	17	-41.92	2.39	55	41.96	6.28	12.10	7.46	68.58	6.32	4.94	8.19	S.
10	8	-36.63	3.62	18½	36.68	4.29	6.68						S.
11	15	-34.00	3.72		18.91	1.85	4.21						K.
12	11	-33.00	2.26		10.81	2.30	.51						K.
13	23	-32.20	1.70	93	29.73	1.69	13.91						S.
14	40	-23.59	3.04		14.10	1.53	2.93						S.
15	8	-22.25	4.31	43½	32.53	2.29	1.81						S.
16	4	-16.13	3.31	75	10.68	1.95	2.31						S.
17	12	-15.70	3.71	54	26.33	2.22	4.86	2.46	63.75	9.21	.87	10.45	S.
18	31	-12.10	4.25	45½	14.27	1.92	5.43						S.
19	15	-10.20	2.13		48.03	5.17	4.65						K.
20	22	-6.30	4.13		33.33	6.17	5.80						S.
21	14	-4.93	3.14	71½	20.78	1.69	10.18						S.
22	15	+14.25	2.68	30	23.75	1.42	4.89						S.
23	16	+15.60	3.19	51	19.54	3.53	5.31						S.
24	13	+17.35	2.12	21½	13.82	1.54	6.19						S.
25	15	+18.33	2.03	61½	14.78	2.47	4.33						K.
26	15	+19.24	2.51	30	15.58	2.10	3.89						S.
27	13	+31.79	1.80	63	15.15	1.69	5.10	2.43	37.54	7.03	9.03	8.15	S.
28	20	+42.00	20.— 64.		7.63	1.44	1.00						K.
29	14	+56.79	1.28	44	11.19	1.18	6.07						S.
30	15	+70.00	3.00— 5.00		12.20	1.45	.35						S.

¹Observer sat.²Roughly estimated.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY
OF CORNELL UNIVERSITY.

Communicated by E. B. TITCHENER.

III.

TWO POINTS IN REACTION-TIME EXPERIMENTATION.

By R. WATANABE, PH. M.

We suppose that every psychologist hopes to win something for *psychology* from experiments made upon the simple reaction-time. The simple reaction is the exact type of a voluntary action, and its study should throw light upon the processes involved in this. At present, however, but little has been done towards a psychological interpretation of reaction results, despite the great extent of reaction-literature and the mass of published figures. Indeed, Lange's distinction of the sensorial and muscular forms—which has aroused so much controversy—and Martius' supplementing of it by the introduction of a third (central) form, have constituted almost exclusively the basis upon which psychological discussion has rested.

In the present paper, two questions will be treated: that of the value of subjective (introspective) control of the simple reaction-experiment, and that of the canceling-out of refractory figures. We shall endeavor to deal with each from the psychological point of view.

(1) What is the value of introspective control of the duration of a reaction? Martius ascribes a considerable degree of importance to it, though admitting that introspection is not adequate to the exact estimation of *Störungswerthe*.¹ But the question is not that of whether introspection can determine the relative durations of reactions of 129 σ and 130 σ —to take an extreme instance from Martius²—but rather this: can self-observation place a given experiment under the rubrics, "conditions good, instructions followed," and "conditions bad," or "instructions not followed," with such accuracy, that its verdict suffices at once to admit the experimental result to, or exclude it from the durations employed by the experimenter in averaging? The mean variation of the simple sensorial reaction, *e. g.*, may normally amount, in certain sense departments, to 40 σ . Does self-observation reject times, which, if included in the final calculation, would raise the mean variation above that normal magnitude? We wish again to emphasize the fact that it is for this question a matter of indifference whether

¹Über die muskuläre Reaction und die Aufmerksamkeit. *Phil. Stud.*, VI. pp. 196-7; 204-5; 207-10; 214; 216. Dr. Bliss curiously makes Martius' results entirely negative. *Studies from the Yale Psychological Laboratory*, 1892-3, p. 26.

²P. 204.

we can exactly estimate time-periods of 100-300 σ , or not. We possess now our objective time-norms; we can check our own experiments by reference to those norms; we can pass from the investigation of the "time" of reaction to further investigation. It is, therefore, not enough to state—or to prove—that "the mind is unable to accurately estimate small divisions of time under the most favorable conditions."¹ The "mind" works through the *Reproduktion der Theilelemente des einzelnen Reactionsvorgangs, soweit er psychischer Natur ist.*² So that—to restate once more—the problem runs: is this reproduction sufficiently accurate to serve as a control of the admission or rejection of the single experimental result in the experimental tables?

Dr. Bliss finds that introspection is "of great value in estimating the general conditions of an experiment and showing the influences which affect the results," though it is "not to be trusted in estimating the results themselves,"³ since our time-estimation is not sufficiently accurate. It is clear, we think, from what has been said above, that the writer has misapprehended the function and manner-of-working of introspection in this particular case. He continues: "Its (the mind's) judgment is particularly liable to be affected by the conditions of the reaction. Its report is what it thinks ought to be, rather than what it actually sees." But this is merely a charging of the "mind" with liability to autosuggestion and *Einstellung*;⁴ and these are "constant errors" in all psychological experimentation, the end of which is the recording of a judgment.⁵ So far, therefore, a clear case against introspection has hardly been made out.⁶

The issue can only be decided by appeal to experimentation. And we believe that, here as elsewhere, strict regard must be had to the sensorial muscular difference. Experiments which were carried out at the Leipzig Institute,⁷ and others which have been made at Cornell,⁸ point, as it seems, convincingly to this conclusion.

In each case the observer, isolated in Lange's way, was given pencil and paper, and required to note down after every experiment his opinion as to the validity of that experiment. The injunction was to the following effect: "Mark all accidental sources of disturbance (stray sounds, entrance of sudden light, 'catching' of the attention by objects in the room, etc.), and state, as explicitly as possible, whether you have reacted as you have been directed (following all instructions as to attention to signal, resting of finger on key, position of head and body, direction of the attention during the experiment, etc.)." Some reagents tended very quickly to confine themselves to such notes as "quick," "slow," but this attempt to estimate the reaction in terms of time was discouraged, it being pointed out that the times were not by any means so important as the qualitative self-analysis, in which introspection (in Martius' sense) furnished a training. Between experiment and experiment there was allowed to elapse a time sufficient for the "settling-down of the mind" to a new essay, unbiased (except for constant influences) by those which had preceded.

¹ *Stud. from the Yale Psych. Lab., i. c.* The statement is sufficiently daring, in other connections.

² *Phil. Stud., VI. 216.*

³ *L. c.*

⁴ For which cf. Külpe, *Grundriss*, pp. 208, 470; 44.

⁵ Both in both methods of procedure, the "wissentliches" and the "unwissentliches Verfahren." Cf. *Studies*, etc., 43.

⁶ On p. 44 a position is taken by Dr. Bliss which accords more nearly with our own but which can hardly be reconciled with that of p. 36.

⁷ *Phil. Stud., VIII. 138 ff.*

⁸ *This Journal*, Jan., 1894.

We subjoin two specimen tables. In these, *b.*="bad," *g.*="good," *s.*="short," *l.*="long."

TABLE I.

Dec. 8, 1891. Reagent *Wn.*
Sensorial. Light-stimulus.
Ordinary key. Unit = 1 σ .

1. 499 *too l.*
2. 810 *quite b.*
3. 248 *g. or s.*
4. 342 *g.*
5. 341 *g.*
6. 210 *too s.*
7. 530 *too l.*
8. 331 *g.*
9. 251 *g. or s.*
10. 283 *g.*
11. 221 *s.*
12. 348 *g.*
13. 337 *g.*
14. 126 *quite b.*
15. 344 *g.*
16. 274 *too s.* [!]
17. 325 *g.*
18. 367 *l.* ?
19. 414 *l.* ?
20. 343 *g.*
21. 206 *s.* ?
22. 311 *g.*
23. 304 *g.*
24. 290 *g.*
25. 331 *g.*
26. 348 *g. or l.*
27. 278 *g. or s.*
28. 327 *g.*
29. 255 *g. or s.*
30. 292 *g.*
31. 332 *g.*
32. 273 *g.*
33. 322 *g.*
34. 420 *too l.*
35. 325 *g.*

TABLE II.

Dec. 1, 1891. Reagent *Wn.*
Muscular. Light-stimulus.
Ordinary key. Unit = 1 σ .

- 223 *l.*
- 145 *g.*
- 190 *g.*
- 213 *l.*
- 214 *s.* [!]
- 165 *g.*
- 174 *g.*
- 156 *g.*
- 289 *g.* [!]
- 246 *g.* [!]
- 259 ? ?
- 209 *g. or s.* [!]
- 142 *g.*
- 230 *l.*
- 195 *g.*
- 194 *g.*
- 199 *g.*
- 162 *g.*
- 195 *g.*
- 181 *g.*
- 139 *g. or l.* [!]
- 112 *g.*
- 146 *g.*
- 124 *g.*
- 202 *l.*
- 201 *g.*
- 203 *g.*

[!]

Table I. contains thirty-five sensorial experiments.¹ Eleven of these were rejected by the reagent.² If we average the remaining twenty-four (all those in the record of which the judgment *g.* occurs), we obtain the value (311.8 = reduced) 298 σ , *m. v.* 27.6 σ ; and practically the same time-value results, if we admit (16) 274, the only case in which the observer's estimate was inaccurate. This was a series taken early in the reagent's course of practice. (His sensorial time finally averaged [reduced] 260 σ .) Introspection is already wholly adequate to the work required of it.³

¹ This, a practice-series, is longer than the experimental-series proper. The best length for the latter seems to be 20-25 experiments. Cf., among others, Dr. Bliss, p. 17.

² *W.* in *Phil. Stud.*, VIII, 138.

³ As we are not dealing here with the qualitative course of the reaction-experiment in detail, we omit from the experimental records all enumeration of the various factors which determined the reagent's judgment. Though valuable in itself, such enumeration would only serve to obscure the present issue by over-weighting the text. We may, however, repeat that the abbreviations in the text do not stand for time-judgments: "l." or "s." is simply an inference (of experimentee or experimenter) from the qualitative judgment or judgments entered in the reagent's protocol.

Table II. shows twenty-seven muscular times, taken from the same reagent, at a somewhat less advanced stage of practice. (Three experiments failed altogether; nothing is said of these in the table.) The time of this observer proved ultimately to be (reduced) 178,6 σ , m. v. 19,8 σ . The result of these practice experiments, according to his own protocol, would be (179,2 = reduced) 189 σ , m. v. 31,3 σ . The length of the reaction-time itself, and the magnitude of the mean variation, might be ascribed to deficiency of practice; and this explanation would hold, in the rough.¹ Looking at the figures in detail, however, we see at once that the introspective criterion is not valid. Let it be remembered that the judgments were not arrived at in terms of the time-value of the whole reaction-process, but in terms of fulfillment or non-fulfillment of experimental directions and conditions. With this standard of comparison (4) and (5) are judged as long and short respectively; (9) and (10) judged as good (these were sensorial reactions, the reagent tending, during his then stage of practice, to slip unconsciously from the muscular form to the sensorial, which was easier for him); (12) judged as short; (21) as long; (25) as long; (26) and (27) as good. Introspection is, therefore, as inadequate as it was in the former instance adequate.

It may be urged that it is unfair to make these practice-experiments the norm of experiment in general. The objection does not hold. As regards sensorial times, the introspective control certainly improves, but can improve only little, with increased practice;² so that Table I. is a stronger witness to its validity in this sphere than a later-taken experimental series would be. As regards muscular times, the introspective control cannot improve with practice; there is nothing present by which it should improve. The observer can only say: right or wrong, in this direction or in that. Now, when practice is complete, one obtains four general types of muscular reaction: (α) correct reactions; (β) (technically) premature reactions; (γ) (technically) false reactions, and (δ) bad reactions, due to inattention, etc., or to disturbance from without. Of these (β) are as a rule regarded by the experimentee as correct; indeed, we shall admit that they *are*, in all probability, qualitatively correct reactions, if we accept Külpe's explanation of them as muscular reactions to memorial representations of the stimulus. (γ) are usually cognized by the reagent as reactions to signal, or to some interferential stimulus. Where this is not the case, they are regarded as correct; and they may be qualitatively correct—answers to the thought, "I have to react muscularly to a sense-impression."³ As to (δ), external disturbances are, of course, controlled without the aid of introspection; while internal disturbances do not, as a rule, occur; so that introspection has but a very restricted field for activity. One frequently obtains an unbroken series of (α). To sum up. During muscular practice, introspection proves untrustworthy. After practice, the sphere in which the coincidence of its verdict with the objective result can be tested is too small for the drawing of a definite conclusion; the reaction tending to become automatically regular, or, when irregular, not admitting the necessary comparison.

It is needless to multiply instances. We shall quote only two other series, chosen at random from a large number.

¹ We do not mean by this to contradict Martius. *Phil. Stud.*, VI. 196.

² Even so, it is crossed by the tendency to automatism in the practised reagent.

³ *Phil. Stud.*, VI. 331.

TABLE III.

Feb. 25, 1893. Reagent *We*.¹
 Sensorial. Sound-stimulus.
 Lip key. Unit = 1 *c*.

1.	327 ?
2.	333 ? <i>L</i> .
3.	259 <i>g</i> .
4.	243 <i>g</i> .
5.	257 <i>g</i> .
6.	226 <i>g</i> .
7.	264 <i>g</i> .
8.	416 <i>L</i> .
9.	231 <i>g</i> .
10.	229 <i>g</i> .
11.	270 <i>g</i> .
12.	239 <i>g</i> .
13.	246 <i>g</i> .
14.	222 <i>g</i> .
15.	281 <i>L</i> .

TABLE IV.

Feb. 17, 1893. Reagent *L*.¹
 Muscular. Sound-stimulus.
 Lip key. Unit = 1 *c*.

154 <i>s</i> .	[!]
92 <i>g</i> .	?
105 <i>g</i> .	
102 <i>g</i> .	
— <i>g</i> .	[!]
107 <i>g</i> .	or <i>s</i> .
127 <i>g</i> .	
27 <i>g</i> .	[!]
131 <i>g</i> .	
118 <i>g</i> .	or <i>s</i> .
112 <i>g</i> .	

We have not enough experimental material to enable us to form an opinion with regard to the value of introspection in the case of central times.

To summarize: So far as our experience extends, we are convinced that:

1. Introspection affords an adequate control for the simple sensorial reaction;

2. While it is very much less trustworthy in its verdict upon the simple muscular reaction, the conditions of experimentation being unfavorable to its employment.²

(2). Are discrepant figures to be struck out in averaging? The answer to this question is already given, with the answer to the former. (*a*) In the case of sensorial times, it will be best to cancel all those which the reagent regards as untrustworthy; even though there are thereby sacrificed a certain number of results which accord with the established norm. (*β*) With muscular times it is different. All provably false reactions, and with them the obviously premature reactions (Table IV. [5] and [8]), must be put aside. For the rest, arbitrary canceling is, fortunately, rendered unnecessary by the extreme regularity of the reactions obtained from a practised observer. If obdurate results do occur, elimination by making the number of experiments very great seems to us to be the only right mode of procedure.³

¹ This JOURNAL, Jan. 1894.

² By way of a final criticism of Dr. Bliss's results under this head, we would say: (1) That the question, "Are these reactions muscular or sensorial?" (p. 37) raised after the event, can hardly be answered, except schematically, in terms of the figures and their mean variations. It should have been put earlier. And (2) that the results on pp. 33-5 appear to emanate from a too "careful" observer. It is one thing to introspect, another to inspect your introspection.

³ Cf. Dr. Bliss, p. 18.

IV.

ON THE QUANTITATIVE DETERMINATION OF AN OPTICAL ILLUSION.

H. W. KNOX, A. B.

It is a well-known fact that, under certain conditions,¹ a dotted line appears longer than a point-distance which is objectively equal to it. We do not propose here to deal with the explanations (Hering-Kundt, Aubert, Wundt, etc.) proposed for the phenomenon; but rather to describe an attempted numerical determination of the special illusion, with a view to the ascertainment of regularity or irregularity in the course of its absolute or relative sensible discrimination.

Kundt² gives particulars of three quantitative series. He and his reagents observed the blackened ends of steel points, 30 mm. long, upon a background of white paper. Five such points were employed: A, B, C and E were kept constant; D was moved to and fro, until AD was for sensation = DE. The data obtained were as

A B C ←D→ E

follows. (1) Distance of D from the nodal point of the observing eye, 338 mm. Two observers, R, L, and binocular experiments. Experiments massed = 240. AB = 20.2 mm.; BC = 40.2 mm.; AE = 241.9 mm.; AD = 117.64 mm. Error = 6.62 mm. (2) Distance, 328 mm. Two observers. R and L experiments. Experiments massed = 80. AB = 22.6 mm.; BC = 65.5 mm.; AE = 241.9 mm.; AD = 116.55 mm. Error = 8.80 mm. (3) Distance, 226 mm. One observer. R, L. Experiments, 40. AB = 25 mm.; BC = 10.5 mm.; AE = 100 mm.; AD = 48.52 mm. Error = 2.96 mm.³

Aubert used four white verticals upon a black background. The distance 1-4 = 100 mm.; 5 was moved to and fro, until 1-4 = 4-5.

1 2 3 4 ←5→

The experiments, at 2000 mm. distance, gave the error 12 mm.; at 1000 mm., this amounted to 10.5 mm.; at 500 mm., to 11 mm. This latter determination makes it about one-tenth of the whole comparison-distance. Kundt's third series, the conditions of which are similar to those of Aubert's third, had made it as small as one-thirty-fourth. Aubert suggests that Kundt, influenced by knowledge of the illusion, and being his own observer, had unconsciously corrected it.⁴

Before making any remarks upon the results of Kundt and Aubert, we will set forth those obtained in our own experimental series.

New Experiments. We employed, as background, white cards, 125 x 75 mm. in size. The center of each card was the center of the whole line, composed of the dotted line (AB) and the point-distance (BC). BC was kept constant, throughout each of our four card-

¹Not under all. If the "dotted line" is a point-distance halved by a single point, it appears smaller than the point-distance. Cf. Mellinghoff in Wundt, *Phys. Psych.*, 4th Ed. II, 142.

²Foggeendorff's *Ann. d. Phys. u. Ch.*, 1863, Vol. 120 (196). Pp. 113 ff.

³These are absolute errors. Thus, in the last case, the equation (AD) 48.52 = (DE) 51.48 gives the error 2.96. It is with this absolute error that we are dealing. Kundt reckons the error from the objective center of AE; in the case cited = 1.48. He points out, rightly, that for comparative purposes it should be judged not from the objective, but from the subjective (*Augenmass*) center of a point-distance objectively = AE. Cf. Hering, *Beiträge*, 1861, I, 69.

⁴*Physiol. d. Netzhaut*, 266 ff. *Physiol. Optik.*, 630, 1. Cf. Helmholtz, *Physiol. Optik*, 1st Ed. pp. 462-3.

series. It shall, therefore, be termed C , in the experimental tables; while AB is termed V (variable). The dots in AB (marked in draughting ink with a Keuffel and Esser dotting pen) were 0.5 mm. apart, measured from center to center; and, approximately, 0.3 mm. in diameter. In *Series I.*, $BC = 25$ mm.; AB varied by half mm. increments from 17 to 27 mm. In *Series II.*, $BC = 30$ mm.; AB varied between 20 and 34 mm. In *Series III.*, $BC = 35$ mm.; AB varied between 24 and 38 mm. Finally, in *Series IV.*, $BC = 40$ mm. AB varied between 29 and 44 mm. Great care was observed that the dots might be equidistant, and of as equal magnitude and clearness as was possible.

The cards were held by the observer at the distance of clear vision (350-400 mm.), and the distances estimated by diffused daylight; the conditions of holding, of illumination, etc., being kept as uniform as possible. For the elimination of constant errors, horizontal judgments were made in equal numbers of $V-C$ and of $C-V$;

vertical judgments of $\frac{v}{c}$ and of $\frac{c}{v}$. The method employed was a form of the method of minimal changes. We took, e. g., $V < C$, and increased it till it appeared $= C$. This value was noted down. For some few cards further, the subjective equality might persist. So soon as the limit was reached at which V appeared $> C$, its value was again noted. Similarly with $V > C$. The mean of these four lengths gave, of course, the estimation value (R) of C in terms of the dotted line.

Experiments were in no case made upon an observer for more than fifteen minutes consecutively. The manner of judgment differed with different individuals. We were able to distinguish five methods of judging. (a) Judgment by reference of the final dots of C and V to the center and boundary-lines of the card. Curiously enough, C appeared to increase (decrease) within limits, as V increased (decreased).^[1] (b) Judgment by superposition of V upon C , or vice versa. (c) Judgment in terms of eye-movement. (This is the method presupposed by Wundt, in his discussion of the illusion. For the Hering-Kundt theory it is indifferent whether the eye move or not. Aubert leaves the question of the influence of eye-movement open.) (d) Judgment by bisection of the whole line, $V + C$. (e) Judgment by memorizing C , and comparing a series of V with its memory-image. The method (a) was employed by one subject only, and only in a few experiments. The method (e) must not be too sharply distinguished from (b). It is significant that, whatever the method of judgment, the illusion remains. How small a part is played by individual differences in this regard will be shown by our tables. As a strict control of the method of judgment was impossible, it seemed best to allow each observer to follow the line of least resistance. The result was that the principal types,—(b), (c) and (d),—were not infrequently employed in common for the formation of one and the same judgment.

We found that there was manifested in some observers a tendency to regard C as the distance from inside edge to inside edge of the limiting dots, while V was measured from the outside edge of one to the outside edge of the other. In computing our results, however, we measured both lines from dot-center to dot-center. Each dot is approximately 0.3 mm. wide. C was, therefore, seen too short by 0.3 mm.; V too long by the same amount. Now the rela-

[¹This suggests an experimental method for the quantitative investigation of apperceptive completion (Wundt, Münsterberg, Külpe, Grützner, etc.). Experiments on this subject are at present being carried out in the Cornell laboratory.—E. B. T.]

tive difference-limen for moderate distances is put by Külpe and others at one-fiftieth.¹ A distance of 25 mm. would, therefore, be just noticeably different from one of 24.5; 30 from 29.4; 35 from 34.3; 40 from 39.2. Our values were 24.6, 29.6, 34.6, 39.6. The difference of each of these from its computation-value is, therefore, subliminal, and may be neglected. C may stand at 25, 30, 35 and 40 mm. What of V ? Shall we read, for a V of 22, 22.3; for a V of 28, 28.3? Plainly, 22 is just noticeably different only from 22.44; 28 from 28.56; etc. Again, the difference of the actual from the computation-values is subliminal; the latter may stand. To say that C of 25 = V of 22, and to say that C of 24.6 = V of 22.3 are, for psychophysical purposes, the same. It is not as if there were summation of errors, and C of 24.3 were compared with V of 22.6. In that case, 24.3 would be noticeably different from 25; though 22.6 would not differ from 22. We have mentioned this tendency, although it does not affect our results, because, as can readily be seen, it might under certain conditions of judgment exercise a considerable influence, and demand recognition in the final computation.

All our experiments were made binocularly, and without restriction as to eye-movement. Fairly complete series were obtained from six subjects: Miss Healy (*H.*), Miss Washburn (*W.*), Messrs. Knox (*K.*), Pillsbury (*P.*), Titchener (*T.*) and Watanabe (*Wa.*). The results are subjoined. Under R are given the estimation values of the four constants. The n column shows the number of records taken, each record including the determination of r_o' , r_o'' , r_u' and r_u'' , from which the average was calculated. Under the rubric *m. v.* is given the total mean variation of each separate R , calculated from the whole number of experiments.² The mean variation of R_1 , R_2 , etc., from each R , it seems unnecessary to give in a separate column; for the first three constants it lies between one and five tenths mm.; for the last it averages five-tenths,—*W.* having the largest *m. v.*, 1.2 mm., and *Wa.* the smallest, 0.1 mm.

TABLE I.

Reagent *H.* Slight astigmatism³. Method (*c*) predominantly follows. Special practice only. Unit = 1 mm.

SERIES.	C=25			C=30			<i>R.</i> C=35			C=40		
	<i>n.</i>	<i>m. v.</i>		<i>n.</i>	<i>m. v.</i>		<i>n.</i>	<i>m. v.</i>		<i>n.</i>	<i>m. v.</i>	
$C-V$	22.25	3	1.5	26.95	3	2.25	31.41	3	2.58	36.91	3	1.91
$V-C$	22.00	3	2.0	26.25	3	2.25	30.75	3	2.25	36.91	3	2.08
$\frac{V}{C}$	22.83	3	1.6	27.31	3	2.75	30.83	3	2.00	37.00	3	2.08
$\frac{C}{V}$	22.83	3	1.6	27.33	3	2.00	32.00	3	2.00	37.08	3	2.25
$Hor. \Delta^4$	-2.88			-3.40			-3.92			-3.09		
$Vert. \Delta$	-2.17			-2.68			-3.59			-2.96		

¹*Grundriss*, pp. 371-2. Wundt, *Vorlesungen*, 2d Ed. p. 162.

²This is, of course, not a true *m. v.* But it is sufficiently accurate for our purpose. Cf. Gruber, Ueber die spezifische Helligkeit der Farben, *Phil. Stud.*, IX. p. 435.

³Corrected by glasses.

⁴ $\Delta = R - r$.

TABLE II.

Reagent W. Very slight astigmatism¹. Methods (a) and (c).
General and special practice. Unit = 1 mm.

SERIES.	C=25 n. m. v.			C=30 n. m. v.			R. C=35 n. m. v.			C=40 n. m. v.		
C-V	23.00	3	1.08	27.66	3	1.45	32.50	3	1.25	38.16	3	1.33
V-C	23.33	3	0.91	27.08	3	0.95	31.25	3	1.33	36.83	3	1.16
$\frac{V}{C}$	23.16	3	0.91	27.75	3	1.00	32.13	3	1.08	36.50	3	1.00
$\frac{C}{V}$	22.75	3	0.93	27.75	3	1.00	32.50	3	1.25	37.50	3	1.08
Hor. Δ	-1.84			-2.63			-3.13			-2.15		
Vert. Δ	-2.05			-2.25			-2.69			-3.00		

TABLE III.

Reagent K. Vision normal. Method (e). General and special
practice. Unit = 1 mm.

SERIES.	C=25 n. m. v.			C=30 n. m. v.			R. C=35 n. m. v.			C=40 n. m. v.		
C-V	22.50	3	0.62	27.33	3	0.91	32.50	3	0.75	38.16	3	1.33
V-C	23.08	3	0.76	27.91	3	0.75	33.08	3	0.83	39.00	3	0.66
$\frac{V}{C}$	23.25	3	1.00	28.25	3	0.75	33.58	3	0.66	37.75	3	0.66
$\frac{C}{V}$	23.33	3	0.79	27.41	3	1.00	33.00	3	0.87	38.08	3	0.91
Hor. Δ	-2.21			-2.38			-2.21			-1.42		
Vert. Δ	-1.71			-2.17			-1.71			-2.09		

¹ Not sufficient to call for correction by glasses.

TABLE IV.

Reagent P. Vision normal. Method predominantly (b). General and special practice. Unit = 1 mm.

Series.	R.											
	C=25	n.	m. v.	C=30	n.	m. v.	C=35	n.	m. v.	C=40	n.	m. v.
C-V	23.28	16	0.63	28.25	16	0.64	32.98	16	0.75	37.75	16	0.72
V-C	23.82	16	0.69	29.25	16	0.63	34.04	16	0.68	39.00	16	0.59
$\frac{V}{C}$	23.10	10	0.62	27.60	10	0.56	32.34	10	0.48	36.87	10	0.68
$\frac{C}{V}$	23.38	10	0.56	27.72	10	0.60	32.67	10	0.50	36.82	10	0.77
Hor. Δ	-1.45			-1.25			-1.49			-1.83		
Vert. Δ	-1.76			-2.34			-2.50			-3.16		

TABLE V.

Reagent T. Vision, L., normal; R., myopic¹. Method predominantly (b). General and special practice. Unit = 1 mm.

Series.	R.											
	C=25	n.	m. v.	C=30	n.	m. v.	C=35	n.	m. v.	C=40	n.	m. v.
C-V	20.96	8	0.87	24.03	8	0.96	28.62	8	0.93	33.90	8	0.87
V-C	20.62	8	0.78	24.40	8	0.82	29.18	8	0.87	34.56	8	0.81
$\frac{V}{C}$	20.20	5	0.65	23.07	5	0.70	28.05	5	0.85	33.25	5	0.85
$\frac{C}{V}$	20.55	5	1.00	24.40	5	0.70	28.15	5	9.95	34.15	5	0.85
Hor. Δ	-4.21			-5.79			-6.10			-5.77		
Vert. Δ	-4.63			-6.27			-6.90			-6.30		

¹See below, Remarks (5).

TABLE VI.

Reagent Wa. Vision slightly myopic. Method mixed. General and special practice. Unit = 1 mm.

SERIES	C=25				C=30				R				C=35				C=40			
	n.	m.	v.		n.	m.	v.		n.	m.	v.		n.	m.	v.		n.	m.	v.	
C-V	23.41	3	0.58		28.75	3	0.66		32.91	3	0.58		39.55	3	0.58					
V-C	23.75	3	0.41		29.25	3	1.58		33.08	3	0.58		37.91	3	1.06					
V																				
C	22.41	3	0.66		26.00	3	0.75		33.33	3	0.58		35.91	3	0.25					
C																				
V	24.33	3	0.83		30.16	3	0.75		34.58	3	0.50		39.25	3	0.50					
Hor. Δ	-1.42				-1.00				-2.01				-1.27							
Vert. Δ	-1.63				-1.92				-1.05				-2.37							

Remarks. We notice (1) that the illusion holds for every observer, for both vertical and horizontal judgments, and whatever the method of judgment.

(2) Since our judgment of vertical distances is in general less accurate than our judgment of horizontal (Chodin, Volkmann), we should expect to find a higher value of Δ in the former case than in the latter. The twenty-four comparisons of our tables give fourteen confirmations, ten refutations of this expectation. Two of the contrary cases, however, may be regarded as neutral (2.83, 2.17; 2.63, 2.25): so that we have fourteen *r*, eight *w*, two =. Of these eight, four belong to Table I.; two to Table III.; one each to Tables II. and VI. The results of Table I. were taken (a) from a previously unpractised subject, who (b) began with horizontal judgments. These latter, therefore, may well be regarded as belonging to a stage of less complete practice than the vertical judgments. We have, then, in conclusion, fourteen *r*, four *w*, two =. When we consider the fewness of our experiments, we cannot but think that this is as strong a confirmation of our expectation as could have been hoped for.

(3) Binocular bisection of horizontal distances is not subject to any constant error; binocular bisection of verticals is subject to the constant error of over-estimation of the upper part of the field of vision.¹ We should, therefore, expect to find the *m. v.* of our vertical Δ 's greater than that of our horizontal. The results, if Table I. is omitted, are: *r* 6, = 7 (.99, .92; .79, .76; .66, .59; .63, .58; .82, .82; .90, .85; .58, .54), *w* 7. [Table I. gives *r* 2, = 1 (1.7, 1.6), *w* 1.] This is curious. We are unable to offer any explanation of the result.

(4) Is there any constancy of the relative or absolute sensible discrimination? Of the latter, obviously not. The values of $\frac{\Delta}{r}$ are:²

¹ Wundt, *Grundzüge*, 4th Ed., II. 139, 140.

² *Op. cit.*, I. 343.

- I. Hor.: $\frac{1}{8}-\frac{1}{8}$, just over $\frac{1}{8}$, just over $\frac{1}{8}$, just over $\frac{1}{8}$. Vert.: $\frac{1}{11}-\frac{1}{12}$,
 $\frac{1}{11}-\frac{1}{12}$, $\frac{1}{10}-\frac{1}{10}$, $\frac{1}{12}-\frac{1}{12}$.
 II. Hor.: $\frac{1}{11}-\frac{1}{11}$, $\frac{1}{11}-\frac{1}{12}$, $\frac{1}{11}-\frac{1}{12}$, just over $\frac{1}{10}$. Vert.: $\frac{1}{12}$, $\frac{1}{12}-\frac{1}{12}$, $\frac{1}{12}$, $\frac{1}{12}$.
 III. Hor.: $\frac{1}{11}-\frac{1}{12}$, $\frac{1}{12}-\frac{1}{12}$, $\frac{1}{12}-\frac{1}{12}$, $\frac{1}{12}$. Vert.: $\frac{1}{14}-\frac{1}{15}$, just over $\frac{1}{14}$, just
 over $\frac{1}{14}$, $\frac{1}{15}$.
 IV. Hor.: $\frac{1}{17}$, $\frac{1}{17}$, $\frac{1}{17}-\frac{1}{17}$, $\frac{1}{17}$. Vert.: $\frac{1}{14}$, $\frac{1}{12}-\frac{1}{12}$, $\frac{1}{14}$, $\frac{1}{12}-\frac{1}{12}$.
 V. Hor.: $\frac{1}{8}-\frac{1}{8}$, $\frac{1}{8}-\frac{1}{8}$, just over $\frac{1}{8}$. Vert.: $\frac{1}{8}-\frac{1}{8}$, $\frac{1}{8}-\frac{1}{8}$, $\frac{1}{8}-\frac{1}{8}$, $\frac{1}{8}-\frac{1}{8}$.
 VI. Hor.: $\frac{1}{17}-\frac{1}{17}$, $\frac{1}{17}$, $\frac{1}{14}-\frac{1}{15}$, $\frac{1}{17}-\frac{1}{17}$. Vert.: $\frac{1}{13}-\frac{1}{13}$, $\frac{1}{13}-\frac{1}{13}$, $\frac{1}{13}$, $\frac{1}{17}$.

This apparent chaos reduces itself to some order on close inspection. To notice are the following facts. (a) The values of Table I. are all too large, for the reason already given. Moreover, here the vertical discrimination is (contrary to rule) finer than the horizontal. An explanation of this fact has been also suggested above. (b) The irregularities of Table III. may be referred to the fact that the reagent K. was the experimenter in all the experimental series of the other tables, and had himself prepared the cards employed in experimentation. (c) The smallness of certain fractions in Table IV. may be due to the very large number of experiments made with the reagent P. This number was probably too large. One of the difficulties in the way of the quantitative determination of an optical illusion is just this, — that the observer must be practised enough to be a good discriminator, but not so practised that he minimizes the illusion by correction. (d) Table V. we shall deal with separately, later. (e) The $C = 40$ mm. gives throughout a smaller fraction than we should expect. The reason for this does not, however, lie in any of the conditions of the illusion under investigation, but in the nature of our apparatus. Our cards were 125 mm. long. The 40 mm. series of lines varied between 69 and 84 mm. The average length of the line, the dotted part of which was judged equal to 40 mm., was 77 mm. Plainly, the terminal dots of this are dangerously near the card-edges: the distance on either side amounts only to 24 mm. When the cards were being prepared, the question was raised as to whether the 40 mm. series could safely be introduced, or whether it would not be better to use a larger sized card. We decided to test the matter; and our results show decisively the determining influence of the too-near limiting line. (f) Table VI. is puzzling. Want of practice will not explain the

heterogeneousness of the $\frac{\Delta}{r}$ values; for Table I. shows a clear uniformity, on the basis of very much less practice. We are inclined to suspect a more or less sporadic tendency on the part of the reagent to correct the illusion. Such a tendency is hard to guard against; and conscious avoidance of it may easily lead to exaggeration of the illusion.

Premising that the values of Table V. are explicable, and taking into account all the facts above enumerated, we would conclude from the experimental results, — though quite tentatively, and subject to correction by further investigation, — that *within the limits*

$C = 25-40$ mm. there is a constancy of $\frac{\Delta}{r}$, at about $\frac{1}{2}$ for horizontal estimation, and at a somewhat higher value for vertical. For the latter determination, see, especially, Tables IV. and V.

(5) How are the results of Table V. to be explained? We notice (a) that it is a typically good table, as regards the illustration of the uniformities which we have discussed. $\frac{\Delta}{r}$ is constant; the vertical Δ 's are greater than the horizontal; discrimination is affected by the "boundary-error" in the 40 mm. judgments; the

$m. v.$ shows 1 r , 1 w , and 2 = cases. (b) But the numerical values of \triangle and $\frac{\triangle}{r}$ are far too high.

The reagent T . proposed and planned the investigation. He had more familiarity than any of the other five subjects with the phenomena of optical illusion. He, therefore, approached the experimentation with a definite expectation of what would, in general, result from it; and he endeavored to give the illusion full play, being aware of the tendency towards correction which is apt to arise in the course of a sustained consideration of similar phenomena. It may be that this expectation and endeavor combined to bring about an exaggeration of the illusion; just as the knowledge and practice of the reagent P . combined to decrease it.

But T 's vision is also not normal; L . being emmetropic, R . myopic.¹ All the judgments of Table V. were made with the naked eye; i. e., monocularly, by L . Could this fact have influenced the estimation? In the cases of horizontal distances, Kundt was able in Series I. to mass his L ., R . and binocular results, (i) for a far-sighted subject, (ii) for his own unequal (*normal-kurzsichtig*) vision.² In Series II. he massed two sets of judgments, one made principally with L . the other with R .³ Series III. gave a subliminal difference for his own unequal R . and L .⁴ (L . = 48.26 mm.; R . = 48.78. AE = 100.) In determining the subjective middle of AE = 100, as point-distance, he found that the L .-judgment gave 50.33 for the left half, the R .-judgment 49.845 for the same half.⁵ He observes, after calculating the R . and L . errors of Series III. on this basis, that "sich die Verschiedenheit meiner Augen wieder sehr bemerklich macht;"⁶ but it is plain that the "very noticeable difference" is quite inadequate to explain the figures of our Table V. We must, therefore, lay the main stress upon the explanation which we first suggested.

Supplementary Remarks. (1) We have already noticed Mellinghoff's statement, that a point-distance halved by a dot appears smaller than a simple point-distance, which is objectively equal to it. It would be interesting to investigate systematically the quantitative relation of the illusion under discussion to the number of dots. We prepared a series of cards, upon which were two point-distances, AB and BC , of 30 mm. length. BC was varied in ten ways: left open, halved by one dot, divided up at equal intervals by 2, 3 . . . 9 dots. Some ten judgments gave the following constant result:—

¹ Formula for reading glasses: L ., convex, 0.25 + R ., concave, 1.25 diopter. For long-distance glasses: L ., concave, 0.09 + R ., 2.75 diopter.

² L . c., p. 132.

³ P. 133.

⁴ P. 134.

⁵ P. 135.

⁶ P. 136, note.

Dots in BC	REAGENT:				
	H.	K.	P.	T.	Wa.
0	=	=	=	=	=
1	=	—	—	—	—
2	=	+	+	+	=
3	+	↓	Illusion constantly increases	↓	Small +
4	↓	↓		Maximum +	"
5	↓	↓		↑	"
6	Maximum +	Maximum +		↑	"
7	↑	↑		↑	"
8	↑	↑		↑	"
9	+	+		+	"

Obviously, here is a problem, the investigation of which promises to bring to light new uniformities.

(2) *Practice.* The constancy and extent of the influence of practice on these experiments cannot be better shown than by the presentation of a series of observations from the results of the observer *P.* The series is *V-C*; *C* = 25.

Oct. 12, 1893.	<i>R.</i> = 22.87	<i>m. v.</i> = 0.62
" 18,	23.00	0.50
" 20,	23.12	1.12
" 23,	23.25	0.62
" 25,	24.00	0.75
" 26,	23.87	0.87
" 28,	24.12	0.75
Nov. 2,	23.37	1.37
" 3,	24.50	0.75
" 5,	25.12	1.12
" 9,	24.87	0.43
" 10,	24.37	0.43
" 11,	24.87	0.62
Dec. 6,	23.75	0.50
" 8,	24.75	0.25
" 10,	24.00	0.50

etc.

Practice has already been discussed above: *Remarks* (4).

(3) Can we compare our $\frac{\Delta}{r} = \frac{1}{3}$ with the values obtained by Kundt and Aubert? Aubert's $\frac{1}{10}$ is tempting; but he was dealing with vertical lines, not with points. And his filled space contained only two verticals. We have seen, under (1), that the character of the filling is essential in the illusion. Kundt employed points, it is true, but his distribution of them was curiously arbitrary. And his experiments are, on other grounds, hardly comparable with our own; so that the divergence of the two sets of results need not trouble us.

[Further experiments upon this special illusion are in progress in the laboratory.—E. B. T.]

V.

SOME APPARATUS FOR CUTANEOUS STIMULATION.

A. By M. F. WASHBURN, A. M.



FIG. 1.

1. The first *Aesthesiometer* appears to be that described and figured by Czermak (*Physiol. Studien*, III. Abth., 1855). Later instruments have followed the model of this, with the exception that both points are usually fixed at the same horizontal level, whereas in Czermak's *Stangenzirkel* "one limb (the terminal) is shorter than the other, and movable in the vertical direction." This latter arrangement allows of the application of two simultaneous pressures to a curved surface. But the general objection to the aesthesiometer—that there is no guarantee of constancy of stimulation-intensity from experiment to experiment—holds of all the slide-rule forms of the apparatus.

It appeared to us that the objection could be best met by a modification of the instrument, which would allow the two points to fall by their own weight, and with approximately constant friction, through a fixed support. And mechanical considerations narrowed this plan down to that of allowing a single rod, carrying the two points, to fall through such a support. The manner of constructing and affixing to the rod the cross-bar in which the points are inserted was suggested to us by the rough instrument employed by Professor Jastrow in his anthropometrical laboratory at the World's Fair.¹ The new aesthesiometer took final shape as follows.

FIGURE 1.

A is a heavy brass bulb, for holding in the hand. Through it passes a light steel rod, B, threaded at either extremity. The upper end carries any chosen one of a series of circular weights, C. The lower end is screwed into any chosen one of a series of blocks of hard rubber, D, which carry two rubber points, accurately distanced, and accurately turned to a final diameter of one-third mm. B is grooved over a certain portion of its length; and in the groove runs the point of a screw, E, the head of which is level with the surface of the brass bulb. The Cornell laboratory possesses a series of D, from a single point to a point-distance of 28 mm., the increments being 1 mm.; two AB; and weight-combinations from 15 to 90 gr. The dimensions of the whole apparatus can be chosen at pleasure.

The great advantages of this form of the aesthesiometer are: (1) The practical constancy of pressure, as between point and point, and as between experiment and experiment; if B is kept clean and well-oiled, its friction with A is minimal. (2) The absolute

¹ Professor Jastrow's perfected instrument is figured and described in the catalogue of the Garden City Model Works, 124 Clark street, Chicago, Ill., p. 1. The form with adjustable points has the disadvantage that the points themselves are formed each by a vertical and an inclined line, instead of by two lines equally inclined to the horizontal plane.

accuracy of the point-distances; there is no room for error of adjustment. Its disadvantages are: (1) Its restriction to the method of right and wrong cases; it can hardly be employed for minimal changes, unless two experimenters take part in the work done with it, and there are two *AB*, and a very long series of *D*—or unless *D* be remodeled, on the old slide-rule plan. (2) Its expense. (3) The fact that, at any other than a very moderate inclination of *AB* from the vertical direction, constancy of pressure is not obtained.

For demonstration experiments, *D* may be replaced, either by a rubber bar carrying roughly-pointed pyramids of hard rubber, as in Professor Jastrow's original apparatus; or (perhaps better) by a bar, *e. g.*, of wood, carrying a piece of sheet-rubber (one-hundredth inch) accurately cut to fine points at a definite distance from one another.

The instruments were made by the Pratt & Whitney Co., Hartford, Conn.

2. For *punctual* and *areal* cutaneous exploration, various instruments have been proposed, *e. g.*, by von Vintschgau (*Pflüger's Archiv*, X. 4. XVI. 318), Goldscheider (*du Bois-Reymond's Archiv*, 1887, 428), Dessoir (same *Archiv*, 1892, 308). After several failures, we have succeeded in constructing a fairly good, though by no means ideal, apparatus for the purpose. It is as follows.

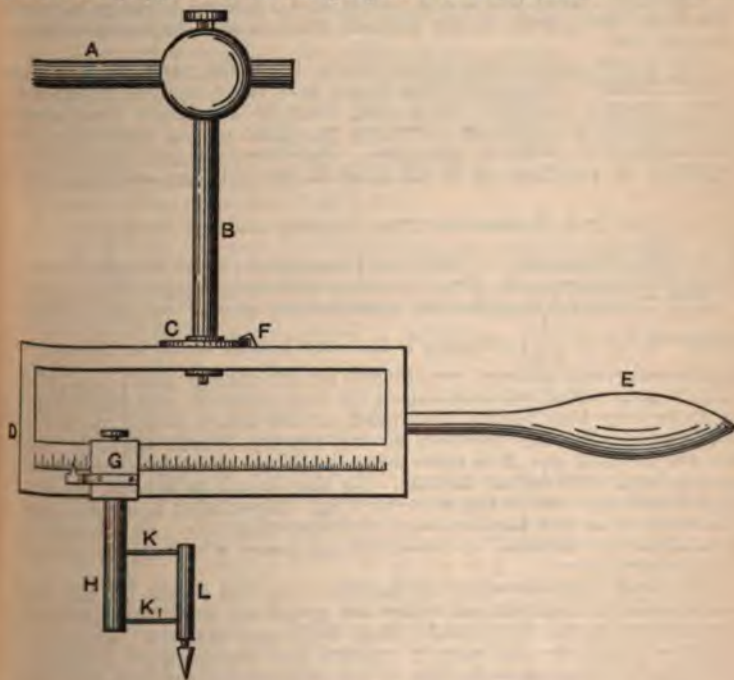


FIGURE 2.

A is a steel bar, movable in the vertical direction along the standard to which it is attached. *B* is a similar bar, movable around *A*. *C* is a fixed brass disc, divided up into degrees. *D* is a frame

of hard rubber, rotatable by the handle *E* around the extremity of *B*, and carrying a pointer *F*, which indicates the extent and direction of its movement, in terms of the scale marked upon *C*. The lower edge of *D* is provided with a mm. scale. Along this scale runs, in either direction, the brass carriage *G* whose pointer can be set at any number upon the scale. Attached to *G* is the brass rod *H*; and this latter carries on two stiff steel springs, *K* and *K*₁, the rubber point *L*. By means of *L* the skin can be explored, in circles of continually increasing diameter. The springs *K* and *K*₁ prevent any catching of *L* by inequalities of the cutaneous surface. "Pressure spots," etc., can be accurately mapped by reference to the scale upon the disc *C*; and, if any one point of the stimulated area is known, a series of experiments can be repeated with complete accuracy of localization from day to day.

The introduction of *K* and *K*₁ is essential. An arm, revolving round the lower extremity of *B*, and carrying *L* directly, is unworkable; even if a spline-and-feather be inserted in *B*, the slight roughness of the superficial skin causes in this case a dragging of *L*, and prevents exact localization.

The point of *L* may be made detachable from its stem, to allow of the application of various forms (areal and punctual) of stimulus (for temperature, pressure, pain). A spring-point enables the instrument to be used as a sensibilometer.

This instrument was made partly by C. Krille, Leipzig, partly in the Yale and partly in the Cornell workshop. Its cost should not exceed \$10.

For linear cutaneous stimulation, triangles may be cut from sheet-rubber (one-hundredth inch) and their apices inserted in light wooden handles. It is very easy to construct these simple instruments in sufficient numbers to admit of the employment of the method of minimal changes. Constancy of pressure must be learned by practice, as in the case of the ordinary aesthesiometer.

B. By E. W. SCRIPTURE (Yale University) and E. B. TITCHENER.

3. The *Kinesimeter* of Hall and Donaldson (*Mind*, X. 403) is a very imperfect instrument. The following criticisms suggest themselves:

(a) To start a machine by interlocking gears, means a variable error of $e = \pm \frac{t}{2g}$; where *g* is the number of teeth in the started gear, and *t* the rate of revolution of the motor gear; e. g., if the motor gear make one revolution in two seconds, and move a gear with twenty-four teeth, the speed of the latter at the moment of starting *dt* will vary between \pm one-twenty-fourth second. With a slow-moving car, it is sometimes possible actually to see the car move backward for an instant. As the speed increases, the error is diminished; but in the slow-moving kinesimeter it is very large. This objection is a fundamental mathematical one. In practice, no machines are started by interlocking gears where other methods can be employed.

(b) This variable error is thrown into the shade by the large constant error introduced, when the gears are not interlocked by a movement in the line connecting their two centers. Any movement around an axis simply pushes the second gear forward or backward. On a reproduction of the original instrument, this error amounted to about one-eighth inch in the movement of the car; the radius for the interlocking movement being about one and one-half inches. With the slow speeds used, the error would be rendered

negligible only by the employment of a radius of three feet or more.

(c) The stretching of the belt for the car was affected by placing the end-pulley on a spring. Every change of friction in the car would produce a change in the spring, a relaxation of the belt and a jerk in the movement of the car.

(d) To start a machine suddenly by connection with a kymograph throws more work on the latter, and lowers its speed, until the governor has time to react. During this time, therefore, the motion of the car is too slow.

(e) When the moving point meets with an elevation upon the cutaneous surface, it rotates upward; when it meets with a depression, downward. The upward movement differs but little from the horizontal; the downward movement is greatly accelerated. Between the fastest and slowest rates, for a nominally constant rate given by the car, there is a wide interval; the *m. v.* is very large.

(f) In the presentation of the results of the investigation, nothing is said as to these errors and constants of the apparatus; no measurements of its accuracy are given; there is not even a statement of the *m. v.* of the final result.]

In constructing a kinesiometer for the Cornell laboratory, we have introduced the following improvements:

(a) This error has been eliminated by the employment of friction-gears. The difficulty caused by the increased pressure of the wheels against their axis is removed by allowing two friction-wheels to clasp a third.

(b) This error does not enter into friction gears.

(c) This error is eliminated by tightening the belt with a screw-fastening for the pulley, as in machines in general.

(d) This error can only be avoided by the employment of a heavy fly-wheel on a vertical axis, run by some form of small motor (water, electricity, kymograph, etc.).

(e) This error is partially eliminated by the direct up-and-down movement of an extra point added.

The new instrument in the Cornell laboratory was built in the Yale workshop. It can be supplied at the cost of \$90—\$100. Its salient features are as follows. (1) *Table*. Of brass casting, hard metal. Top planed perfectly true; V's planed at the same time as table. Legs fastened by four screws. One leg has adjusting screw, to compensate any unevenness of support. Nickel plated, with coating of copper first. (2) *Car*. Brass, nickel plated. Four wheels, turned on a perfectly true arbor. Bearings are bushings of hardened tool-steel; holes ground and lapped, so as to ensure perfect trueness in running. Side-looseness of car easily taken out, and wheels easily taken off their bearings. Shaft of hardened tool-steel, ground and lapped; perfectly true and round. Horizontal slide holding vertical rod readily adjustable by special mechanism. Vertical rod itself (carrying rubber stimulus-point, wheel, tube, or whatever is preferred) held in position by brass nut clamping conical head of support on horizontal slide. (3) *Rotating Power in Head*. Comprises three gears and three friction rolls. Held in two brass plates, nickel plated; bearings are hardened bushings, set firmly in place. Movement of car regulated by lever; two directions and rest. Pressure of driving rolls against principal roll maintained by two springs, the tension of which can be adjusted by two brass nuts on the end of a bar connected with lever. Since the rolls are held together by spring-pressure, and not by a positive stiff joint, any unevenness (dirt on surface, etc.) will not affect the movement. Rolls perfectly true, turned on centers. By moving top plate, the whole driving section can be

taken apart. Rubber pulley held on its bearing by small screw on top, with conically shaped head. Shaft counter-sunk to fit; tightening screw expands slotted end of shaft, and the pulley is so held firmly in position. Three notches in support holding reversing lever. Center notch keeps car at rest. Balanced pointer mounted on two hardened centers. Case in which gear is held entirely independent of outside case holding large friction roll. Pressure against driving roll will not interfere with shaft of rubber pulley, which is mounted on bushings of hardened steel; the friction obtained by pressing the rolls together has nothing to do with running of this shaft, the two pressures being independent. Endless cord propelling car is joined by a knot, fitting into a rest and slot affixed to the car. Adjustment for tightening cord is a pulley at the other end of the table, with thumb-screw beneath.

This kinesimeter has been thoroughly tested on the smoked paper of the kymograph. It was found to be absolutely accurate, to the degree of accuracy obtainable from the kymograph.

ACCURATE WORK IN PSYCHOLOGY.¹

By E. W. SCRIPTURE, Ph. D., Yale University.

There are periods in the life of a science when it becomes necessary to take a decided stand against the tendencies prevailing at the present time, when the battle of the moment is not against the old enemies, but against the very ones that apparently support the new. The danger that threatens us comes directly from the psychological laboratories. We make experiments without an idea of the first laws of experiment; we make measurements regarding the fact that a science of measurement exists; we use apparatus without knowing the principles of its construction. Instead of changing all the conditions, except one, in a series of experiments to keep them constant, we vary a lot of them at the same time, often not knowing which ones are varied, and we assume to make deductions from the results of such work. Instead of carefully eliminating or correcting the sources of variation in our measurements, we manipulate the results as we think best. Instead of studying the apparatus with the utmost care to find the constants of errors, we prefer to consider that our results are only relative anyway and that constant errors do not count.

The struggle for ever increasing accuracy is the vital principle of the physical sciences. No astronomer, physicist or biologist would for a moment hesitate to declare that his work aims at the employment of ever more careful methods.

Very different is the case in psychology! We frequently hear it said that psychological experiments and measurements can never be accurate or trustworthy; at best they can only give an inkling of the truth of the case. The deduction made is that any particular apparatus is unnecessary, all the work can be done by any one who has fifty cents to spend for some colored paper. The conclusion is that, although we have psychological laboratories everywhere, on the one hand, with new ones added every year, almost all the scientific work of any value comes from two or three laboratories. The instruction in psychology is about on the level of the lessons for beginners in chemistry, or "physics in the laboratory." Such instruction has its value in its place, but the really scientific pursuit of physics and psychology is quite a different thing.

I will say nothing about the necessity of long training in the laboratory,² of the need of proper equipment in the way of apparatus, etc.; but I wish to call attention to a fundamental misconception by many American psychologists. This I do by using a single example for illustration; the reader can

¹ Delivered at the second annual meeting of the American Psychological Association, New York, 1893.
² 1892, XIX. 127.

easily extend the application. It is now nearly a century since Maskeleyne discovered that our mental activities required time for their occurrence. Hundreds of investigations have been made on its varieties and complications, and, yet, — with the greatest care that we can exercise to-day, our measurements of the simplest form of reaction-time give a mean variation of 10r to 12r. Putting the average of such a reaction-time at 180r, this means a mean error of, say, 6%, — a degree of inaccuracy that would not be tolerated in physics. Moreover, we know that these variations are due largely to such factors as distraction of attention, fatigue, general conditions of time and place, errors of apparatus, etc., none of which we attempt to evaluate or to more than roughly eliminate. As soon as we leave the simplest forms of reaction-time the mean variation becomes much larger; when we get to the complex forms of reaction, we sometimes do not know what we are measuring.

What would a true scientist do in such a case? In the first place, years of labor would be spent in getting the sources of variation under control. Scientist after scientist would seek means of controlling the amount of distraction to ever greater fineness and of measuring the effects of the various degrees of distraction on the reaction-time. Still others would step by step evaluate or eliminate the effects of different degrees of fatigue; others would manage to make the conditions of time and place practically constant. The story of such a gradual elimination of sources of error in astronomical recording, reaching over 3,000 years, is told by Jevons.¹ The subject of calorimetry will indicate how physicists go to work; the endless investigations on nerve irritability and conduction furnish a parallel case for physiology. Let us turn to psychology. Year after year, measurements on the time relations of mental phenomena go on at Leipzig with ever increasing accuracy and with the continual discovery of unsuspected facts. Investigations on new subjects that can be treated at first only qualitatively, are also taken up; e. g., experimental aesthetics, association of ideas, etc., but the fact is always kept in mind that each subject taken up should be pushed forward from time to time, ever a little further into the domain of accuracy. And in America? — From one of the leading laboratories we have the bold declaration that accurate work is not even desirable. More than once have the results of measurements been published without a statement of mean variations or probable errors, or of any data that would enable the reader to judge whether the methods were accurate enough to justify the conclusions drawn or not. Wundt considers such amateurism and carelessness to be the most dangerous enemies of modern psychology. It is this tendency to superficiality and dabbling, so common among American students, which must be eradicated at the beginning of their laboratory instruction. In the work of the Yale laboratory the attainment of accuracy and trustworthiness is made the indispensable condition in all research-work.

The illustration given above will serve to indicate the methods of search for errors and the labor spent on their elimination, as they are followed at the Yale laboratory :

1. Errors of the apparatus. The Hipp chronoscope, with which these experiments are usually made, has been rejected as not accurate enough. Aside from the errors due to remanent magnetism, springs, etc., the very construction of the catch-pin (b. in Wundt's *Physiologische Psychologie*, 4th ed. II. fig. 217) involves a mean

¹"Principles of Science," p. 271.

variation of 1%. We have, therefore, rejected all chronoscopes and developed the graphic method to such a degree that we can make and count records of any desired accuracy with less work than with the Hipp chronoscope.¹ All errors from time-markers have been eliminated by using the spark method. In fact, the degree of accuracy attainable is limited only by the accuracy of the tuning-fork. By an alarm-thermostat placed beside the fork, even the error due to changes of temperature is kept below $\frac{1}{10,000}$ of a second. Thus our apparatus is absolutely accurate for all records in thousandths of a second. I hardly need to say that we use only break-circuit keys for stimulating and reacting. To avoid errors due to muscular tension, we use our specially constructed reaction-key, that has no spring and that can be held in any position.²

2. Errors of surroundings. Of course, the experimenter, the recording apparatus and the stimulating apparatus are in a part of the building distant from the person experimented upon. He sits in the reacting room perfectly alone, knowing nothing of what is going on. The warning click of a sounder tells him to concentrate his attention; a click occurs in the telephone, or a Geissler tube flashes out, or an electric shock pricks the skin; he reacts in response and all is again quiet. All light and moving objects are, of course, excluded. Dr. Bliss's experiments have shown that a steady light of moderate intensity causes no distraction; we may consequently at the present stage of accuracy have the room lighted up by an incandescent lamp, if the observer is made more comfortable thereby. Disturbing sounds are probably the worst sources of error; their exclusion has been a difficult problem, but we have solved it by our isolated room. The distractions due to insufficient ventilation, changing temperature, changing barometric pressure and changing humidity have not yet been eliminated, but the arrangements thereto have been made and will be completed before long.

3. Errors due to poor powers of introspection. Trustworthy observation on the fundamental laws of mental life can be made only by trained observers. Let me show you a picture for a moment and then remove it. How much of that picture can you remember? If I had shown it to an average individual, the amount remembered would have been very small; yet you all know the story of Robert Houdin and his son, who by painstaking training were finally able to tell every article in a shop-window after casting a momentary glance at it,—the son, indeed, was able to glance for an instant over the shelves of a library and then tell from memory the title, cover and position of each book.

You had the picture before you for a few seconds; how much more difficult must it be to carefully observe and remember the rapidly changing phenomena of consciousness. The conclusion seems self-evident. You would not put a car-driver to seeking double-stars with the Lick telescope, and I think you will agree with Prof. Titchener, who claims that thorough psychological work can be done only by those trained in introspection. Indeed, it would seem unnecessary to mention the matter, if it were not for the wide-spread impression that everybody is just as capable of

¹Bliss, "Researches on Reaction-time and Attention." Studies from the Yale Psych. Laboratory, 1893, p. 3.

²Scripture and Moore, "A new Reaction-key and the Time of Voluntary Movements." Stud. Yale Laboratory, 1893, p. 83.

conducting a psychological investigation as the most experienced psychologist. Aside from the lack of technical knowledge and the ignorance in methods of research (research being a distinct art that has to be learned), there is the total lack of training as observers.

4. *Errors of statement.* There is a fourth source of error of which I am almost ashamed to speak publicly. Psychologists are constantly making measurements and giving the results in figures in a way utterly regardless of the existence of a science of measurement with definite rules. I will not detain you with any remarks on the subject. To careful workers acquainted with the subject, anything I could say would be quite superfluous; to others, anything I could say would also be quite superfluous, although for a different reason.

SOME PSYCHOLOGICAL ILLUSTRATIONS OF THE THEOREMS OF BERNOULLI AND POISSON.

By E. W. SCRIPTURE, Ph. D., Yale University.

Professor Bruns (*Ueber die Ausgleichung statistischer Zählungen in der Psychophysik*, *Phil. Stud.*, 1893, IX. p. 1) has pointed out the loss of energy in psychology consequent on the ignorance of the past development of the exact sciences. It is my desire in the present article to show how we can distinguish good psychological work from indifferent and bad by employing means known to every mathematician, but generally unknown to psychologists. The fact that I make use of recent publications by Professor Münsterberg for this purpose is not in any way to be taken as a disparagement of his work. The statements made in his "Studies from the Harvard Psychological Laboratory" in the first number of the *Psychological Review* for 1894, led me to inquire how far the conclusions drawn were justified by the facts given. As he has not verified his results by the proper mathematical means, the labor fell upon his readers.

In the chapter on Optical Time-Content, a series of conclusions is drawn in regard to the overestimation of an interval of time filled with one kind of visual impression as compared with an interval filled with another kind. The first question to ask is this: Accepting the facts exactly as stated, how much reliance is to be placed on the conclusions drawn from them? For all statistical work a definite answer was given by Jacob Bernoulli. Briefly stated, it is this: If there are n total cases in which A occurs r times and is absent s times, then the certainty that the observed result r agrees with the true result within the limits

$$l = \pm r \sqrt{\frac{2rs}{n}}$$

is given by

$$P = \frac{2}{\sqrt{\pi}} \int_0^l e^{-t^2} dt + \frac{\sqrt{n} e^{-\frac{l^2}{2rs}}}{\sqrt{2\pi rs}}$$

In case III. there are results given for six persons in their over-estimations of a changing color as compared with a monotonous one in 100 experiments each. The arithmetical mean of all the results is fifty-nine, but the range of variation extends from fifty-one for J. to sixty-five for P. The average range of variation is thus, $l = 7$. With what confidence can we state that the actual average fifty-nine expresses the truth? Substituting the values, $l = 7$, $r = 59$, $s = 41$, $n = 6$, in the above equations, we find that $P = 24\%$. Or, in other words, we would be justified in expecting that once out of every

four times a result lying somewhere between fifty-one and sixty-five would be obtained, which—it must be confessed—is a very small amount of trustworthiness in scientific work.

What should Professor Münsterberg have done? The answer is given by a reversal of Bayes's theorem, which I believe I am justified in making. It can be stated as follows: If the probability of the occurrence of an event is p , and of its non-occurrence is q , then, in order to assign a limit, l , with a certainty of

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} \frac{e^{-t^2}}{e} dt$$

the total number of cases must be $n = \frac{2 \gamma^2 p q}{P}$. As the experiments were made in hundreds, the numbers fifty-one, fifty-nine and sixty-five are also percentages. Thus $p = 59\%$ and $q = 41\%$. In statistical work we generally demand a degree of certainty.

$$P = 0.999978$$

on a scale of 1 = infinite or absolute certainty. For this value, $\gamma = 3$. By carrying out the processes indicated, we find that $n = 872$. From which we must conclude that with so inaccurate a method and with such variation among the individuals, a conclusion is justifiable only on the basis of experiments on nearly 900 individuals. Every statistician knows that experiments on a few individuals are of no value unless there is practically no variation among them. On the basis of six individuals, Professor Münsterberg says: "It appears to me unquestionably that, etc." Case III. is, moreover, flatly contradicted by case II., where the individuals disagree completely.

A very pretty illustration of one of Poisson's theorems can be drawn from Professor Münsterberg's statements on "Memory," p. 38. Two series, each of 2,140 presentations, visible and audible, were compared as to the frequency of mistakes. How large must be the difference between the results, in order to justify the conclusion that they are really different? The theorem can be stated thus: If in two groups, n and n' , the observed frequencies are p, q, p', q' , then we can conclude with a certainty of

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} \frac{e^{-t^2}}{e} dt$$

that the difference between the true probabilities R, R' , will be

$$K = R - R' = p - p' \pm \gamma \sqrt{\frac{2 p q}{n} + \frac{2 p' q'}{n'}}$$

Let us take the most unfavorable case, W., which gives 25.1% for the visual series and 31.6% for the auditory. Here $n = n' = 2,140$, $p = 0.251$, $q = 0.749$, $p' = 0.316$, $q' = 0.684$, and, with a demanded certainty of $P = 0.999978$, $\gamma = 3$. The computation gives 0.058 for the limits within which the observed difference may vary from the true difference. The observed difference is $31.6 - 25.1 = 0.065$, which exceeds the limits of possible variation. Professor Münsterberg is thus justified in concluding that the visual memory excels the auditory memory under the given circumstances.

ATION OF THE INTERFERENCE TO THE PRACTICE EFFECT OF AN ASSOCIATION.

By JOHN A. BERGSTRÖM.

ent paper gives the result of a further study of memory method described by the writer in this JOURNAL, Vol. V. While the problem studied here has a close connection with previous paper, it has been in part suggested by Dr. Berg's "Gedächtnisstudien," Part I., *Beiträge*, Heft 4, to which reference was made before, and by experiment X. of Müller and Schumann's recent studies, "Experimentelle Beiträge zur Untersuchung des Gedächtnisses," (*Zeitschr. f. Psych. u. Phys. der Sinne*, Bd. VI., 2 and 3, p. 173, 1893.)

Statement of Dr. Münsterberg's problem is this: Can a given association function automatically while some effect of a previous association with the same stimulus remains? He concludes that it can, and that nerve currents do not divide like electric currents along different lines of association inversely as the resistance, but that a slight difference, one way or another, diverts the discharge in that direction. The experiments, as may be seen, were made with some of the common habits of daily life, as the opening of the door of his room, dipping the pen in the ink, taking the watch out of the pocket.

Statement of Müller and Schumann's problem is this: When a nonsense syllable has been learned till the first correct response is possible, and is then relearned to the same extent within a certain interval, will more repetitions be required if, in the meantime, the syllables of the series have been associated with other syllables? Two twelve-syllable series were learned in one hour — one for a test, the other for a comparison experiment. The syllables of the test series were then united with twelve new syllables and made up into two series of such a nature that one of the test series should enter into an association with the new syllables.

After the new series were then learned. Two hours from the beginning of the test, the original test and comparison series were relearned. The number of repetitions required for the first learning of the test and comparison series were 7.29 and 13.0 repetitions, for the relearning 7.29 and 7.89, respectively.

As the authors point out, the effect of the interference of the new series is neither demonstrated nor disproved, since there is a possibility of error in the experiment. A considerable part of the time in learning a nonsense syllable series is spent in learning the new syllables, and in this case the syllables of the test series were relearned about twice as many times as those of the comparison series. The final relearning of the two. The influence of the greater familiarity with the syllables is, perhaps, considerable.

able, and all that can be said is that the interference effect is not greater than this practice effect. There is also the fact that the new syllables contain some of the elements of those that were displaced.

The authors, however, suggest another reason, namely, that associative interference is a small and transient influence, and that so few repetitions only are needed to make the first association act to the exclusion of the other that the interference effect, if any, is not great enough to appear, and that Dr. Münsterberg's conclusion referred to above may here find additional evidence. The impressions of their subjects, however, favor the opinion that the interference of associations has considerable influence, especially at first in learning new series.

Y The experiments reported in two brief articles upon this subject by the writer, demonstrate the fact of interference and show the rate of its decrease with time, and that it is nearly a constant quantity after a little practice. In the case of these experiments we are, however, dealing with the formation of a new association to take the place of a previous one, and not with the revival of a still earlier one, so that they do not exclude the hypothesis that in the case of two associations connected with one stimulus, a slight superiority of one makes it appropriate the whole nervous discharge.

+ The common view is, perhaps, that a nervous excitation radiates in all directions with an intensity proportional to the permeability, which is in turn, within certain limits, proportional to the number of repetitions; and that the work of breaking up a habit is roughly proportional to the work of forming it. It is hard to say, from general considerations, what should be expected in this case. Interference, as an obstruction to association, is often injurious to the individual, and we might expect that some means of obviating the difficulty should have been developed. On the other hand, it makes for the stability of the associational system of the brain — the very great degree of which is usually not realized in view of the great variety and apparently erratic nature of the mental processes. Interference may, on the other hand, be due to a fundamental mechanical condition, over which natural selection can have no influence, and might, therefore, be expected to appear in simple cases, while in the more complex cases, means of avoiding it may have been developed.

These special questions may all be grouped under the more general problem of the relation of the interference to the practice effect of an association. To the solution of this question in its simplest cases the experiments of this paper aim to contribute. Four cases are possible. The interference effect may be greater than the practice effect; it may be equal to it, or less than it; or the relation of the two may be a variable one.

The experiments were made during March and April, 1894. The subject, M. E. B., was wholly ignorant of the object of the experiments during the entire time, and had no special interest in the matter, except to make as quick records as possible. M. E. B. had become very expert from practice in previous experiments. There had, however, been no practice for about a year, and the records at first are about seven per cent. longer than those of the year before, or those made at the end of the present series. All the records made are used in the tables.

For a fuller description of the experiments, and the precautions necessary, the reader is referred to the previous paper (this JOURNAL, Vol. V. No. 3). The experiment consists of sorting eighty cards into ten different piles, each to contain eight cards with the

same picture. In sorting the same pack a second time, a given card may be placed in the position which it originally occupied or in one of nine other positions. If the piles, in the second trial, are in the same positions as in the first, we have a simple practice effect; if in one of the nine other positions, the pictures on the cards enter into associations which necessarily exclude their former associations and we have an interference effect. The first table gives the result of a preliminary experiment, the plan of which will be seen to be like that of experiment X. of Müller and Schumann's studies. The conditions here are, however, much more simple. The influence of changing familiarity with the objects to be associated is minimal in this case, since the outline pictures used have become perfectly well known by long practice; and any given picture is associated first with one movement or position and then with another, and forms only to a small extent a member of a series. Let A_1 and A_2 denote two entirely different arrangements of the piles into which the same pack is to be sorted; also, let B_1 and B_2 be different arrangements of another pack, and D an entirely different pack. The numbers at the upper right hand corner indicate the number or order of the repetition. Each experiment consists of two parts, a test and a comparison set of records, and may be represented as follows:

$$\begin{array}{ccc} \text{I.} & & \text{II.} \\ A_1 \text{ --- } A_2 & \text{--- } A_1^2 & B_1 \text{ --- } D \text{ --- } B_1^2 \end{array}$$

The interval from A_1 to A_1^2 , and from B_1 to B_1^2 , is 315 seconds. A_2 and D were begun 120 seconds after A_1 and B_1 respectively. The length of time required for A_2 and D is such as to make the interval between A_2 and A_1^2 , and D and B_1^2 , also approximately 120 seconds (105-112). The packs represented by D had words printed at the top instead of outline pictures. These are about as difficult to sort as A_2 , and were used to make the amount and distribution of the work done in the interval of II. approximately equal to that done in the interval of I., so that no difference of nervous excitement or fatigue might disturb the results. After a little practice the variations of the experiment are very small. This is probably due to the uniform attention which can be given to it, since the conditions are natural and the work is rather pleasantly exciting. The material is also quite regular.

There are twenty-four records for each value given in Table I., that is, 144 in all. The middle record is given as the most probable value, rather than the arithmetical average. What corresponds to the probable error of the average was obtained by taking the differences between the middle record of the whole and the middle records of the upper and lower halves respectively and dividing by $\sqrt{24}$. The middle record of the whole is, of course, the average of the 12th and 13th records. The distribution of errors is quite asym-

TABLE I.

	A_1	A_2	A_1^2	B_1	D	B_1^2
ord	60.5	83.1	75.6	60.2	90	52.7
of ion	+1.15, -.7	+ .95, -1.42	+1.25, -1.52	+ .84, -.88	+1.88, -1.8	+1.14, -.95

metrical in A_1 , A_2 , A_1^2 , and B_1^2 , but in the reverse direction in A_2 and A_1^2 from that which we usually expect in psychological measurements.

The interference effect of A_2 upon A_1^2 will be found by taking the difference between A_1^2 and B_1^2 , the time which would have been required by A_1^2 if A_2 had not been sorted. This is 22.9 seconds, which is very nearly equal to the interference effect of A_1 upon A_2 , -22.6 seconds. The effect is evidently quite marked and there is not a single negative result, either in these or the subsequent records. We may make three hypotheses with regard to the nature of the process: first, that A_1 and A_2 efface each other and that consequently the sorting of A_1^2 takes place under nearly the same conditions as A_1 , and hence requires less time than A_2 ; or, secondly, that the practice effect is not effaced, but remains unchanged as a tendency, and that the difference between A_2 and A_1^2 is simply due to the practice effect which remains over from A_1 . Thirdly, we might have partial effacement and partial permanence of the individual associations. If the first hypothesis is true, then a totally different arrangement, A_3 , should have the same advantage as A_1^2 . If the third is true, A_3 would require less time than if there were no effacement. Evidence in connection with Table II. will be given to show that the second hypothesis is the true one. General considerations also support this view. Any given idea enters into a great variety of combinations, and if its different associations tended to efface each other, we could not recall at will the different associates. As has been pointed out before, we have difficulty in these cases, due to the interference of association, and must often wait till the strength of the interfering association diminishes before we can get the one we want; but the fact that we can get it at all shows that it was not effaced.

This preliminary experiment does not, however, give us the relation between the interference and practice effect even as far as it goes; and a much greater number of repetitions of each association is desirable. As far as we know yet, practice may be more or less permanent and have a greater or less influence than interference. Further, the relative value of the divisions within the range of variation of the experiment ought to be known, or something like a zero method adopted for studying the subject. To meet these requirements the following experiment, which is the one upon which this discussion is based, was made. Using the same notation as before, the plan of the first part of the experiment may be represented as follows:

$A_1^1 A_2^1 A_1^2 A_2^2 A_1^3 A_2^3 \dots A_1^8 A_2^8$
and that of the second, or comparison part, by

$A_1^1 B_1^1 A_1^2 B_1^2 A_1^3 B_1^3 \dots A_1^8 B_1^8.$

A pack was sorted every three minutes, so that each part of the experiment required forty-eight minutes. Consider any two corresponding records in part I., as (A_1^6 and A_2^6). The memory work done upon each association of A_1^6 , before it is itself sorted, is 5×8 , or forty practice associations, and also forty interference associations. The interference associations have an advantage in being a little more recent. We know, from the experiments of the previous paper, approximately the rate of decrease of interference at this stage of practice for this subject. The difference between the interference for a two and an eight minute interval is only 3.49 seconds. On the whole, we should expect the accumulated advantage of the interference associations to come within five seconds. Accordingly, if the practice and interference effects of an association are equivalent, we should expect that A_1^6 will require from one to five seconds

nger than A_1 , which we find to be the case, if we take as a more probable value of A_1 , 61, the average of 58.53 and 63.48, or if we take the middle records. Similar reasoning applies to all the members of the A_1 series. A_2 , on the other hand, has been preceded by forty practice associations and forty-eight interference associations. As regards time, the interference associations have the same advantage as in the case of A_1 . We should, accordingly, expect to find the records of the A_2 series a little longer than those of the A_1 series.

All the precautions found necessary in the experiments of the previous paper have been observed here, and in addition, since it was necessary to use the same cards twice in these experiments and the number of repetitions was so great, an interval of a couple of weeks was observed before the same pack was used again. The arithmetical average and the usual probable error are given in Tables II. and III., since the number of records seemed too small for the same treatment as those in table I. Each average is the average of eight records. The middle records of the A_1 series, that is, the average of the fourth and fifth records, are also given, since this series is especially important.

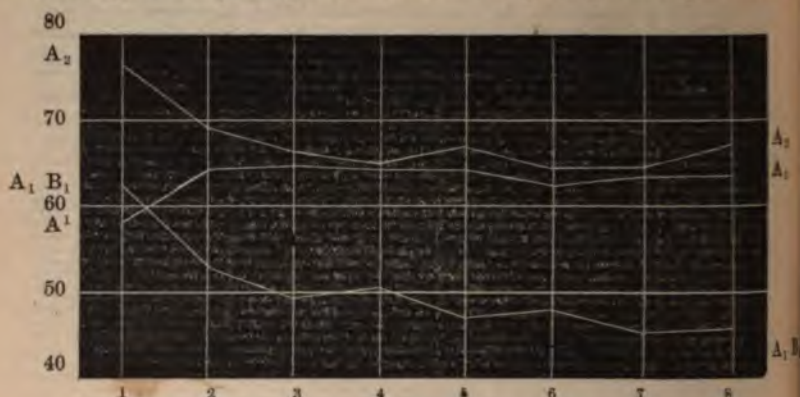
TABLE II.

No. of packs sorted.	1	2	3	4	5	6	7	8
A_1 Series	76.63	69.15	66.3	64.53	66.61	64.5	64.1	67.08
P. E. Av.	± 1.55	$\pm .728$	$\pm .95$	± 1.03	$\pm .982$	± 1.087	± 1.37	± 1.22
A_2 Series	58.53	64.95	65.30	64.05	63.98	62.68	63.85	63.35
P. E. Av.	$\pm .807$	$\pm .989$	± 1.47	± 1.38	$\pm .906$	± 1.00	$\pm .713$	± 1.36
A_1-B_1 Series	63.48	52.85	49.86	50.6	47.2	47.95	45.52	45.52
P. E. Av.	± 1.27	± 1.35	± 1.07	± 1.06	$\pm .893$	$\pm .969$	$\pm .129$	± 1.1
Middle Records of A_1 Series	59.4	63.7	62.7	63.1	63.3	62.4	63.	62.4

The records of the A_1 and B_1 sets of the comparison experiment $A_1 B_1$, correspond closely and are united in the averages.

The first record of the A_1 series is smaller; and that of the $A_1 B_1$ series is greater than we should expect at this stage of practice. The average of the two, 61 seconds, is probably more nearly correct. The first middle record of the A_1 series (59.4 seconds) is, contrary to the usual rule, larger than the arithmetical mean, which indi-

cates that this mean is too small. The middle records of the A_1 series are, with this exception, smaller than the corresponding averages. They are quite uniform and are perhaps to be preferred.



The facts of Table II. may be seen more easily in the accompanying cut. $A_2 A_2$, $A_1 A_1$, and $A_1 B_1 - A_1 B_1$ are the curves which correspond respectively to the A_2 , A_1 , and $A_1 B_1$ averages of the table. The abscissas represent the number of repetitions of each association, which varied from one to sixty-four. They may also represent the number of packs sorted, *i. e.*, from one to eight, and, since this was done at regular intervals, they may also represent the time. If we select any point along the abscissas, as, for example, the last, we see at once that under approximately the same conditions the preceding fifty-six practice associations reduce the record to 45.52, and that fifty-six practice and fifty-six interference associations so balance each other that the record is very nearly what it would be if they had not been made. It is, of course, a little higher, but this can be explained by the fact that the interference associations are a little more recent. The total interference effect of the sixty-four associations, as we know from eight tests taken at the end of the $A_1 B_1$ series, makes the record 83.4 seconds. In general, we may say that the line $A_1 A_1$ would be above or below its present position if the interference effect were not approximately equivalent to the practice effect, but either greater or less. If we assume that the practice effect is proportional to the number of repetitions, and that the interference effect is equal to the practice effect, multiplied by a certain constant, greater or less than unity, the line $A_1 A_1$ would evidently be straight, though making a certain angle with its present position. With the same assumption regarding practice and the number of repetitions, if the interference effect sustains a variable relation to the practice effect, $A_1 A_1$ would evidently be curved. If, on the other hand, the practice effect is not directly proportional to the number of repetitions, but is some other function of it, the last two assumptions about the relation of interference to practice being maintained, $A_1 A_1$ would have a curvature respectively similar to or different from that of the true practice curve. The true practice curve may, of course, differ considerably from the usual practice curve obtained by experiment, since the mechanical conditions of the experiment may prevent any reduction in time and yet the result of practice

how itself by a greater permanence of the associations. The true practice and interference curve should represent the energy of the associations, which is here the thing considered. If we take the interference records of our experiment as even approximately proportional to the true interference effect, it is evident that the first eight repetitions have relatively much greater influence upon the associative tendency than the subsequent fifty-six. Whatever the relation between true practice and the number of repetitions may be, the fact that $A_1 A_1$ is nearly parallel to the axis of abscissas, and is nearly straight from 2 to 8, and is, besides, but slightly above the level of records which have been preceded by neither practice nor interference associations, can only be explained by assuming the equivalence of the practice and interference effects. With the assumption of a variable relation of interference to practice, we might explain the fact that $A_1 A_1$ is straight and parallel to the axis of abscissas on any assumption of the relation of practice to the number of repetitions, but not its present position.

The total number of repetitions is about twelve times that required for the first free repetition from memory, since the cards can be thrown without reference to the piles when about two-thirds of the first pack has been sorted. The result is accordingly true for quite an advanced stage of practice.

There are, however, certain sources of error in the experiment which should be mentioned. With continued work most individuals experience at first an exaltation, then a depression of power. With the present distribution of the work, and this stage of practice, the variations for M. E. B. from these causes are very small, if demonstrable at all. In a previous experiment, tests at the beginning and end of an hour's work gave the records 57.05 and 56.30 respectively. The average of twelve hours, in which the kind and distribution of the work were very much like that of experiment II., does not show any such fluctuations. With two exceptions, the experiments of Table II., as well as those just referred to, were made about 8 A. M. The other two were made at what the subject felt were favorable times in the afternoon. All that is important is to take a time in which the subject's energy is not liable to variation.

The question regarding the nature of the nervous process, discussed in connection with experiment I., can now be answered definitely. If the series $A_1 A_1$ and $A_2 A_2$ do not simply balance but actually efface each other, any other arrangement of the same cards A_3 , would have the same advantage as A_1^s or A_2^s . At the end of four experiments a test was made with A_3 . The average of the records is 91.6 seconds, showing a great amount of interference. Hence, while different associations with the same stimulus interfere, they do not efface each other, but retain an individual existence. They probably do not even partially efface each other, as is shown by the following fact: The total interference effect of the sixty-four repetitions of the $A_1 B_1$ series gives a record of 83.4 seconds as an average of eight experiments. The tests by which the interference is shown were made as regular continuations of the experiment. While considerable variation must be allowed in the case of 91.6 seconds, it does not seem probable that the interference of the $A_1 A_2$ series is any less than that of the $A_1 B_1$ series, as would be expected if the contradictory associations partially effaced each other. This last matter is, however, not quite certain. In the $A_1 A_2$ series there are two associations to interfere; in $A_1 B_1$ only one. Furthermore, a little more work is done upon the $A_1 A_2$ series, the result of which we may, perhaps, suppose would be

to make the associations A_1 and A_2 stronger than they would be without interference. These facts leave room for the possibility of a small partial effacement. The results of previous experiments, on the other hand, support the view that the interference effect of two or more associations is not greater than that of one, other things being equal; since, if a succession of different arrangements, A_1, A_2, A_3, A_4 , etc., are used, the time of A_2 is considerably longer than that of A_1 , but the time of A_3, A_4 , etc., is nearly the same as A_2 , and the fact that the interference is greater in the A_1, A_2 than in the A_1, B_1 series might meet the demands of the second consideration, if they are just.¹ It seems probable, therefore, that there is not even a partial effacement of the associations.

In previous experiments the amount of interference remained quite constant, even while there was considerable change in general practice. We may explain this by assuming that the practice effect for that particular part of the process represented by the interfering associations attained its full measure very soon. While there is no great difference in general practice noticeable between experiments I. and II., there is considerable with regard to the interference effect. This may be due to the greater complexity of this part of the process in these experiments. It even seems probable that if experiment II. had been made at the stage of practice of experiment I., the records would have shown that the interference effect is greater than the practice effect. The test of the matter is of course to be made when the subject is in good training and capable of taking advantage of or mastering the practice and interference tendencies with something like uniform effectiveness. The amount that A_1^2 is above A_1 especially shows differences in the two groups of experiments gathered up in Tables I. and II., being 15.1 seconds in the first as compared with 3.95 seconds, or at most, 6.45 seconds, if we take the average of A_1 , Table II., at 58.53 seconds. A third group of experiments was accordingly made exactly like the first. The result is given in Table III. There are ten records for each average.

TABLE III.

	A_1	A_2	A_1^2	B_1	D	B_1^2
	56.80	72.32	63.28	55.37	82.06	47.06
P. E. Av.	$\pm .987$	± 1.1	$\pm .807$	$\pm .930$	± 1.28	$\pm .589$

The intervals are not quite the same, but the difference is not such that we can expect much change in the results from this source as compared with Table II. The close correspondence with Table II. is sufficiently evident. Here, as in Table I., $A_2 - A_1$ very nearly equals $A_1^2 - B_1^2$, the differences being 15.52 seconds and 16.22 seconds respectively, showing that the interference effect of A_2 upon A_1^2 is the same as that of A_1 upon A_2 .

For the proper estimation of the results of measurements, we must know the range of variation and the scale of equal values of

¹AMERICAN JOURNAL OF PSYCHOLOGY, Vol. VI. No. 2, p. 273.

the instrument we employ. The shortest record made in experiment II. in the $A_1 B_1$ series is 36.8 and the longest, with this series, that is, in the interference experiment, 101 seconds. The difference of the average of these interference tests, 83.4 seconds, and the last average of $A_1 B_1$, 45.52 seconds, may be called the average range of this experiment. This is not, however, the maximum average range, since the minimum record suggests the possibility of a still lower average, and we have an interference average of 91.6 seconds. The lower limit is fixed by the maximum rapidity of handling the cards and the process of discrimination and choice. The upper limit is not so well defined and probably exceeds the time which would be required if the memory of the positions did not co-operate at all—which would seem a natural limit—since the subject looks in the wrong direction more than he would if the matter were left to chance.

To get some estimate of the value of the divisions of the range of variation, we may ask: What is the variation which equivalent tendencies of association produce above and below sixty-one seconds?—the record made when no previous associations are present. The total practice effect of the first fifty-six associations of the $A_1 B_1$ series reduced the record to 45.52 seconds. From the general course of the curve we can assume with only a small error that the effect of sixty-four associations would have been the same. The total interference effect of sixty-four associations gives an average record of 83.4 seconds. The difference above sixty-one is 22.4 seconds, below 15.52 seconds for these equal forces. To get a comparison of smaller variations, let us assume that the records of the first paper¹ are comparable with those of experiments II. The justification for this is that the stage of general practice is about the same, and especially that the interference with the 120 seconds interval is nearly the same in both cases—namely, 13.57 seconds and 15.63 seconds. The practice effect of eight associations, with a 300 seconds interval, is 8.15 seconds. The interference effect (from Table III. of the above-mentioned paper) is about twelve seconds. The ratio $\frac{8.15}{12}$ is nearly equal to $\frac{15.52}{22.4}$. Accordingly, as measured by the variations in the experiment, the practice effect is two-thirds the interference effect; although, as measured by a method in which there was only a slight change in the records, they are equivalent. The latter is of course the true measure and shows that the variations above are relatively greater for the same associative tendency, in these special cases, in the ratio of three to two. These records are not sufficient to enable us to map out the scale more in detail.

To summarize the results briefly: We have shown that under the simple conditions of this experiment, the interference effect of an association bears a constant relation to the practice effect, and is, in fact, equivalent to it. As regards the nature of the process, it has been shown that we are dealing with the interference of still persisting associations and not with the results of their effacement.

To estimate the results correctly we must refer to the conditions of the experiment. The cards are sorted with the greatest possible speed. Every assistance the memory of the positions can give is utilized. All conditions, except the memory factor, are approximately the same in each test; and the variations in the records are due to the latter. As has been stated before, psychophysics processes, higher than simple successive association, seem to be

¹AMERICAN JOURNAL OF PSYCHOLOGY, Vol. V. p. 361.

excluded. If at the end of the $A_1 A_2$ series of tests M. E. B. tries to recall the positions of both A_1 and A_2 , this can usually be done, though with difficulty. Considerable time would be required to fix the twenty positions in memory, especially since they are contradictory. The associations are here in balance, and in sorting with the greatest possible speed, it is nearly the same as if there had been no previous associations; nevertheless, if the subject takes time to think, every card can usually be placed correctly, from memory. This is another proof that the associations persist and have not been effaced, and also that in sorting with the greatest possible speed, higher processes are excluded, but that such may modify the result greatly if the work is done deliberately.

Since we know what the relation of practice to interference is in a simple case, we have the basis for understanding the changes of the results when other factors enter.

The nervous system has, as is well known, inherited tendencies of growth and adjustment to external circumstances. Perhaps the simplest of these, for the organization of nervous activity, is the tendency of nerve currents to run from one impression to the next succeeding. This is modified in many ways by special tendencies of higher order, which may be classed as fundamental practical adjustments, or practical interests. If these are given an opportunity to influence the results, we are not dealing with associations *per se*, but with these as modified by other more powerful forces. These means of avoiding interference, however, require time and energy. More time is required, otherwise we might expect them to assist us when we act with the greatest possible haste. More energy is required, since the process is more complex and the work of analysis, which is the incorporation of parts of one process in another, is proverbially difficult. There are, besides, great numbers of secondary tendencies by which simple successive association is transformed. Moreover, where the conditions are more complex than in this experiment, certain elements may enter and change the results. A person who speaks several languages finds that the words of the same language tend to be recalled together. Some persons, also, know and can use two different systems of shorthand. In these cases the elements associated are used as members of a group, for the exclusive employment of which there seems to be a strong tendency. The inconvenience of interference is thus to some extent avoided in these cases, but probably only by proportionate expenditure of energy.

The theory as to the character of nervous associations, referred to in the beginning, namely, that a nervous discharge does not radiate into all lines of association, but is wholly appropriated by the association which has a momentary advantage, is evidently not supported by the results of these experiments. The fact that the approximate proportionality of the interference and practice effects of an association can be demonstrated in any case in which simple successive association is utilized to the full extent, leads us to suspect that where facts seem to correspond with this theory, some accessory associations have been developed to bring about the result.

In conclusion I wish to acknowledge my indebtedness to Dr. Sanford for valuable suggestions and to my wife, who has greatly aided me in this study.

PSYCHOLOGICAL LITERATURE.

I.—NEUROLOGY.

By C. F. HODGE, Ph. D.

Brain Surgery. M. ALLEN STARR. New York, 1893; pp. 295, fifty-nine illustrations.

The primary aim of this book, as the title indicates, is to give the surgical status of the subject of brain localization. In addition to this, it may serve to furnish an expression of what has come to be generally accepted in this country in this much controverted subject.

Chapter first treats of "The diagnosis of cerebral disease." Here we learn that "there are certain areas upon the cortex of the brain, not necessarily co-extensive with either lobes or convolutions, whose functions are accurately known." Of these the author would make five, viz.: (1) The sensori-motor area; (2) The speech areas; (3) The visual area; (4) The auditory area; (5) The areas of the sensations of smell and taste. Each of these is briefly outlined with the aid of the clearest possible diagrams. With regard to the "sensori-motor" areas, the view is taken of foci with wide intermerging areas of representation. "Each motion, each part of a limb, has a wide general representation over the cortex and a special representation at a limited area." "The areas of representation of the different limbs merge into one another." "This explains the fact that excision of a small area does not totally paralyze the portion of the limb chiefly represented on that area. The adjacent areas represent to some extent that limb and hence can govern it if need be." The author reasserts his view, as the designation of this region indicates, that there is some loss of tactile sensation as well as paralysis when a lesion of the motor areas occurs. Speech areas, motor, auditory, and visual, are located, in the left hemisphere for right-handed persons, in the posterior part of the third frontal convolution, in the first and second temporal convolutions, and in the lower parietal angular region respectively. The location of graphic speech, the power of writing, is not definitely determined, cases on record pointing to both the second frontal convolution and the lower parietal near the hand centre. In regard to the visual centre the author follows Henschen in placing the primary centre in the calcarine fissure. Audition is located in the first and second temporal convolutions, each ear being connected with both hemispheres, so that total deafness is never caused by unilateral lesion. Taste and smell occupy the tip of the temporal lobe, lower and median surface. Like the centres for hearing, each lobe supplies the sense organs of both sides, so that unilateral lesion rarely produces noticeable symptoms. The

frontal region, the great terra incognita, has been in an uncertain way irresistibly associated with higher psychic functions. The author's experience seems to support this view. Lesions of this region cause no disturbance of motion, of sensation, or of speech. "Yet for the coördination of facts into orderly series, for comparison, and for analysis of knowledge gained through the senses, the healthy state of the frontal lobes appears to be necessary. And lesions in the frontal region, especially upon the left side, are quite uniformly attended by mental dullness, apathy, lack of concentration and imperfect self-control." The functions of the basal ganglia, optic thalami and corpora striata, are still undetermined. If a lesion here does not invade the internal capsule, its presence can not be detected during life. The cerebellum Starr considers as the organ for control of bodily equilibrium.

In Chapter II. pp. 19-113, we have discussed at length trephining for epilepsy. Cases are clearly stated, and supplementing this a microscopical study of two of the cases by Van Gieson is given. The net result of operations reported is ten cases cured; six, improved; eleven, not improved; and two fatal. Trephining for imbecility due to microcephalus, Chap. III., 114-156, is an operation where the surgeon encounters great odds. Out of thirty-four cases, fourteen died, and five showed no improvement; while eight were somewhat, and seven greatly, improved. When gross atrophies are present, surgical interference is of no avail; but when brain tissue has been arrested in its development by cysts, clots, or tumors, or by early union of sutures, the removal of these disturbing conditions by giving more space may result in stimulating growth and in improvement or cure. Chapters follow upon trephining for cerebral hemorrhage, for abscess and for tumor of the brain. In this last field Starr has added to his 300 cases of brain tumor in patients under twenty years, 300 from adults; and his table combining the two lists of cases shows some striking facts touching the relative frequency of various kinds of tumor at different ages. Tumors of tubercular origin are more than three times (152 to 41) as frequent in children as in adults. Also tumors of the cerebellum, pons and medulla are more than twice as numerous in children. Of the whole 600 tumors, for reasons of character and position, only forty-six were clearly open to operation and only thirty-seven, about six per cent., could probably have been successfully removed. Trephining for insanity, Chap. VIII., has proved of service in the rare cases, about two per cent., where insanity has developed immediately after serious injury to the head. It has also been tried, both in this country and in England, for the relief of general paresis, but without effect, or accompanied with only such temporary improvement as may occur in any case of this disease.

Trephining for headache, Chap. XIX., has been successful in two cases, in both of which the disease was of traumatic origin and sharply localized. The book closes with a chapter on the operation of trephining, which is of purely technical interest.

The Sensory Motor Functions of the Central Convolution of the Cortex Cerebri. F. W. MOTT. Brit. Med. J., II. pp. 685-7. 1893.

The point of this research consists in the very satisfactory evidence which it brings to bear upon the sensory functions of the so-called motor areas. The work was done chiefly on monkeys, portions of the brain being removed with a Horsely brain knife. Definite regions were paralyzed in this way and after determining these, sensibility was tested by spring clips of different strengths

plied to the skin. The animals reacted uniformly, always removing the clips from the side unaffected by the lesion in the brain, and a general not noticing them on the paralyzed side. The natural explanation is, of course, that the animal is unconscious of sensation upon that side. A more detailed account of these experiments may be looked for in an early number of the *Journal of Physiology*.

Preliminary Observations on Some Changes Caused in Nervous Tissues by Reagents Commonly Used to Harden Them. HENRY H. DONALDSON. *Jour. of Morphology*, Vol. IX., pp. 123-166. Boston, 1894.

This paper casts a shade of doubt over the records of brain weights as they are usually accepted. Unless it is known exactly how any particular brain has been treated before weighing, the weight as recorded may be anywhere between thirty per cent. too large and thirty per cent. too small. In general, bi-chromate of potash solutions swell, while alcohol has a tendency to shrink the brain, and these processes may even pass beyond the limits indicated above. A large number of experiments were made on sheep's brains in a number of different solutions, and the general reactions recorded. To have the research cover as much of the field as possible, these were then repeated upon sharks' brains and upon a series of human brains. Results were in all cases entirely similar. All possible variations of temperature, strength of solution, manner of cutting the brain, degree of dryness, drainage, age of individual, length of time post mortem, etc., were taken into careful account, so that with these data at hand, it is now possible to correct the weight of any given brain to its original weight when fresh.

In the gross changes, however, we have but a small part of the value of this research. The brain swells or shrinks on account of changes taking place in its tissue elements, the nerve cells. The sizes of these may, therefore, be far from normal, as given by the text-books. For consideration of this side of the subject, we must await a subsequent chapter. The paper should be in the hands of everyone who is contributing to neurological science. We confess to some disappointment in not finding an explicit set of directions for obtaining the most nearly correct weight of the brain possible, the outline of a method which would unify and make comparable the work of different observers. Gathering some such statement from the article, we should say that the brain should be weighed fresh, as it comes from the skull, with pia intact. A note should record whether the olfactory bulbs and pituitary body have been retained, and describe where the division between medulla and spinal cord has been made. The state of the blood vessels should also be described. If immediate weighing is not possible, careful note should be taken of all treatment to which it is subjected up to the time at which it is weighed.

An enormous amount of work has been condensed into little more than twenty pages by stating nearly all the results in tabular form. The tables, forty-eight in number, give the briefest and clearest statement of the case possible, and make the data easily accessible for reference.

Brain Preservation, with a Résumé of Some Old and New Methods. PIERRE A. FISH. *The Wilder Quarter-Century Book*, pp. 385-400, 1 Plate.

This will be found a convenient compendium of some of the better methods of brain preservation, chiefly with reference to

gross specimens. Contributions to the subject have not yet resulted, according to the author, in discovering the ideal method of preparation; but to one who has himself experimented along this line, this will not appear as any unpardonable failure. A number of experiments have been made, and as a result, a new fluid is recommended. Its composition is water and alcohol, each 400 c.c.; glycerine, 250 c.c.; zinc chloride and sodium chloride, 20 gm. each. The specimen, after having its ventricles, and, if possible, its blood vessels, injected with this fluid, is immersed in it for three days, and subsequently in a mixture of equal parts of the fluid and seventy per cent. alcohol for a week or more, when it is finally preserved in ninety per cent. alcohol. Further experiments are under way, which we may hope to hear from later.

The Effect of Stimulation and of Changes in Temperature upon the Irritability and Conductivity of Nerve Fibers. W. H. HOWELL, S. P. BUDGETT AND ED. LEONARD. *Journal of Physiology*, Vol. XVI., pp. 298-319. London, 1894.

The purpose of this research, somewhat different from similar studies in this field in which attention has centred about irritability of nerve fibers at different temperatures, is to discover the relations of temperature to conductivity, with the hope of throwing some light on the nature of the nerve impulse. By the use of both medullated and non-medullated nerves, it was hoped to learn something also in regard to the function of the medullary sheath.

The method consisted in cutting the nerve and laying it in a small brass tube, around which water of any desired temperature was made to circulate. Experiments were made on both frogs and mammals, rabbit, dog and cat. It has been known for some time that nerves may be warmed or cooled within certain limits to a point where they lose their irritability and conductivity, and that, on bringing them back to a temperature between these limits, they may regain both these properties. The principal additions to our knowledge contributed by the present investigation, refer to the order in which conductivity is arrested by cooling in different classes of nerve fibers, and to something unique in nerve-fiber-physiology, viz., a rapid fatigue of certain fibers at the point of stimulation, hence called by the authors, "stimulation fatigue." Since this is a point by itself, it may be considered first. "Stimulation fatigue" was discovered in connection with stimulation of the sciatic nerve of the cat for the study of the effects of cooling on motor fibers, vaso-constrictor fibers, and secretory fibers to the sweat glands of the paw. In every case, whether cold was applied or not, stimulation ceased to have any effect on the constrictor and secretory fibers, and it looked as though a genuine nerve-fiber-fatigue effect had been demonstrated. Later, it was found that this fatigue existed only at the point of stimulation, and that if the electrodes were shifted a mm. down the nerve, no similar fatigue of motor fibers was present, and since these are medullated and the vascular and secretory are non-medullated, it seemed to point to functional difference due to the medullary sheath. To test this point, stimulation was applied to the rami between the spinal cord and sympathetic ganglia, where both secretory and vaso-constrictor nerves are supposed to possess medullary sheaths. Here the results crossed, the vaso-constrictor fibers being not subject to stimulation fatigue, as predicted, while the secretory fibers were fatigued as before. This breaks down the interesting generalization and leads to the conclusion that the phenomenon of stimula-

tion fatigue must be due to differences in the conducting portion of the fibers, rather than in any difference in their sheaths.

Several other points of interest may be abbreviated from the conclusions as follows: 1. Cooling may be conveniently used to block the nerve impulse where it is desirable to suspend conductivity without injury to the nerve. 2. The temperature at which conductivity is suspended varies somewhat in different fibers, lying between 5 and 0 C. The cardiac inhibitory fibers of the rabbit offer an exception to both of the above rules in not regaining conductivity well and in losing it at 15 C. 3. "A nerve impulse in passing into a stretch of fiber of different temperature may suffer an increase or a diminution in force, according as the temperature of this portion of the nerve is above or below that in which the impulse originated." The force of the impulse is increased by heat and diminished by cold. 4. The method of cooling may be used to differentiate the physiological varieties of nerve fibers combined in a common trunk, viz., to separate vaso-constrictors from vaso-dilators in the same trunk, inhibitory and augmentory fibers in the vagus, etc.

A Microscopical Study of Changes Due to Functional Activity in Nerve Cells. C. F. HODGE. *Journal of Morphology.* Vol. VII. pp. 95-168. Plates VII. and VIII. 1892.

The earlier experiments in this research were first reported in this JOURNAL for 1888, '89 and '91, and dealt respectively with the changes produced in spinal ganglion cells by electrical stimulation and with the process of recovery from fatigue thus produced. It is unnecessary to recapitulate the results of these experiments further than to remind the reader that the nucleus became smaller, irregular in outline and stained darker as stimulation was continued, and the cell protoplasm became more or less vacuolated according to the degree of fatigue induced.

The point in which the present paper forms an advance is in a study by similar methods of effects in the nerve cells of normal daily activity, and it is intended in this review to cover this last section of the work.

The experiments were made by taking the animals, English sparrow, pigeon, swallow and honey bee, at the beginning and end of their day's work. The above animals were chosen because of their constant and well defined rhythm of diurnal activity. Similar preparations of the cerebrum, cerebellum and spinal ganglia were compared in six pairs, morning and night, of birds, and the cerebral ganglia of ten couples of bees each morning and night. The result, which is of greatest interest to psychologists, is that a greater degree of fatigue-change is often produced by ordinary daily work than can be obtained by electrical stimulation. Sets of cells were measured as in the former experiments, and the nuclei were found in all cases smaller in the evening specimens. This difference in the spinal ganglion cells of the birds amounted to from thirty-three to sixty-four per cent., showing an average for the birds of forty-eight and two-tenths per cent. The nuclei of the cells of the occipital cortex showed a slightly greater difference, thirty-six to sixty-nine and seven-tenths per cent., with an average loss of fifty-one and five-tenths per cent. In the honey bee experiments, the nuclei of the antennary lobes were measured and showed a shrinkage in volume of from nine to seventy-five per cent. The spinal ganglia of two foxes were also examined and only a moderate degree of change was demonstrated. As the carcasses after skinning could not be identified, the amount of fatigue, or the length of

chase, could not be determined. In the motor cells of the spinal cord of a patient dying of hydrophobia, the nuclei were found to be much shrunken, being nine per cent. smaller than corresponding nuclei in a so-called normal human cord. It is not strange that we should find a greater amount of change in daily fatigue than in artificial stimulation. It is not possible to obtain secretion of a gland or contraction of a muscle by application of electrical stimuli equal to that produced by the normal nerve impulse to gland or muscle. Two plates containing thirteen figures give the entire research at a glance.

The Formation of the Medullary Groove and Some Other Features of Embryonic Development in the Elasmobranchs. WILLIAM A. LOCY. Jour. Morph., Vol. VIII. 1893.

The Optic Vesicles of Elasmobranchs and their Serial Relation to Other Structures on the Cephalic Plate. Ibid., Vol. IX., pp. 115-122. 1894.

Metameric Segmentation in the Medullary Folds and Embryonic Rim. (Prelim. Comm.) Anatomischer Anzeiger, IX., pp. 393-415. 1894.

Locy states at the close of the last paper that he is not ready, as yet, to generalize upon the segmentation of the vertebrate nervous system. However, the facts which he brings together point strongly toward a helpful generalization in the near future. It has come to be quite generally held by morphologists that it is the mesoblast which becomes segmented primarily, and that segmentation of the neural tube is moulded by these bone and muscle somites and accommodates itself to them. Contrary to this view, in embryonic stages much too young to show any trace of mesoblastic somites, Locy finds a perfectly regular, symmetrical and constant segmentation of the neural plate. He has succeeded, in one of the sharks, in tracing out this segmentation in a consecutive and orderly way, and has also succeeded in demonstrating it in early embryos of *Amblystoma*, *Diemyctylus* and *Torpedo Ocellata*. In all Locy finds eleven metameres in the expanded portion of the neural plate which represents the brain. These are distributed to the three primitive cerebral vesicles as follows: six for the third vesicle, two for the second, and three for the first. Later all traces of this segmentation become masked by the development of special structures throughout this entire region. The fact of such a segmentation appearing so early should be given prominence in working out the ancestry of vertebrates. At first all the metameres are alike, which would indicate an ancestral form of this character, i. e., without differentiation in the neural tube. As cephalization advances, differentiation takes place, and it is in this that the primitive segmentation is lost.

This brings us to the first pages of our author. The first structures to make their appearance in the segmented neural plate are the pits which represent the optic vesicles. Just behind these appear a second pair of depressions, the so-called accessory optic vesicles, which give rise later to the pineal gland. Still a third pair of pits may be observed behind the second, but these early become obscured. This series of depressions is taken to represent a multiple eyed condition, common enough in invertebrates, but not known in any vertebrate, and this, too, is of significance in a search after the ancestral form. The arrangement of these optic vesicles in a laterally symmetrical series inclines the author to the view that the eye may be homologized with the sense organs which spring from the lateral line.

The Epiphysis of Telosts and Amia. CHARLES HILL. Jour. Morph. Vol. IX., pp. 237-266. Pls. XII. and XIII. 1894.

The special significance of this paper consists in its filling a gap in our knowledge of the pineal structures in the bony fishes. In connection also with Locy's observations on Elasmobranchs, it is of interest to note that Hill finds two independent outgrowths arising from the roof of the mid brain in the telosts examined and in amia. These together form the epiphysis, but each vesicle remains distinct. The author considers it probable also that in their primitive position they were side by side and subsequently become crowded into the median line by the growth of the hemispheres. The plates, twenty-two figures, give the clearest possible account of the subject.

Comparative Study of the Epiphysis and Roof of the Diencephalon. A. D. SORENSON. Jour. Comp. Neurol., Vol. IV., pp. 12-72. 1894.

This paper gives the literature of the pineal region more in extenso than any that we have. Each author is treated separately and the main points have been gathered together, summaries copied, etc., so as to give the organ as it has been described in the different classes of vertebrates.

Treatment and Prophylaxis of Insanity. JOHN PUNTON, M. D. Alienist and Neurologist, Vol. XV., pp. 52-66. St. Louis, 1894.

The particular form of mental disease treated by our author is melancholia, the most common and most easily managed of insanities. It may arise as a congenital neurosis or be acquired. Primarily, according to Meynert and Clouston, melancholia arises from trophic disturbances, malnutrition, of the cortex. This theory the author permits to dominate his treatment. Insomnia is a frequent symptom, but drugs which have a tendency to interfere with nutrition, opium and the bromides, are contra-indicated. Sulphonal is the best remedy to apply in these cases. Everything must be done to bring up body weight, generally deficient in melancholiacs, and force nutritive processes to the utmost. For this purpose quinine, strychnia, phosphorus, arsenic, cod-liver oil, mineral acids, vegetable bitters, hypophosphates, et al., and especially foods, milk and eggs: "Three quarts of milk a day and six eggs for months." Yet any treatment will fail without daily exercise in the open air. The chief aim of the paper is to direct the attention of the medical profession to the importance of preventive measures while there is possibility of cure. If this in the case of congenital defects be begun at birth and even before, and continued through nursery and school life, tendencies of this character may be eradicated. An index of especial value in cases tending toward melancholia is body weight. Patients should weigh at least once a month, and if any loss is detected, they should adopt measures immediately to make it up and keep it up.

The Value of Sugar, and the Effect of Smoking on Muscular Work. VAUGHAN HARLEY. Journal of Physiology, Vol. XVI., pp. 97-122, London, 1894.

The experiments were made in Mosso's laboratory with the ergograph. Nasse, Brücke and Weiss have shown that glycogen in the muscles decreases in amount during activity and accumulates during rest. And more recent researches of Chauveau and Kaufmann have demonstrated that sugar in the blood disappears much more rapidly when circulating in an active than in a resting muscle.

With this as a basis, the author attempts to decide the practical value of sugar as an energy-producing food. He finds in using the ergograph that he fatigues much as Lombard does, viz., with periods of partial recovery. He can thus use the point of first fatigue as a measure of the condition of his muscles. As another measure he uses the total amount of work accomplished by the first thirty contractions. Diet was carefully regulated, and the experiments were repeated daily at the same hours. As in all former work in this line, practice was found to increase strength and regular diurnal variations occur. The total amount of work on a sugar diet is almost equal to that on a full diet, although fatigue occurs sooner. Days of fast compared with days on which 500 gms. of sugar were taken, showed an increase in amount of work for the sugar days of from sixty-one to seventy-six per cent. The effect of sugar added to a light meal increases work from six to thirty-nine per cent.; and even when added to a heavy meal, gives an increase of from two to seven per cent. Smoking has a much slighter effect on Harley than on Lombard, causing a diminution in work scarcely demonstrable.

Neue Darstellung vom histologischen Bau des Centralnervensystems.
 RAMON Y CAJAL. Archiv für Anatomie und Entwicklungsgeschichte, pp. 319-428, 35 Figures in text. Leipzig, 1893.

In the above form we are presented with a translation from the Spanish, to which numerous revisions and additions, both in text and figures, have been made by Cajal himself. It furnishes a clear connected statement of this successful investigator's views and researches up to date, and in addition to this a running critical review of the work of others. Specifying, in a single instance, we note that Cajal denominates Dogiel's position as to anastomosis of cell processes as heretical, "ketzerisch." But the good reasons which he is able to advance remove from the sentence all taint of the Spanish inquisition. The spinal cord, cerebellum, cerebrum, different regions, retina and olfactory bulb are treated in order. No brief review of this important paper can be attempted.

Report in Pathology (upon gross and microscopical lesions found in thirty-eight cases, autopsies of insane patients). T. P. PROUT. Annual Reports of the New Jersey State Hospitals, 1893, pp. 99-117.

Dr. Prout is resident pathologist in the Morris Plains Asylum, and is to be commended upon the industry and patience with which he has dealt with his difficult problems. Gross lesions, either in the brain or its membranes and blood supply, are demonstrable in all cases. Among these, thinning of cortex and atrophy of convolutions with diminished or increased consistency of brain substance, anæmia, and oedematous and opaque condition of the pia, are especially frequent. Nearly all the cases were chronic, over half being terminal dementia (thirteen) and general paralysis (seven). Concerning his microscopical findings, Dr. Prout says: "The changes in the cells in all these cases were, it seems to me, ample to account for the mental disturbance manifested, provided we may consider the cortical cell the seat of mental activity, a point quite generally conceded." Degenerations of all sorts, fatty, pigmentary and granular, vacuolation of cell protoplasm and especially of nucleus, with fragmentation of the nucleus, are described in some detail and are figured. It is probably to be put to the account of Dr. Prout's equipment that no brain weights appear

in his excellent tabular statements of cases. With the apparatus supplied to state institutions, it is useless to insist that *weight of body, weight of brain and stature* should be given in case of every autopsy, but no really available data can be obtained for this country until this is done. For anyone who may wish to repeat and confirm Prout's observations, more exact statement as to the location examined will be necessary, and some indication of the methods employed. Definiteness upon these points would have added greatly to the practical value of the work.

Der Einfluss des Trigeminus auf die Hornhaut. J. GAULE. Centralblatt f. Physiol., Bd. V., pp. 409-15. 1892.

Wie beherrscht der Trigeminus die Ernährung der Hornhaut. Ibid., pp. 450-56.

Spinalganglien und Haut. Ibid., Bd. V., pp. 689-97.

Spinalganglien des Kaninchens. Ibid., Bd. VI., pp. 313-26.

Weitere Experimente an den Spinalganglien und hinteren Wurzeln. Ibid., Bd. VI., pp. 785-802. 1893.

Die trophischen Veränderungen und die Muskelzerreissungen. Ibid., Bd. VII., pp. 646-54. 1894.

Die trophischen Eigenschaften der Nerven. Berliner klin. Wochenschr., Vol. XXX., pp. 1065-68 and 1099-1102.

Zur Frage über die trophischen Functionen des Trigeminus. C. ECHARD. Centralbl. f. Physiol., Bd. VI., pp. 328-32. 1892.

Ueber das Vorkommen von Muskelzerreissungen an gefesselten Kaninchen. H. E. HERING. Ibid., Bd. VII., No. 18.

Erwiderung auf Herrn Prof. Gaule's Bemerkungen über die bei gefesselten Kaninchen vorkommenden Muskelzerreissungen. H. E. HERING. Ibid., Bd. VIII., pp. 854-7.

The above group of papers may be cited as an interesting contribution to the subject of the trophic influence of the nervous system. For several years Gaule has been working to bring some physiological explanation to bear upon the array of facts, like that of herpes zoster, decubitus, atrophy of glands and muscles, after their nerves are severed, and many others, which seem to indicate clearly a trophic action of the nerves. He begins with the cornea, where effects may be most clearly observed, and performing Majendie's experiment on the fifth nerve and Gasserian ganglion, makes out definite changes in the cornea, drying and necrosis of the epithelium cells, wholly dependent on the operation and which cannot be thwarted by any possible protection of the surface. The fifth nerve was cut in different experiments at three points, through the Gasserian ganglion, between pons and ganglion and between cornea and ganglion. The result upon the cornea did not follow when the cut was made between pons and ganglion, and this at once proves that insensibility of the cornea cannot be the cause of its becoming dry, and in consequence, necrotic; but that necrosis of the cells is the cause of its drying, and hence that the cells of the Gasserian ganglion are true trophic centers for the cornea. Thus the usual explanation is exactly reversed. Similar observations were made upon the skin in frogs and rabbits, the hair interfering with entirely satisfactory study of the latter. In the frogs, destruction of the spinal ganglia was found to cause changes com-

parable with those described for the cornea, with certain differences in the dorsal and ventral regions. These consisted in a drying of the dorsal parts and a corresponding swelling with turgescence of the ventral. Beside changes in skin and cornea, injury to the spinal ganglia produces most unaccountable alterations in many of the deeper organs. The adrenal bodies show constant change, and the reproductive gland of the opposite side becomes shrunken and of especial interest; the muscles become the seat of active changes. It is to these that the author devotes chief attention in his later papers. They consist in hemorrhages, varying in size from that of a ten cent piece or larger to that of a millet seed, into the substance of the muscle or into the connective tissue septa. At first they seemed to occur throughout the entire muscular system, in greater or less numbers, out of all observable connection with known anatomical relations, "ganz unberechenbar" as to location. Later Gaule succeeded in demonstrating certain anatomical paths for two muscles, the biceps and psoas. The effect could be made to appear in the muscles by a number of operations upon the spinal ganglia, by stimulating with the interruption of a strong (four to five Daniel cells) constant current (an induction current was not effectual), and by cutting or cauterizing the ganglion. These must all be done without previously opening the sac or disturbing the blood supply of the ganglion.

Echard maintains that such flaws in the cornea, as Gaule describes in his first papers, are readily produced in rabbits by a little rough handling, a little dust, hair chippings and the like getting into the eyes, even if no operation whatever is made, or if the fifth nerve be cut close to the pons. Gaule answers this criticism by calling attention to the fact that the root of the fifth nerve contains many ganglion cells at its emergence from the pons. He also brings forward an experiment of crucial importance. If the Gasserian ganglion be cut upon one side and the root of the fifth nerve be cut upon the other side above the ganglion, where no cells occur, the cornea will degenerate upon the side of injury to the ganglion, and this cannot be prevented by care and protection, whereas the cornea on the other side remains normal. Here we have a satisfactory answer given to the old argument against trophic influence of nerves based upon insensibility and consequent neglect of parts concerned, both corneæ being, in this case, equally insensible.

Hering's work, done and reported apparently without reference to Gaule's experiments, goes to prove that hemorrhages in the muscles may be produced in rabbits by their voluntary efforts to escape, when simply tied in the manner usual for operation. These hemorrhages closely resemble those described by Gaule as trophic, and occur most frequently in the biceps and psoas, exactly the muscles most clearly affected in Gaule's experiments.

This brings us to a consideration of Gaule's work upon the sympathetic ganglia. He found that if he stimulated or injured the superior sympathetic ganglion, changes invariably took place in the biceps and psoas muscles. The path of this influence, according to the author's view, must lie in the spinal cord and must reach this through the ramus communicans. From stimulation upon one side, the muscles of both sides are affected. Further, a peculiar complication exists in the fact that one branch of the superior cervical ganglion acts to inhibit all trophic influence of the ganglion upon the above named muscles. If this twig is stimulated, stimulation of the ganglion at the same time produces no effect. So definite is the reaction that Gaule is able to lay bare the living biceps and upon stimulation of the ganglion, actually observe the

whole process. As stimulation of the ganglion proceeds, the muscle, at about the middle, near the entrance of the nerve, first becomes rough and irregularly contracted, then cedematous. The muscle fibers apparently dissolve and tear apart under the strain of normal tonus; the capillaries, unable to support the strain, are torn, thus producing the hemorrhage, and in from three to ten minutes may be seen the formation of a typical ulcer. After the operation this heals slowly, with formation of a scar.

Thus Gaule is able to prove, as he thinks, the absence of all connection between his experiments and those of Hering. It is further stated that the rabbits experimented on are not tied down, but held by the hand, and that rupture of the muscle fibers may occur without perceptible voluntary contractions. The essential fact, upon which Gaule insists, is that trophic changes may be produced in muscle substance by which its power to resist strain is greatly diminished. He experiments further to prove this by extending similar muscles with different weights. Normally, a weight of 5,000 gms. does not cause rupture of muscle fibers, while 500 gms. does so invariably, if the spinal ganglia are injured. Thus resistance is lessened to at least one-tenth of normal.

We can no longer, according to Gaule, speak of "trophic nerves." The whole nervous system is trophic.

II.—ANTHROPOLOGICAL PSYCHOLOGY.

A. F. CHAMBERLAIN, PH. D.

LINGUISTICS (and related subjects).

Further Notes on Indian Child-Language. A. F. CHAMBERLAIN. *Amer. Anthropol.* (Washington), VI. (1893), 321-322.

Notes that it is the mothers who teach the children many of these words.

Secret Language of Children. O. CHRISMAN. *Science* (New York), XXII. (1893), 303-305.

Treats of the secret language in use among school-children and others, and contains original observations on the Tut-language, as spoken by the children in Gonzales, Texas.

On the Words "Anahuac" and "Nahuatl." D. G. BRINTON. *Amer. Antiq.* (Chicago), XV. (1893), 377-382.

An interesting onomatological study. The author brings out the curious fact that in the Zapotec Indian tongue of Mexico (as also in Huastec), "the verb 'to know' is a reduplication of the first person of the personal pronoun *na*, 'I'; *na-na*, 'to know,' literally, 'my mine,' that which is with me, essentially mine."

The Origin of Literary Forms. C. LETOURNEAU. *Pop. Sci. Mo.* (New York), XLIII. (1893), 673-682.

Treats of the forerunners and the beginnings of literary æsthetics and their progress through the various periods of human development. Refers to the Fuegians, ancient Greeks and Latins, Chinese. The author considers "that there are good grounds for supposing that women may have especially participated in the creation of this lyric of the erotic kind."

A New Method of Teaching Language. W. VIETOR. *Educ. Rev.* (New York), VI. (1893), 350-359.

In this paper Professor Vietor, of the University of Marburg, gives an account of the new or reform method of direct or imitative language teaching as practiced in many of the German secondary schools, with a brief sketch of a reform lesson.

Reform in Modern Language Instruction. J. J. FINDLAY. *Ibid.*, V. 334-344.

Discusses Gouin's system chiefly.

Relations of Literature and Philology. O. F. EMERSON. *Ibid.*, V. (1892-'3), 130-141.

Argues for the complete separation of the study of two such diverse subjects as in accord with the specializing tendencies of the age, and with the necessities of academic progress.

La Degenerazione nello stile dei paranoici erotici. G. C. FERRARI. *Rivista Sperim. di Fren. e di Med. Leg. ecc.* (Reggio), Vol. XIX. (1893), 329-363.

Follows up the study of Tanzi, who sought to show the analogies between the writings of paranoics and those of primitive peoples. A detailed account of observed "degeneration in the style of paranoics," illustrated by numerous citations in prose and in verse, from the original documents. The anomalies are considered atavistic returns due to the fundamentally degenerated condition of the patient.

Ueber eine Depressionsform der Intelligenz in sprachlicher Beziehung. M. O. FRAENKEL. *Ztschr. f. Psych. u. Phys. d. Sinnesorgane* (Hamburg-Leipzig), VI. (1893), 230-232.

Treats of peculiarities (the use of weak forms of strong verbs, *gehen* for *gegangen*, *mir* for *ich*, and *vice versa*, etc.) observed in the speech of a twenty-three year old (seemingly idiotic) woman. Notes the fact that in French, which she speaks like her mother tongue, the same mistakes are not made.

The Language Used to Domestic Animals. W. H. CARRUTH. *Dialect Notes* (Cambridge, Mass.), Part VI. (1893), 263-268.

Discusses the peculiar words in calling, driving away, etc., cows, swine, horses, sheep, dogs, fowls, in various districts of the United States, giving phonetic transcriptions of each sound noticed. An interesting beginning of an important study.

On the Source of the Italian and English Idioms, Meaning "To Take Time by the Forelock," with Special Reference to Bojardo's Orlando Innamorato. Book II. Cantos VII.-IX. G. E. MATZKE. *Public. Mod. Lang. Ass. Amer.* (Baltimore), VIII. N. S. I. (1893), 303-304.

An interesting psycho-linguistic study. The paper is supplemented by others by Professor G. L. Kittredge, *Mod. Lang. Notes* (Baltimore), VIII. (1893), 459-469; and Dr. K. Pietsch, *Ibid.*, 469-475.

The Order of Words in Anglo-Saxon Prose. C. A. SMITH. *Public. Mod. Lang. Ass. Amer.* (1893), 210-244.

A timely consideration of a much neglected subject. The author discusses the influence of Latin, and later of Norman-French, inversion, transposition, etc. His conclusion is, "The leading dif-

ference between the word order in Anglo-Saxon and that in Middle English or Modern English, is found in the frequent transposition occurring in Anglo-Saxon dependent clauses," and the introduction of Norman-French only consummated the influences at work to produce this.

The Absolute Participle in Middle and Modern English. C. H. Ross. *Ibid.*, 245-302.

This is a thorough-going and statistical study, and the author's statement that in Modern English the absolute participle "is an important adjunct to the style, to which it imparts variety and compactness. It gives life and movement to the sentence, and is the ready resource of all writers of narration and description for the purpose of expressing subordinate conceptions," ought to lead to the revision of certain text-books of rhetoric and composition. Mr. Ross' part is based upon the examination of twenty-nine Middle English, seven Old French, one Italian, and sixty-one Modern English texts.

A Grouping of Figures of Speech, Based upon the Principle of their Effectiveness. H. E. GREENE. *Ibid.*, 432-450.

The author's order is: "Synecdoche, Metonymy, Stated Simile, Implied Simile, Metaphor, Personification, Imperfect Allegory, Pure Allegory. Kenning, which points sometimes toward Metonymy, sometimes towards Metaphor, I place between Metonymy and Metaphor." Professor Greene remarks that "since the publication nearly forty years ago of 'The Philosophy of Style' by Herbert Spencer, there has been a gradual consensus of opinion in favor of the view which he advanced—that the aim of all rhetorical devices is economy of the attention of the reader or hearer."

Polysynthesis in the Languages of the American Indians. J. N. B. HEWITT AND J. OWEN DORSEY. *Amer. Anthropol.* (Washington), VI. (1893), 381-407.

This is an examination by Mr. Hewitt upon the basis of Iroquoian and Dr. Dorsey upon the basis of Siouan speech of the "polysynthesis," held by many authorities to be characteristic of American tongues and in criticism of views expressed by Dr. D. G. Brinton, upon this question of great psychological import in linguistic study. Of the American Indian tongues it may be said: "They, like the languages of the older hemisphere, have traits which are found in the majority of languages, and they also individually have others which are idiomatic." Nay, more, what Professor Whitney has written about holophrasis in view of the languages of the Indo-European family, applies with equal force to the languages of the American aborigines, the word-sentences of which are the same in kind with those of the former.

On Adaptation of Suffixes in Congeneric Classes of Substantives. M. BLOOMFIELD. *Amer. Jour. of Philol.* (Baltimore), XII. (1891), 1-29.

In this excellent paper, Professor Bloomfield treats a subject of great psychological interest and one with which the students of the languages of our American aborigines are tolerably familiar. The author, however, confines his field of view to Indo-European speech, treating of the suffixes occurring in the names of the body and its members, words for right and left, assimilation of opposites

and assimilation of congeners, suffixes in names of birds, animals and plants in Greek, suffixes in names of divisions of time, etc. The paper is a distinct contribution to linguistic psychology.

The Spelling Reform (Bureau of Education, Circular of Information, No. 2, 1895). F. A. MARCH. (Washington), (1895), 96 pp. 8vo.

This is a revised and enlarged issue of the pamphlet published by the United States Bureau of Education in 1881 and exhibits the progress of the movement for the reform of our absurd system of spelling since that date, progress that must encourage the distinguished advocates of the innovation.

A propos de la réforme orthographique. CH. LERAIGUE. *Revue Pédagogique* (Paris), Tome XXII. (1893), 213-232.

Discusses the report of the Dictionary Committee of the French Academy. The author finds fault with the committee in one point only—it has favored the hyphen too much.

Une Objection contre la Réforme de l'Orthographe: Étymologie. A. BERNARD. *Ibid.*, XXIII. (1893), 36-43.

The author holds that etymology has nothing to do in principle with orthography, and supports his views by numerous well-chosen illustrations.

Simplified Spelling; A Symposium. *Amer. Anthropol.* (Washington), VI. (1893), 137-206.

In this symposium on the question: "Is simplified spelling feasible as proposed by the English and American philological societies?" the following distinguished gentlemen took part: Professor F. A. March, Dr. W. T. Harris, Dr. Alexander Melville Bell, J. M. Gregory, W. B. Owen, E. T. Peters, C. P. G. Scott, J. C. Pilling, A. R. Spofford, B. E. Smith, Professor W. D. Whitney, Major J. W. Powell. The chief who answered the question in the negative, was Dr. Spofford, who said, "The cardinal error of the spelling reformers is: They forget that the language of a people is an evolution, not a creation." Perhaps the most interesting contribution to the symposium is that of Mr. J. C. Pilling, who gives (pp. 182-186) a brief account of the syllabaries used by missionaries and others to record and to teach Indian languages.

The Native Calendar of Central America and Mexico; A Study in Linguistics and Symbolism. D. G. BRINTON. (Philadelphia), (1893), 59 pp. 8vo.

This new and extended study of the calendar-systems of Central American and Mexican aborigines contains much of psychological interest. The mathematical basis, the day-periods, the solar year, divination by the calendar, and calendar festivals are briefly referred to, but the main portion of the work (pp. 20-49) is devoted to acute analysis of the day and month names, with a discussion of their symbolism (pp. 49-59), and hieratic significations. According to Dr. Brinton, the philosophic conception, which, without any straining, was intended to be conveyed by the calendar, covering the career of human life from birth till death in old age, was this: "The individual emerges from the womb of his mother, and the parturient waters, as did the earth from the primeval ocean; he receives breath and with it life, which is supported by repose and food. The man reproduces his kind; the woman, at the risk of

death, brings her child into the world. The chase and tilling of the ground are the leading occupations of peace, and he who holds firm through illness, suffering and hardships will gain the prize of life. Having reached the acme of his career, the decline commences. Losses multiply, years increase, and though knowledge and wisdom are augmented, old age comes on apace with failing powers, with vanquished struggles, with sickness and death; until, at last, its course run, its task completed, the soul quits the worn-out body and soars to its natural haven and home, the abode of the sun."

Speech Tones. ALEXANDER MELVILLE BELL. (Washington), (1893), pp. 18.

A paper read before the Modern Language Association of America, December 27, 1893, and devoted to the consideration of tones, accent, characteristic national tones of speech.

Internal Speech and Song. J. M. BALDWIN. *Philos. Rev.* (Boston, New York, Chicago), II. (1893), 385-407.

Treats of "words in our mind," "tunes in our heads," pitch-recognition, etc. A useful contribution to the literature of the psychology of language. Professor Baldwin makes some new suggestions.

Studies of Animal Speech. E. P. EVANS. *Pop. Sci. Mo.* (New York), XLIII. (1893), 433-437.

Discusses the investigations and statements of Wenzel (1808), Hanikl (1830-40), Richard (1857), Radeau (1869), Nicaise and Garner.

The Speech of Animals. H. N. LYON. *Science* (New York), XXII. (1893), 324.

The author describes the "efforts at communication" made by a pet fox squirrel.

Report of Committee on Classification of Methods of Instructing the Deaf. *Amer. Ann. of the Deaf*, XXXVIII. (1893), 291-414.

An exhaustive treatment by specialists of the question of the instruction of the deaf; treats of sign-language and other matters of psychological and linguistic import, besides the various other methods of language-instruction.

Speech for Deaf Children. L. E. LITTLETON. *Pop. Sci. Mo.* (New York), XLIV. (1893), 363-372.

A general discussion of the subject.

Manual of Linguistics. A concise account of general and English phonology, with supplementary chapters on kindred topics. By JOHN CLARK. New York, G. P. Putnam's Sons, 1893, pp. 318.

This is not a purely psychological book, but will interest and instruct all interested in the psychology of speech. The author is well read, and writes on the origin of speech and of letters, or sound relations, semi-vowels, spirants, consonants and explosives, analogy, ablaut and accent, Grimm's law, etc., with a wide view of what has been done. The author builds mainly on Brugmann and Sweet, and regards phonology as a very exact science. The horizon is Indo-European. Speech was polyphyletic and not monophyletic, and gesture, and festal excitement aided; imitation may have

been the germ of all. The primitive Aryan talked not roots or sound-norms, but sentence-words; "Speech craft is not yet dead;" the great epoch were metals and smith-lore. Phonic law is universal and suggests lines of research; analogy is constructive and a psychological factor, while phonic law is physiological, so that they complement each other. These are some of the *dictata* that may suggest the quality of the book.

The Early Spread of Religious Ideas, Especially in the Far East.
JOSEPH EDKINS. F. H. Revell Co.

"Let us continue to follow the New Testament, the Samaritan Codex, and the Septuagint, in regarding the Book of Genesis as pre-Mosaic and compiled from documents in the age of Moses, under his direction." "But invaluable as it is, the Book of Genesis is in the early part so brief and incomplete that we are obliged to look beyond it for information, and must make search in the Zendavesta, the Vedas, the Chinese sacred books, and in the Buddhist Sutras. If God spake to man by Adam, Enos, Enoch and Noah, as Christians believe He did, the truths and duties He taught must exist in some form in the literature of their descendants, among whom are the nations which possess these sacred books." "The most suitable theory for the investigator to hold is that which represents the early monotheism of Asia, wherever found, in China or in Persia or in western Asia, as resulting from divine revelation aiding the human faculties of conspicuous men." "The revelations made to Moses and the prophets were specially entrusted to the care of the Jews. The monotheisms of China and Persia are a survival of the revelation made to Enoch, Noah and other primeval patriarchs." "Emigrants, when they passed beyond the reach of inspired patriarchs, would easily forget a part and alter another part of the truths taught them in the first ages." "The world was peopled twice. . . . The traditions of the Chinese classics were to a large extent ante-diluvian. The traditions of Taoist and Buddhist books are post-diluvian." "Why should we necessarily have mythological periods in history?" "Mythology is a morbid growth from philosophy." "This conception of God (the idea of a Trinity) . . . belongs to the last revelation, long ago made in the plains of Babylonia." "Hence it appears to be perfectly safe teaching that divine light granted to Mesopotamia, as the Book of Genesis tells us, was certainly conveyed in radiating lines from the primeval home of mankind to the farthest borders of Asia, and beyond the sea to America. It seems, then, to be right to represent whatever religious and moral truth we find believed in by the nations of the far east as having been conveyed to them from the earliest home of mankind, and as being derived at first from divine revelation." F. D.

Die Medecin der Naturvölker. DR. MAX BARTELS. *Ethnologische Beiträge zur Urgeschichte der Medecin.* Leipzig, 1893, pp. 361, 8vo.

In this work Dr. Bartels has done for medicine what Ploss did for the history of woman and the child. All the important aspects of primitive medicine, — disease, physician, diagnosis, medicaments, water-cure, massage, relation of the sick and the well, sympathetic treatment, knowledge and diagnosis of special diseases, special pathology and therapeutics, epidemics, surgery, etc., are dealt with, and the extensive bibliography appended shows that the author has familiarized himself with the more recent researches in

America and elsewhere. Most interesting are the chapters on the "doctors," "medicine-men," "shamans," of various peoples, their status, rights and privileges, training, etc., and the section on diseases ascribed to supernatural causes. Particularly noticeable is the wide-spread belief in the evil nature of the spirits of women who have died unmarried, or in child-bed, these in many cases being regarded as very inimical to young children. Among many tribes the position of shaman is hereditary and it is often held by women; in some parts of Africa twins are usually trained to practice the healing art. In South Australia, a young man becomes a doctor "by seeing the devil," i. e., a spirit imparts to him in a dream the requisite knowledge and authority. Of "heroic treatment," many examples are cited from all over the world. The book is one which will interest alike the psychologist and the student of the history of medicine.

Primitive Music. An Inquiry into the Origin and Development of Music, Songs, Instruments, Dances and Pantomimes of Savage Races. With Musical Examples. R. WALLASCHEK. London, 1893, XI. pp. 326+8, 8vo.

This timely, most interesting and valuable work is another evidence of the importance of ethnology for the science of art and the psychology of æsthetics. As Roskoff showed that even the most barbarous peoples had some germs of religious belief, so Wallaschek declares that "however far we might descend in the order of primitive people, we should probably find no race which did not exhibit, at least, some trace of musical aptitude, and sufficient understanding to turn it to account." The author treats of: General character of the music of primitive races; singers and composers in primitive times; instruments; the basis of our musical system; physical and psychical influence of music; text and music; dance and music; primitive drama and pantomime; origin of music; heredity and development. Following are some of Wallaschek's conclusions: "It is quite as difficult a matter to scientifically establish a pure musical type as the purity of a race" (p. 65). "The hypothesis, however, that savage races had female voices, would be entirely unfounded, although some singular exceptions (Australia, China, etc.) speak in favor of it at first sight" (p. 77). "It is with the vocal organs as with those of hearing; there has been practically no change in historic times at any rate, although to-day we may, perhaps, be better able to realize their capabilities than heretofore" (p. 79). "The difference between people with and without harmonic music is not a historical, but a racial one" (p. 144). "Among savages the influence of music is far more distinctly noticeable than among people in a higher state of civilization" (p. 163),—the power of music as solace and curative in affliction and disease is widely recognized. "In the relation of text and music there seems to have been little change from time immemorial" (p. 171). Wallaschek's theory of the origin of music is as follows: "From the character of primitive music, as exhibited by the musical practices of savages, I venture to conclude that the origin of music is to be sought in a general desire for rhythmical exercise, and that the 'time-sense' is the psychical source from which it arises" (p. 294). He rejects Spencer's theory of its origin from the "natural melody of emotional speech," as well as the "bird-song" theory. With reference to heredity and development, the author declares: "I consider it downright impossible that heredity of acquired modifications contributes anything to the development of

music" (p. 268). A bibliography of several hundred titles adds to the usefulness of a book which ought to be in the hands of every student of art and man.

Die Urgeschichte der Familie von Standpunkte der Entwicklungslehre.
VON WAGNER. Biologisches Centralblatt, XIV. Bd. (1894),
s. 65-71.

This is a review of a special chapter in the recent and important work of H. E. Ziegler, "Die Naturwissenschaft und die sozialdemokratische Theorie, etc. (Stuttgart, 1894),"—a work which may be styled "Principles of Sociology upon the Basis of Natural Science." Ziegler deals at considerable length with the primitive family and criticises sharply the well-known views of Morgan and his school, maintaining that the evidence for the family life of the anthropoids points to monogamy, and laying stress upon the psychological aspect of the question,—the instinctive nature of conjugal love, jealousy, love of parents for their children,—he holds that in all probability, even in primitive times, the family life of man was monogamous. In this he agrees with Westermarck. In zoölogical life Ziegler recognizes three stages of development in the sexual relations: 1. The lowest stage, among the sponges, echinoderms, etc., where the sperm-cells are emptied in the water and wander about seeking the eggs, which are likewise migrating. 2. The "Begattungspaarung," found among many worms, arthropods, molluscs, and among the vertebrates, with fishes, amphibia and reptiles,—where two individuals unite for the purpose of reproduction and soon after separate again. Here not seldom appears a sort of love-play, an instinctive wooing and fleeing, or a caressing which precedes the copulation. After copulation, the care of the offspring devolves upon only one sex, usually the female, more rarely, as is the case with the *Gastrosteus aculeatus* and the *Alytes obstetricans*, the male; the highest stage of the method of sexual reproduction entails permanent pairing and the sharing of both sexes in the bringing up of the young—the typical method among birds and mammals. Although the permanent pairing is not everywhere developed in the same manner, but there is everywhere a *psychical relation* between the paired individuals, recognition, dependence, instinctive impulse (love), jealousy. The sexual relation may be either polygamous or monogamous, and for man a monogamic sexual relation seems primitive and natural.

III. EXPERIMENTAL.

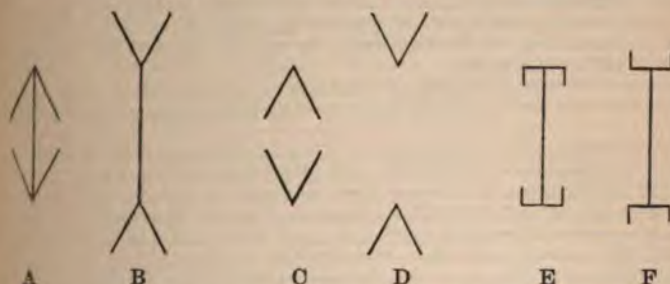
RECENT STUDIES OF AN INTERESTING OPTICAL ILLUSION.

- (1) *Optische Urtheilstäuschungen.* MÜLLER-LYER. Du Bois-Reymond's Archiv, 1889, Supplement-Band, 263-270.
- (2) *Ueber ein optisches Paradoxon.* BRENTANO. Zeitschrift für Psychologie, III., 1892, 349-358.
- (3) *Optische Streitfragen.* LIPPS. *Ibid.*, 493-504.
- (4) *Ueber ein optisches Paradoxon (Zweiter Artikel).* BRENTANO. *Ibid.*, V., 1893, 61-82.
- (5) *Une nouvelle illusion d'optique.* DELBŒUF. Revue Scientifique LI., 1893, 237-241.
- (6) *Les illusions d'optique.* BRUNOT. *Ibid.*, LII., 1893, 210-212.

- (7) *Zur Lehre von den optischen Täuschungen.* BRENTANO. *Zeitschrift für Psychologie*, VI., 1893-94, 1-7.
 (8) *Erklärung der Brentano'schen optischen Täuschung.* AUERBACH. *Ibid.*, VII., 1894, 152-160.

Reference may also be made here to the article of JASTROW in this JOURNAL, IV., 381, which contains matter upon this illusion.

The typical form of the illusion that is discussed in the papers above is shown in the first two figures below. The distance between the apexes of the angles in both cases is the same; it appears, however, considerably greater in *B* than in *A*.

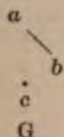


(1) This illusion was first mentioned, so far as the reviewer is aware, by Müller-Lyer, who described it with a number of others of similar character, in 1889, in an article reviewed soon after in this JOURNAL (III., 1890-91, 207). It seems worth while to recall this important paper, as it has been generally overlooked by subsequent writers. Müller-Lyer's explanation, which rests in part upon analogous effects observed in other figures, is that in judging such distances we involuntarily take into account not only the distances themselves, but also a portion of the surrounding spaces also. If, for example, figures *A* and *B* are completed by drawing vertical lines connecting the tips of the short oblique lines, the spaces enclosed would be smaller in *A* than in *B*.

(2) Brentano, without knowing of the work of Müller-Lyer, set about explaining the illusion himself. Of possible explanations he mentions four, but rejects the first three, namely: First, that the small added lines suggest traction and consequently compression in *A* and extension in *B*; second, that the addition of the small lines obscures the termination of the longer ones, which might be expected to result in overestimation in one case and underestimation in the other; and third, that we compare the lines by means of the muscular feelings resulting from running the eye along them and that the small added lines attract attention, which causes the eye to start and stop a little beyond the end of the lines in one case, and a little short of the end in the other. These Brentano rejects for the following reasons: The first, because curves (which do not suggest traction) may be put in place of the short lines without lessening the illusion, and also because the illusion persists, as in *C* and *D*, when there are no central lines to be stretched. The second, because where there are no central lines there can be no question about their termination [a plausible statement, but one that has little force, for an imagined line is almost certainly substituted for the actual one removed—a performance of which Brentano makes much in his

second paper]. The third, because the illusion ought to hold, and does not in figures *E* and *F*. [A remnant of the illusion does remain, as the author recognizes in later papers.]

Brentano's own explanation is based upon the well-known principle that small angles are relatively overestimated. By a series of simplifications of the typical figure, he finally reduces it to a single line and a point, as shown in figure *G*. The distance *ac* seems a little shorter than it actually is, and this is due in Brentano's opinion to an overestimation of the angle *bac* and an underestimation of the angle *abc*, the lines *ac* and *cb* being furnished by imagination. The overestimation and underestimation thus combine to produce a slight illusory rotation of the line *ab* about its middle point, which would in turn produce an apparent shortening of *ac*. This explanation, once admitted, is easily applied to the more elaborate cases.



Brentano's article concludes with the presentation of a group of figures devised to illustrate and further substantiate his explanation.

(3) The first half of the paper of Lipps is devoted to a question of after-images and need not be regarded here; the second half is a critique of Brentano's paper just noticed. Lipps urges, among other objections, that the overestimation, which Brentano assumes, ought to produce a bending of the short line and not a rotation of it; that the principle of the overestimation of small angles and the underestimation of large ones is not of universal validity, but depends on the attendant conditions, and that Brentano's appeal to it is, therefore, unjustified; and finally, that certain of Brentano's own figures (not to speak of others) cannot be explained by this principle, even if admitted. In support of these objections Lipps offers two striking figures of his own, in which the apparent reversal of Brentano's principle is very marked.

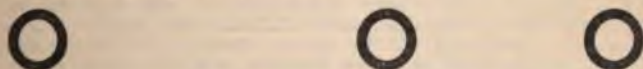
The explanation of Lipps (based upon principles worked out at length in his "Aesthetische Factoren der Raumansehung," Helmholtz Festgruss, Hamburg and Leipzig, 1891) is somewhat as follows: The lines of such figures represent ocular movements. When these movements are free and favored, as they are by extending and expanding lines, the distances passed over seem longer; when they are hindered, as happens with lines of contrary direction, the distances seem short. In Lipps' words, "*Solche frei, 'siegreich' aus sich herausgehende Bewegung nun wird überall in ihrem Erfolg, d. h., hinsichtlich der weite des Weges, der durch sie durchmessen wird, überschätzt, die gehemmte überall unterschätzt.*" This explanation is evidently not very different from the third of the explanations rejected by Brentano, allowing for Lipps' somewhat picturesque way of stating it, and there is, perhaps, some justification for a general statement of this kind, but it is to be noticed that in the familiar illusions of filled and open spaces, hindered movements appear to give a lengthening and free movements a shortening.

(4) In his reply, Brentano takes up the objections of Lipps point by point. He first fortifies his general principle that small angles are relatively overestimated and large ones underestimated by citing further instances, insisting, however, on the relative character of such judgments and on the terms small and large instead of acute and obtuse; of two angles both of which are acute and both are overestimated absolutely, the larger may be less overestimated than the smaller, and so relatively underestimated. He admits readily enough that, like other psychological "laws," this principle

is dependent for its effect in part, at least, on other conditions, but holds that it is sufficiently general for the use which he makes of it. He is wholly successful in explaining the first of Lipps' figures, but (in the reviewer's opinion) only partially so with the second. His replies to Lipps' criticism of some of the figures from his first paper, while apt in the main, do not in every case carry full conviction that he himself is right. Some slight remnants of illusion persist occasionally, and these, he is forced to admit, may have another cause.

Brentano in his counter critique of the explanation of Lipps, denies the special feelings which the latter finds in connection with extending and contracting lines, and holds that if the short oblique lines suggest motion, as they might from their resemblance to arrow heads, it is motion in the contrary direction to that conceived by Lipps — motion outward in *A* above, and inward in *B*. On this supposition, he presents a number of figures, in which an effort is made to strengthen or weaken the illusion by drawing the arrows more fully, but with practically no effect upon it. [This point seems to the reviewer of little significance, for Lipps can certainly reply that the movement suggested by the picture of an arrow is not at all the same as that suggested by a free line; the two belong to totally different psychical levels.] Other figures in which the central line is extended beyond the summit of the angle in both *A* and *B*, with no weakening of the effect or even with a strengthening of it, are much more to the point. A final set attempts to place in opposition the principle of Lipps (in the case in which Brentano admits it) and the principle of the over and underestimation of angles. The figures show a decided triumph of the latter.

(5) Delbœuf was also moved to reply to Brentano's first paper. After giving a careful summary of it, reproducing all the important figures, he adds a number of figures of his own, showing the same or a similar illusion, but in a way not to be explained by Brentano's principle, nor, indeed, by that of Lipps. One of the most interesting of these is one in which the distances to be compared are marked by dots, and the direction of the oblique lines, which in this case do not touch the dots at all, is wholly changed without destroying the illusion. From these he goes on to develop a set of figures of which the type is shown in the diagram below. In this figure the distance from the right edge of the left ring to the left edge of the middle ring is exactly equal to the distance from that point to the right edge of the right ring.



Delbœuf's explanation is essentially the third of those rejected in Brentano's first paper, and not different in the main from that of Lipps. Such illusions are due "to the attraction that lines drawn on an even surface exercise upon the eye;" and again "to the attraction that figures, whatever their form, set at the ends of distances to be measured, exercise upon the eye."

(6) Brunot objects to the explanations of both Brentano and Delbœuf. His opinion is that "in order to judge the mean distance of two objects, the eye instinctively takes the distance of the centres of the figures of the two objects." It is easy to see, then, why *B* should seem longer than *A*, and why the middle ring should seem nearer to the right in Delbœuf's figure. By skillful modifications,

the author applies this same principle to a number of standard cases and, in a somewhat similar way, by taking the general direction of motion suggested by the oblique lines in Zöllner's figure, explains that also.

(7) Brentano's third paper is devoted to Delbœuf. He contends that even if Delbœuf's explanation be true, it does not exclude his own, and presents figures designed to show the two principles in opposing action, with the decided triumph of the latter. [Such figures are really inconclusive, for they would, from Delbœuf's point of view, show nothing more than that lines arranged in different ways are different in effect, which every one would admit.] Of much more force is his objection, supported by clever modifications of Delbœuf's diagrams, that Delbœuf's explanation does not fit his own most characteristic figures, but that they are to be explained in a wholly different way. As to this proper way he is in essential agreement with Brunot above. In concluding he admits that this source of illusion may have coöperated in his typical figures (*A* and *B*), but considers it of subordinate influence.

(8) Auerbach also throws over the explanations of both Brentano and Delbœuf and returns to that of Müller-Lyer, though the paper of that author was unknown to him till his own was in type. The illusion is, he says, "a consequence of the influencing of that which one ought to see by what he sees indirectly in addition," that is, in judging the length of the central line, we take into account the spaces (or imagined lines) to the right and left between the arms of the angles. If this is so, certain deductions are possible with reference to the length of arms, size of angles, and character of the lines forming the arms, etc., which the author finds verified by trial. The points giving the distances to be estimated are quite subordinate things in sensation as compared with the arms; they are simply "not present at all as independent points, they exist only as the places where the arms meet." When means are taken to give the points a certain independence (making them larger, or separating the arms a little from them or making the points different in color), the illusion is weakened, because vision (or attention, perhaps) is less inclined to take the side spaces into account. Variations upon the typical figure are given in support of the author's general view. A few quantitative tests were also made.

As residual from these eight papers, we have, besides a number of interesting observations and a large collection of variant figures, four explanations of the typical illusion. Müller-Lyer and Auerbach explain it by an involuntary regarding of the adjacent areas; Brentano chiefly by over and underestimation of angles; Lipps and Delbœuf by a tendency of the eye to overrun or to come short of the movement required to follow the lines with the eye; and Brunot by an established habit of treating figures or parts of figures as wholes and estimating distances from their centres. A still more general grouping is possible, if we assume, as seems probable, that the misjudgment of angles itself depends in the last analysis on eye movements. Müller-Lyer, Auerbach and Brunot depend on a synthetic tendency in vision; Brentano, Lipps and Delbœuf on a motor tendency. It is almost certain that several causes conspire to produce the illusion in question, simple as it appears, but in what degree each is present cannot be told from present information. The answer to that question must await a thorough quantitative study of the subject.

Grössenschätzungen im Gesichtsfeld. von DR. R. FISCHER. *Archiv für Ophthalmologie* (1891).

The investigation of this subject occupies two articles, one in the first and the other in the third number of the *Archiv*. It represents two different sets of experiments. The first article is devoted to those on the ocular measurement of lines, and the second to the measurement of angles.

In the measurement of lines the experiments were directed in two different ways. The first method was to divide a given line into two equal parts, and the second to make a line equal to a given one. The arrangement for effecting these results consisted of two lines representing a cross, and placed upon a dark background. The intersection of the two lines formed the point of fixation. The whole of the lines forming the cross was at no time visible, but only such portions of them as were to be compared or divided. An apparatus was arranged to mark off the portions desired for this end. The comparisons were made between the upper and lower and the right and left arms of the cross. One arm was given and the effort was to mark off on the other arm a line equal to the given one, and again to divide a given arm into two equal parts. The experiments were conducted first with both eyes and then afterward with the right eye alone.

The experiments comparing the two vertical arms always showed that the lower arm was overestimated. Out of 560 attempts to divide the vertical arm equally, the average error was 6.70 mm. This result, indicating the difference between the two arms, is expressed by the following proportion: Lower arm : Upper arm :: 100 : 106.70. We may say here that the ratio is against the lower arm. In 128 attempts to make the two arms equal, the average error was 3.22 mm. against the lower arm. The proportions could be formulated as before. In comparing the horizontal arms the constant error was very slight, so slight, indeed, that the estimation was almost exactly correct. In 560 trials at halving a horizontal arm, the average error was only 0.79 mm. against the left arm, the proportion between the two arms being: Left : Right :: 100 : 100.79. In 680 attempts to make the two horizontal arms equal, the average error was only 0.73 mm. against the right arm when the given arm was on the left, and 0.43 against the left when the given arm was on the right. The results here seem to show very distinctly a greater accuracy in the estimation of linear magnitudes by the interior and exterior, or horizontal, than by the superior and inferior, or vertical, muscles of the eyes.

In experiments comparing the vertical and horizontal arms of the cross, the lower arms were considerably overestimated. In 960 trials comparing the lower vertical with the horizontal arm, the average error was 14.67 mm. against the lower, and in 800 trials, comparing the upper arm with the horizontal, the average error was 11.66 mm. against the upper vertical.

The experiments in monocular vision were no less interesting. In 950 attempts to make the vertical arms equal, the average error against the lower arm was 5.63 mm., and in the same number of trials to make the horizontal arms equal, the average error was 3.24 mm. against the right arm. The inference in these cases would be much the same as before, namely, the superior accuracy of vision in the horizontal lines. In 280 experiments comparing the lower vertical with the horizontal arms, the average error was 18.19 mm. against the vertical when the left horizontal was taken, and 15.14 mm. when the right was taken. In 280 trials comparing

the upper vertical with the horizontal, the average error was 10.83 mm. against the vertical when the left horizontal was taken, and 7.70 mm. against it when the right was taken. Here, again, the results show the greater overestimation of the lower arm of the cross.

The results at large are very well summarized in the following manner: First, in regard to the correctness of the estimates of magnitudes, the comparisons of the horizontal arms of the cross with each other by both eyes were the only cases approximating accuracy. In the other cases, (1) the lower arm compared with the upper, was uniformly overestimated; (2) the right arm compared with the left was overestimated; (3) in comparisons of the vertical with the horizontal arms, the lower was overestimated more than the upper vertical. Second, the results also show (1) that the estimates follow the psychophysical law, and (2) that the variable error was only half as great in dividing as in comparing lines, a fact which is construed as proving the practicability of the method of the least observable differences.

In the second paper, describing the experiments for comparing and measuring angles, a circle about 36 cm. in diameter was drawn upon a dark background. This circle was divided accurately into definite portions of thirty degrees each, and these marked by threads issuing from the centre and movably fixed in the circumference. The experiments consisted in efforts to divide a given angle in one of the quadrants into equal parts. Assuming the centre of the circle as the point of fixation, the results would show, as in the case of the cross, the capacity of different portions of the eye for judging magnitudes. The mean or average error was assigned in terms of the percentage of the real half angle to be guessed, and marked with a plus or minus sign, according as the eye judged a quantity larger or smaller than the proper one. The first set of experiments was with the right eye, and the second with the left. The results showed perfect similarity between the two eyes, and were summed up by the author in the statement that "when the angle to be bisected was horizontal or approximately so, the upper angles were overestimated, and the lower underestimated." This was true of both halves of the visual field. The experiments represented attempts to halve angles varying from 10 to 150 degrees. Another and distinct set of experiments were efforts to divide 180 degrees equally, beginning at different points in the circumference. The results were practically the same for each eye, and were very striking in one respect. Beginning at zero, which was the terminus of the vertical diameter in the upper half of the circle, and proceeding to the right with every ten degrees as a starting point for the 180 degrees to be divided, the average error was always plus until seventy degrees were reached, when it became minus and remained minus until 150 degrees were reached, when it became plus again up to 180 degrees. Beginning at zero, or 360 degrees, again, and proceeding to the left, the same distances showed similar results. From 360 to 290 degrees, the average error was plus, and then became minus until 200 degrees were reached, when it was plus to 180. This means that the upper quadrants were overestimated in most cases, and the lower quadrants underestimated in most cases.

J. H. HYSLOP.

Ueber Fusionsbewegungen der Augen beim Prismaversuche. von ALFRED GRAEFE. *Archiv für Ophthalmologie* (1891).

The object of the author is to throw light by special experiments on the question whether binocular accommodation is native or em-

pirical, and to test the comparative strength of the two opposite tendencies to fusion, according as the images are homonymous or heteronymous. The experiments were conducted between the limits of the parallel and the convergent position of the eyes. For testing the native and unalterable functions of binocular accommodation, a prism was used in front of one of the eyes, both horizontally and vertically. When the prism was placed horizontally, so as to produce homonymous images, the movements for fusion had to be divergent and the localization was apparently farther off than in reality, and when placed to produce heteronymous images, involving convergent movements for fusion, the localization was nearer than in reality. These facts are taken as indicating a native and fixed function for localization by binocular adjustment. The result was similar for the vertical position of the prism, which had the effect of throwing the images upon different planes in the retina, and there was no appreciable tendency to fusion, even when they could be brought into the median plane. The author, however, found some slight limitations to the absolute fixity of this law. Even in those cases where vertical fusion seemed to take place, there was reason to regard them as abnormal and exceptional. In regard to the comparative strength of the convergent and the parallel movements of the eyes, experiment seemed to show that the convergent were slightly the stronger.

J. H. HYSLOP.

Ueber den Einfluss der Geschwindigkeit des Pulses auf die Zeitdauer der Reactionszeit bei Schalleindrücken. VAN BIERVLIET. Wundt's Philos. Studien, X. (1894), 160-167.

Dr. van Biervliet has measured the sensory reaction-times of eleven university students to auditory stimulation and compared these with pulse rates found by careful counting just before taking the reactions. The instrument used was the Hipp chronoscope, regulated at intervals with the new model Leipzig *Control-hammer*. Six of the eleven subjects showed a regular quickening of the reaction-time with acceleration of the pulse. Four others showed something of the same tendency, but failed at the extremes of fast or slow pulse, and one observer exactly reversed the rule. In view of these more or less discordant cases and of the large size of the mean variation (as in all sensory reactions) when compared with the differences to be established, the quickening of the reaction-time with the pulse rate must be regarded as probable rather than proved. No statement is made as to possible changes in pulse rate during the time of taking a series of reactions, nor are the reasons given for the high pulse rates found sufficiently explicit. One would like to know how far the quickening was due to active exercise and how far to excitement, which last has already been shown (this JOURNAL, IV., 524) to quicken both sensory and muscular reaction-times. Possibly these data may be more fully furnished in the report of experiments on reactions to optical and dermal stimuli that is to be furnished later.

Einige Versuche mit der Wunderscheibe. GRÜTZNER. Pfleger's Archiv, LV., 1893, 508-520.

The author first describes two lantern methods for demonstrating stroboscopic phenomena simultaneously to a large company of spectators. The first presents a single figure in motion, the second a full set of figures. For the full description of these, which cannot be described in short space without the diagrams, the reader is

referred to the original. He next describes a method of mixing colors with the stroboscope, which, in principle, is similar to that of the color top, and might occasionally prove useful. Next follows an application of the stroboscope to the demonstration of simultaneous contrast. Suppose, for simplicity, a stroboscope disk with four slits, two of which (lying in the same diameter) are covered with red glass, the other two being left free. Opposite the stroboscopic disk, on the same axis, is placed a white disk carrying two small black circles. When this combination of disks is set in rapid rotation, the observer sees a white or light red field, in which four dots appear to lie, two red brown and two green, the latter colored by contrast. [Such an experiment seems, like many of Hering's, to speak emphatically for the physiological theory of simultaneous contrast.] A fourth experiment of a more psychological character is the following: A series of stroboscopic pictures in black and white, representing boys playing leap-frog, was taken, and, after it had been viewed for a short time with the stroboscope, the pictures representing one boy in the air exactly over the other, were changed by covering the boy in the air with white paper, those showing the initial and concluding stages of the leap remaining unchanged. The alteration, however, made no difference whatever in the conviction of the observer that he actually saw one boy in the air above the other. Indeed, the suggestion involved in the mere beginning of the spring was sufficient to cause the seeing of the whole of it. A very inviting means is thus offered — as the author points out — for the study of suggested illusion and hallucination. [Work along this line has apparently already been undertaken at Cornell; see page 414 of this JOURNAL.]

Pedagogisch-psychometrische Studien. Zwei vorläufige Mitteilungen.
DR. ROBERT KELLER in Winterthur. Biol. Centralblatt. Bd. XIV., No. 1, 2 and 9.

The present study was suggested by Mosso's experiments upon the influence of mental work upon the ergographic fatigue curve. If fatigue is due to a general deterioration of the blood, we may expect it to affect the muscles and that we may find a test, and possibly a measure of it, in the ergograph record. The latter may not be without value as a test of mental fatigue, even if this is of a more local character, since we may expect the strength and number of the impulses sent down by the brain to be diminished by it. In fact, may it not be possible to determine by this method the relative difficulty and the best grouping of studies in schools? The experiments are few in number and were all made upon a boy of fourteen.

The aim of the first is to study the effect of rapid reading upon the ergograph record. The general plan of an experiment is about the same throughout. Four ergograph records were taken, with intervals of about half an hour. A part of these intervals was spent in rapid reading, or other fatigue work. An hour's rest was then taken and an exactly similar experiment begun. Two experiments were made in the morning, and one, sometimes two, in the afternoon. The general result is that rapid reading at first increases, then lessens the ergograph record, and that the record after the hour's rest is usually lower than the fourth record. The fact that this order is repeated in successive experiments on the same day is some guarantee for a causal relation between mental work and changes in the ergograph record. It is also in harmony with Mosso's results. Similar experiments with the rapid reading

of Latin give similar variations, only fatigue appears more quickly. The effect of singing was studied in two experiments. In this case the period of increased activity drops out. In the second preliminary communication, the influence of reading numbers is studied. This work seems to lessen all the ergograph records in the first two experiments, but, in a third, to have the same effect as reading. This may be explained as due to the fact that practice diminishes the fatigue resulting from a given amount of work. The most important thing in these articles is the suggested application of this method of studying fatigue to pedagogical problems. J. A. B.

Recherches sur la localisation des sensations tactiles. Par M. VICTOR HENRI. Archives de Physiol. norm. et Path., No. 4, Oct. 1893.

Where one considers the localization of two points, the results of Weber's circles are considered good, but where only one is to be located, M. Henri thinks the best way is to do it on a photograph or by word. The photographic method was chosen. It was found that the error in direction was nearly constant for any single point of contact. The experiments on the dorsal surface of the fingers of the right hand show that there is a general tendency to locate the points of contact too near the end of the finger. At the end of the finger, however, the tendency is in the opposite direction, though the size of the errors is very much less. There are also some other points at the beginning of the second phalanx similar to the end of the finger. The errors in the transverse direction are of significance only in rare cases. If the point is placed on the side of the finger, instead of in the middle, the error is easily recognized. For any one point the error in localization is very limited, but it varies between 10 and 2 mm. for different points touched. The corresponding parts of different fingers do not vary appreciably. The middle of the first and second phalanges shows the greatest errors in localization, namely, 10 and 8 mm. Near the wrinkles (at the joints), which separate the two phalanges, the errors are equal to 4 or 5 mm. On the wrinkles and on the third phalanx their minimal value, which is 2 or 3 mm., is reached. At other points, however, they are even less than this. One subject found difficulty in distinguishing between points on the ring finger and those on the middle finger, though the amount of error in either case was constant. The difficulty disappeared in moving either of the fingers slightly. On the palm of the hand and at the commencement of the arm, the errors were different in direction and extent. The direction of error was always towards the part which separated the lower arm from the wrist, while the amount of error varied between 5 and 40 mm., being least when the point of touch was nearest this part and greatest in the reverse.

The conclusions from the experiments on three subjects are: (1) Where points are touched on the skin, they are located on the photograph at a point which, in relation to the touched point, is almost constant in direction. (2) Almost always the point indicated on the photograph is more near to a certain wrinkle or fold of the skin (*e. g.*, when there is a joint below) than is the point touched. (3) As the point of contact approaches this wrinkle the errors in localization decrease.

This shows us that all the points where we can localize a touched point are included on the inside of a fixed curve, which the point touched, as a rule on the inside of it, does not meet. Hence two elements of the curve can vary, namely, the size of it and also the distance from its centre to the point touched. These two elements

determine our spatial sensibility in the sense of touch. It follows, then, that if the two points in Weber's circles are not touched simultaneously, they are not located the same as if they were. If, also, any point B is located at A, the point A will not be located at B.

A. E. SEGSWORTH.

IV.—MORBID PSYCHOLOGY.

Die Mehrheit geistiges Personalitäten in einen Individuum. Eine psychologische Studie. Von DR. S. LANDMANN. Stuttgart, 1894, pp. 186.

The author is a practicing physician, who for forty years has sought to gain a conception of the nature of mental activities, and here gives us his conclusions, which are as interesting as they are new and carefully matured. Binet's theory of *simultaneous* activity of different spheres of consciousness is radically wrong. All facts on which this theory rests may be explained by supposing that the cortical ganglia of the brain can act unconsciously and reflexly when dissociated, as they may be by many causes, from the cells of the cortex which mediate consciousness. After discussing quite a mass of casuistical material concerning catalepsy, somnambulism, suggested acts and hallucinations, amnesia and distraction, anæsthesia, hysteria, etc., the author reaches the following general conclusions: Lethargy and complete hypnosis are to be explained as transient loss of function by all the brain organs. Cataleptic attitudes are the isolated activity of sub-cortical motor centres. Unconscious imitations of movement by cataleptics and hysterical patients with anæsthesia are due to the isolated activity of the sub-cortical visual centre. The unconscious verbal imitations of catalepsy is due to the isolated activity of sub-cortical centres of hearing. Unconscious mimicry by cataleptic and hysterical patients, and the active innervation feelings of anæsthetic hysteria, are due to isolated activity of the sub-cortical centres of feeling. The acts of catalepsy, the suggestions of hypnotism and hysteria, and suggestions of general hallucination tending toward a change of personality, are due to the activity of a larger or smaller group of cells in the cortex, *i. e.*, to the isolated consciousness of a larger or smaller series of connected concepts. Systematic anæsthesia and the "rapport" of hypnosis are due to the activity of cortical cells sensitive to an isolated concept feeling, *i. e.*, an isolated self-consciousness. Hysterical anæsthesia and suggested anæsthesia, lameness, amnesia, etc., are due to the shunting out of isolated cortical cells. Post-hypnotic suggestion and apparent multiplicity of psychic existences are due to the activity of isolated cortical cells, together with the simultaneous normal activity of other brain organs. Systematic anæsthesia, negative hallucinations, and the natural anæsthesia of hysteria, are correlated with the inactivity of the cortical cells responding to the action of the concept, along with simultaneous normal activity, *i. e.*, partial self-consciousness. The automatic activities of self-conscious subjects are due to the isolated activity of sub-cortical ganglia, along with the normal activity of other cortical cells. Finally, somnambulism is due to changing activity of various larger or smaller parts of the brain, with complete inactivity of the other parts.

To complete personality, the sub-cortical centres and all the cortex must act together. The individual who can be conscious of all concepts arising within or without, and of all feelings of activity, is a complete psychic personality. More or less

incomplete egos are those who can bring to consciousness only a part of the concepts of feeling, sense and motion. Dessoir's upper and lower consciousness is rejected. Consciousness is always one and one only, however frequently it may change. Janet's distinction between psychic and psychologic is developed so as to suggest that the subject stands over against its own sensation to which it may open or close its eyes.

La Foule Criminelle. Par PAUL VIGNY. Paris, 1892, pp. 185.

This little volume is translated from the Italian, and is called an essay in the larger field of collective psychology. Society might be regarded as a single person who had always existed, and instead of calling the individual a microcosm, M. Vigny urges that the world is a "macanthrope." Social individuality is for him, as for Tarde, the solar microscope of psychology. The old diction, *Senatores boni viri, senatus autem mala bestia*, illustrates how much worse mobs can be than the persons composing it. Legal responsibility for collective crime is a hard problem, never solved by the classic school of penology. Suggestion, imitation, moral contagion have much, but not final, explanatory power. Mobs are media in which the microbe of evil develops further and easier than the microbe of good. The influence of numbers increases the intensity of emotions to a degree described as psychologic fermentation. Often the plebi reclamanti have the moral inebriation of a despot. The males of the French Revolution were largely made up of degenerates, vagabonds, criminals and fools. Lynch-law and the despotism of majorities, the opposers of the latter, the individualists and aristocrats, the relation between, and frequency of, sudden and premeditated crime, the sensibility of criminals, and hypnotic influence and crime, are discussed from the standpoint of the Morel school and Lombroso.

Le Crime et la Peine. Par LOUIS PROAL. Paris, 1894, pp. 548.

This heavy work was crowned by the academy of moral and political sciences, or rather the memoir out of which it grew was, and there is more to follow, and is printed in Felix Alcan's valuable library of contemporary philosophy. It evidently grew out of the strife between the theory of the Lombroso school that crime is a disease and the old view of strict personal responsibility, which was so bitter two years ago in France. Statistical study of crime in relation to heredity and atavism, insanity, degeneration, sex, ignorance, misery, imitation, passion, politics and free will make up the 300 pages of the first part. The second is devoted to the origin of penal justice,—responsibility and determinism. Crime is not an organic fatality, but it is progressive decay. The penal code must cling to free will against the determinists because it is useful, necessary and true. Even necessitarians, when it comes to practice, have to make of liberty an "idée force."

Die psycho-pathischen Minderwertigkeiten. Von DR. J. L. A. HOCH. Ravensburg, 1891-93, pp. 427.

Reduced ability and responsibility may be innate, permanent, acquired, transient or mixed, to use the adjectives most often recurring (in this amplification in three installments of the suggestive chapter upon the same subject from the author's *Leitfaden der Psychiatrie*, 1888). The work is carefully wrought out with much casuistic material from the author's wide practice, and constitutes a valuable addition to the now so rapidly growing material on border-line phenomena.

Dégénérescence. Par MAX NORDAU. Paris, 1894. 2 vols. pp. 429, 566.

This work is dedicated to Lombroso, who is called "*un des plus superbes apparitions intellectuelles du siècle.*" None of the followers of Morel have traced degeneracy in literature. Some of the current literary modes are forms of intellectual decomposition. *Fin de siècle* is not *fin du race*, but the crepuscule or twilight of the people, and suggests the approach of a chillastic terror, like that when the first thousand years of Christendom was passed. The *fin de siècle* French boy passing a prison where his rich father was confined for the fifth time for fraud, called it papa's lycée. The *fin de siècle* police captain has a cigar and card case made out of an assassin's tanned hide. Horrible Kate Greenaway children, Zola, Ibsen, Nietzsche, Wagner, Tolstoi's Kreutzer Sonata, Paul Verlaine, the symbolists, Maeterlinck, Sarah Bernhardt, sensations unknown to the masses,—all these are degenerative stigmata. Schopenhauer and Hartmann, graphomaniacs with incapacity to act; absence of good judgment and sound common sense, excessive impressionability, a passion for useless baubles, retinal defect, dynamogenic eccentricities, hysterical giggling, precocious adolescence and old age, jactation, mystic presentiments, pre-raphaelism, æstheticism, the salvation armies of Egidy and Tolstoi, Wagner with his delusions of persecution, the neo-Catholics, to whom Parsival is a religious service,—all these things initiate hysterical people into delicious sensations, and cause idiotic ladies to roll up their eyes and cry charming, ravishing. Hypnotism, the "fourth dimension" speculations, spiritism, psychic researchers, animal magnetism, revelation of Isis, the Ethopées of M. Péladan, the idiotic echolalia of the Belgian poet, Maurice Maeterlinck, who, like Walt Whitman, was a fool, but yet a genius, — these are described as parodies of mysticisms.

The second volume is devoted to egotism and the phobias which arise from it, its self-consciousness, Gautier, Flaubert; Mendès with his theory of "sonorities;" Bourget and the "decadents;" Metesch, the delirious philosopher Baudelaire, and Ibsen, who has become a sort of popular poet laureate, as Voltaire and Victor Hugo were. Ibsen's clientele consists of women badly married, or who feel themselves not understood, are vacuous in soul and without occupation; but he is no more their friend than is Sachez-Masach, or Zola, whose realism shows us types more fit for criminal law than for the lunacy commission; the veritists, the "young German" school, William Morris, Leopardi, Lenon, Karl Marx, Karl Bleibtreu, pessimists, and most Hegelism,—all these are degenerate. The twentieth century will be better. All these morbidities will perish, and the way to effect the cure is by the cult of unselfishness. The German "alliance of men against immorality" is to grow. On the whole, the work comes nearer drawing a line between Semites and non-Semites than any book we have ever read. Israel is still the chosen people and all others are degenerate.

Unsoundness of Mind in its Legal and Medical Considerations. By J. W. HUME WILLIAMS, of the Middle Temple, Barrister at Law, London. New York, 1892, pp. 179.

When Beccaria said: "The happiest of all nations is that in which the laws have not become a science," he hit the present state of the question of legal medicine. Common sense still needs to be heard from in determining mental unsoundness and fixing the degrees of responsibility. This should not become a purely legal question, at

any rate. It is probably impossible to fix upon any act so vicious and eccentric that, taken by itself, it could prove insanity, unless it be an involuntary result of neural disease. He must not build theories from above downward, like the architects of Laputa. Medical experts have little weight with juries, and the author would not submit questions of sanity to experts, nor distinguish very sharply between mental, moral or legal insanity. Goethe was right that nothing brings us nearer to insanity than distinguishing ourselves above others, and nothing keeps us sane better than general intercourse with many and often common people. Monomanias, moral and impulsive forms of insanity, which make most trouble in courts, are fullest treated.

Psychiatrische Vorlesungen. Von V. MAGNAN. Leipsic, Heft I., 1891; II., and III., 1892; IV., and V., 1893.

These between three and four hundred pages of the distinguished Belgian alienist contain all his more original papers, about twenty in number, and are translated into German by P. J. Möbius. Most were originally printed from students' notes. Professor Magnan, as is well known, has won his enviable reputation chiefly by his valuable work on the border-line phenomena and cases. It is impossible to do justice to these meaty papers in a brief notice. The best of them, to our thinking, are the lectures on chronic delirium with systematic evolution, which the German school prefer to call *paranoia completa*. His study of degenerate types has never been surpassed. Sexual aberrations, dipsomania, the childhood of criminals, morbid impulses to purchase things, gambling, onomatomania, intermittent phenomena, hallucinations of the right and left brain, heredity,—these are some of the special topics. The author is at his best in casuistic analysis, where, if he is not so minute as Kandinski, his penetration extends in more directions. Our own American Dr. Cowles, however, compares favorably with either of them, so far as he has published.

Ueber die Bedeutung der psychiatrischen Unterrichts für Heilkunde. Antrittsrede in Utrecht. DR. C. WINKLES. 1894, pp. 92.

The psychiatrist fights degeneration and to correct heredity. He must touch hands with the general practitioner on the one hand and with the spiritual office on the other. Degeneration on all hands, due to alcohol, opium, prostitution, anti-hygienic lives, abounds more and more. Every medical student must study psychiatry and hygiene. Doctors used to treat diseases as ontological entities, now they treat patients. They must learn to individualize; and their motto must be *minister non magister naturæ*. Not only persons but individuals are unique. Bertillon never failed to identify his man among 120,000 by the few small parts of the body he tested. Most individual is the nervous system. Psychiatry is no longer unskilled labor, but it has not yet attained due prominence in medical education. Defenses of it have been usually regarded as *oratio pro domo*. Science must not ride so high a horse that it cannot see the ground under it. Doctors have lectured at the sick bed on the anatomy of the brain, general psychology, pathology and even philosophy. Experts differ nowhere so much in court as on questions of sanity. Materialists and spiritualists as such are an anachronism. It was psychiatrists like Meynert, Forel, Gudden, Charcot and Flechsig who gave us the key to the architecture of the brain, and not the anatomists. The sick bed must not be neglected for the laboratory. The old divisions of diseases into

those of the peripheral nerves, spinal cord and brain, are obsolete—since Ramon y Cajal. The nervous system is made of superposed layers of reflex mechanism; each layer has its diseases, and each disease destroys some reflex function. Brain pathology and psychiatry must go together. With institutions for private treatment, psychiatry spoke its first serious word concerning daily life. "Normal personality" is a contradiction in terms. Each is a norm to himself. Men are not responsible for their environments or for their own organization. The root distinction of all is between hereditary and non-hereditary psychiatric forms. The psychiatrist, like the doctor, must study human degeneration in its largest aspects. The disinherited, the disequibrated of all types and degrees are the alienist's peculiar care. He must know every type of degeneration, and collect pedigrees of all his subjects. Knowledge of symptoms of degeneration must become common knowledge. Our ancestors have left us deadly poisons as well as civilization. Wars kill the able-bodied, and weaklings are exempt. Cities rot masses of men, and charity preserves the weakest. Degenerate men and women are mutually attracted, and the stock thus happily dies out. Alcohol use, institutionalized monstrosities increase. Hygiene should teach all this. The test of science is what can it contribute to arrest this tide. The clinic is the school to know and learn to fight degeneration. Doctors should be consulted about marriage, and he should boldly oppose ill-advised unions. The clinic teaches suggestions, spiritism, subtle hysterical symptoms, that psychic blindness is simulation. Judges should see criminal brains. The ideal psychiatrist will study education,—especially that of defectives,—will be a father confessor of youth and maiden, a shepherd of souls for the sick, criminal, and for all at every important crisis of life, will see some good in the worst and some bad in the best. This is far beyond the tedious disputes about responsibility. Common types of degeneracy are hysterical cases with outer-suggestion, self-sacrificing, ecstatic, their brain cobwebs held to be real, with whimsical sympathies and antipathies, always oscillating between extremes; neurasthenics, who always procrastinate, who cannot concentrate, with anxieties, imperative ideas, frequent sex perversions and everlasting relaxation; the introverted hypochondriac, who develops illusions out of his somatic sensations; the impulsive epileptic, quick in anger, irritable and indolent, mystic and morally defective; political agitators, geiuses, etc. The hygienist, with microscope in one hand and organic chemistry in the other, must bravely war upon all these, and behind and above him must stand the psychiatrist.

Les Grands Aliénistes Français. Par le DR. RENÉ SEMELAIGNE. Paris, 1894, pp. 409.

This is the first volume in a series, and treats of Pinet, Esquirol, Ferrès, Fabret, Voisin and Georget. Full page portraits of each are given. The biographies and accounts of the reforms and theories of each are well given.

Die Paranoia, eine Monographie. Von DR. C. WERNER. Stuttgart, 1891, pp. 239.

Twenty-two pages of history, summarizing all accessible literature up to date. Sixty-four pages of general matter on duration, cause, authority, prognosis and treatment, and the rest devoted to thirty special type cases, classified into primary and secondary paranoia, and the primary cases sub-divided into simple acute,

simple chronic, acute with hallucinations, and chronic with hallucinations. The secondary forms are divided into post-melancholic and post-maniacal.

Grundriss der Sprachstörungen, deren Ursache, Verlauf und Behandlung. Von DR. LEOPOLD TREITEL. Hirschwald, Berlin, 1894, pp. 100.

The author attempts to give a general view, neither too comprehensive nor too theoretic, for the general practitioner, and even germanizes many of the technical terms for the benefit of the general reader. Some points, e. g., congenital aphasia and hysterical halting, are treated quite fully. Stammering and stuttering and deaf mutism occupy considerable space. On the whole the brochure adds little or nothing new, the literature at the end is very incomplete, and the eight-page chapter on the origin of language is hardly less than puerile.

Ueber Gesichtsfeld-Ermüdung, etc. Von DR. WILHELM KOENIG. Leipsic, 1893, pp. 152.

This is a laborious investigation by an assistant physician in the lunatic asylum at Dalldorf, to show the relation of retinal fatigue to the concentric shrinking of the field of vision in diseases of the central nervous system, and seems to have been prompted by the classic work of Willbrand on visual disturbances in functional nervous troubles. The latter was based on observations in an ophthalmological polyclinic, and the two together constitute most of our best material upon this topic, which is no less interesting and important than it is new. In all, data from 96 men and 118 women were used. The patient was placed, clad in black, between two windows, with his back towards them and facing a perimeter. Often two papers, each with a different color, were used. Förster's dimensions for retinal limits are assumed as normal, and tests were made for white and the primitive colors, not only on the vertical and horizontal, but in some cases on all nineteen of the radii. The casuistic material is first described in detail for both positive and negative results, in cases of simple psychic disturbance, hysterical epilepsy, chronic and alcoholic epilepsy, paralytic dementia, organic diseases of the brain, and post-traumatic diseases. Of all cases, seventy-four showed limitations of the retinal field, hysteria leading, and organic brain diseases and chronic alcoholism having least. The temporal side was more often affected than the nasal. In the course of the experiment the limitation often improved. The neurasthenic enlargement of the blind spot was also greatest at first and was more often temporal. If the limitation is monocular, it is assumed to be retinal; if binocular, it is probably central. Of the seventy-four negative cases only twenty-three showed reduced acuteness of vision.

Lehrbuch der Krankheiten der Nervensystems, für Studierende und Aerzte. By DR. ARMAND HÜCKEL. Leipsic, 1891, pp. 303, 29 cuts.

The author's ideal is condensation. This he seeks to secure by dividing his material into a general introductory part, treating disturbances of mobility, sensation, nutrition and of innervation of blood vessels, and a special part. The latter treats of the diseases of the peripheral nerves, nerve by nerve and often muscle by muscle, diseases of the spinal cord and its membranes, of the medulla, and lastly of the brain and its membranes, with a final chapter on neuroses, with neurasthenia in an appendix.

Sur un cas d'amnésie rétro-antérograde, probablement d'origine hystérique. J. M. CHARCOT. Rev. de Méd., 1892, XII., 81.

Charcot has detailed the circumstances of a most interesting case of amnesia probably resulting from an attack of hysteria, the latter brought on by the shock of bad news. The patient has wholly lost her memory of all events occurring during the month and a half preceding the attack, while since that time she has only been able to remember for a few moments what is going on around her.

Charcot believes the case to be one of modified hysteria continued under the form of amnesia. Among other reasons for this view is the susceptibility which the patient manifests to hypnotic treatment. Under a regimen of suggestion she is slowly regaining her memory. So the case is one of dynamic origin and not at all due to lesion. If the outcome is successful, as Charcot expects, the case ought to afford much valuable—if not wholly new—material.

J. R. ANGELL.

Amnésie post-éclamptique. H. BIDON. Revue Médecin, Nov., 1891.

This is a brief and instructive account of several cases of amnesia caused by puerperal convulsions. The impairment of memory varies in degree from inability to recall isolated words up to the complete loss of all events connected with the pregnancy and even the married life with its preceding courtship. The most instructive case is of this latter type, to which Bidon has given the name of *amnésie systématisée*. The condition is by no means to be identified, he thinks, with cases of double consciousness, and he is confident that no symptoms are present to indicate either hysteria or epilepsy. His explanation has an attractive plausibility, and is in line with recent psychologic hypothesis. He assumes that during the convulsions the cerebral cells undergo, through profound circulatory disorder, such violent modification as to destroy all the more recent and so more superficial memory traces. The older and deeper traces of earlier events are consequently less affected. No lesion need be assumed.

J. R. ANGELL.

Ein Beitrag zur psychischen und suggestiven Behandlung der Neurasthenie. Von DR. FREIHERRN, von SCHRENCK-NÖTZING. Berlin, 1894, pp. 48.

Of 828 neurasthenic patients, Hösslin found psychic symptoms in 765. The stigmata of nervous asthenia are so mobile and changeable that it is hard to fix their cause. The psyche can cause disease. Psychic cases are either (a) direct concept therapeutics, (b) indirect or negative psychic treatment, and (c) marked psychotherapeutics. Somnolence is mere idioplastic passivity, according to Van Eeden, that comes by inner concentration. Of 8,705 persons, without regard to age or sex, only 519 were refractory, 4,316 hypotaxic, 2,557 became somnolent, and 1,313 somnambulatory. Out of 278 patients hypnotized by various authors, twenty-four were not helped, seventy-two recovered, eighty-two were helped, eleven relapsed. Casuistic tables presented by the author show a predominance of benefit. The enumeration of functions influenced is by far the best yet made, and shows that few functional diseases are unaffected by hypnotic suggestion.

I Problemi dell' Ipnotismo. Letture tenute all' Ateneo Trevigiano nei giorni 22 Maggio e 19 Giugno, 1892. DR. GIUSEPPE STUCCHI. Treviso, 1893, pp. 131, 12mo.

After a general and historical introduction, in which Mesmer, Faria, Braid, and later investigators are briefly referred to, Prof.

Stucchi proceeds to define and explain hypnotism, citing freely from the Nancy and the Paris schools, concluding that: (1) Women are hypnotizable in larger numbers than men; (2) youth are more hypnotizable than persons of mature age; (3) individuals habituated to passive obedience, like ex-soldiers, domestics, workmen, are more hypnotizable than persons who are independent by education and character; (4) the illiterate are more hypnotizable than those whose intelligence is cultivated; (5) those believing in the power of the experimenter are more hypnotizable than the sceptical. The author holds that the opinion of the Nancy school, that all persons are hypnotizable, is exaggerated, and that the opinion of Janet, that only those who are afflicted with nervous maladies, the hysterical, the degenerate, those who are morally or physically exhausted, are hypnotizable, is incorrect. Prof. Stucchi treats of the character of hypnosis and hypnotism, the different states and their marks, the means of producing and ending them, referring to Charcot, Bernheim, Liébeault, Beaunis, Burot, Liégeois, Ochorowicz, giving an excellent résumé.

A. F. C.

V.—MISCELLANEOUS.

Psychology, Descriptive and Explanatory. A treatise of the phenomena, laws and development of human mental life. By GEORGE TRUMBULL LADD. New York, Scribner's Sons, 1894, pp. 676.

This is the seventh book and the eighth large volume put forth by the author, within the last few years, to own all of which now costs the devoted reader twenty-five dollars and fifty cents. Wundt, in a new heavy volume, fourth edition; James and Baldwin, in two volumes each; Külpe, Höffding, Dewey, Murray, Hill, Ziehen and many others; with Bascom and McCosh, perhaps, most fecund of all; new general text-books promised by Ebbinghaus and G. E. Müller, — surely in all this sudden abundance in a field where ten years ago was almost nothing, at least in English, and with all the variety of standpoint represented by these names, it seems time to call a halt, and to pray that the epoch of text-books may gradually fade into an age of special monographs in the many obscure and confused parts of this vast field. The writer of this note has now toiled through a great part of Professor Ladd's new volume, with growing marvel that he has done so much work so honestly and well. It is his best and maturest work, and contains, at least, by hint and suggestion most of the best and ripened concepts of the author's earlier works. The image of the village smith under the spreading chestnut tree, whose "brow is wet with honest sweat," etc., is the ever recurring suggestion. If there is little strikingly new, the good old story of attention, faculties, reasoning, conation, imagination, impulse, instinct, feelings, etc., is clearly and faithfully and sometimes a little exiguously told. It is safe, absolutely safe, for the pupils to whom it is dedicated, and for every one else. We do not find its chief note tediousness, as Professor James does, nor over-analysis, but its all-pervading defect, as we regard it, is timidity, over-caution and conservatism, an inability, now, alas, we fear, grown hopeless, to take the clear, consistent, scientific standpoint. He has toiled nobly on toward the new city of Man-Soul, admirably portrays many of its glories, feels their fascination often deeply, but yet more deeply feels that Diabolos, if he does not still lurk in its darker by-ways, has at least not withdrawn his rear guard, and, therefore, he and his will not cross the gulf that still

lies only too fixed between. It is this step-motherly repression, this reluctance and paucity of concession, the shoals of nice scruples on which he tries to run aground the argosies of hope and promise which may be yet the best thing in a movement so vast, and so rapidly growing, — these are what come dangerously near making this a "psychology without a soul," in a sense more fatal than the author's insistent hylophobia dreams of. This is a book of the old dispensation, dignified and prophetic of, but not itself a gospel of, the new. These souls are not lost, although they die without seeing the full light. The intellect is convinced, but the heart is not converted. Nature is not yet heartily loved and trusted. The reason for this halting attitude, we believe, lies not in the author's lack of long familiarity with the practical details of laboratory and clinic so much as in a sluggishness of religious perception, a lack of prophetic insight and depth. No one has so clearly seen that the old days of opposition between faith and science—the days of Huxley's early papers, of Tyndall's prayer-gauge, of a materialism never academic, and now made obsolete by dynamism—are forever gone, and that a new sense of harmony has arisen, as shown in neo-Christians like Phillips Brooks, who boasted that he had never preached on the relations between science and religion, but always had felt them one; like Drummond, who sees in evolution only the most potent reinforcement of Christianity; like C. M. Williams, in his "Evolutional Ethics;" Paul Desjardins, and many younger men who are to shape the future. Professor Ladd can no more extract sunshine from a cucumber than he can get new religious light or heat from scientific psychology, which to an increasing number is more and more dear because big with promise for larger Christian living. These things should, of course, have no place in a text-book, but should shed a kindly light over it. Without it, we repeat, we are dealing with psychology without a soul, and the teacher is merely kindling a back fire, lest the fire of the "burning bush" spread and kindle the soul with a little enthusiasm.

These home-spun metaphors may express, at least, the present writer's sentiment toward the general spirit and attitude of the book. Its other chief defect is shared with many other text-books. The time, we think, has fully come when every psychological course, and, therefore, text-book, should at least glance at the anthropological, the morbid, the psychogenetic side. Of all three of these fields, taught every year at this university with much copiousness, there is scarcely a trace, while instinct is very inadequately treated. Unlike details concerning the senses, these lie in the scope of the book, but are simply ignored. Yet, just these are the newest and most promising lines of development. In fine, like Porter's "Intellect," this volume is a very valuable and faithfully made summary within its field, and it is there it should be judged. It contributes little that is new, and in its present bulk can do little good as a class book. The small edition which will no doubt follow, we shall await with interest.

Grundriss der Psychologie, auf experimenteller Grundlage dargestellt.
 von OSWALD KÜLPE, Privatdocent an der Universität Leipzig.
 Mit 10 Figuren im Text. Leipzig: Verlag von Wilhelm
 Engelmann: 1893. pp. viii., 471; Index, 472-478.

To write a text-book of experimental psychology—that is, of psychology—in the present state of the science, is a very difficult matter. This statement is, perhaps, best proved by the fact that, until the appearance of Dr. Külpe's work, there existed no text-

book that could in any sense be termed adequate: and this, although more than one psychologist of eminence had brought the matter and manner of his teaching to publication. The reason is not far to seek. Modern psychology demands a more universal training than does any other science. Its representative, in order to be a psychologist, must be neurologist, psychologist and philosopher. He must have worked over the field of experimentation, neurological, psychophysical and psychological, for himself; the three adjectives are arranged in the order of the thoroughness necessary for psychological equipment; and he must be able to psychologize, to coördinate experimental results in a system, under the light of logic (epistemology) and—though to a less extent—ontology. Not to many men is given the ability to weigh details and to generalize from them in equal measure; and not many find time and opportunity for a systematic study of four separate sciences and literatures.

Dr. Külpe is especially well fitted for the task which he has set himself. A philosopher (more particularly an epistemologist) and a teacher of philosophy, he is at the same time an experimental psychologist of great critical and constructive power. He is a pupil of the two foremost living German psychologists—W. Wundt and G. E. Müller. The *Grundriss* is dedicated to the former master, and the author's indebtedness to him is constantly apparent. But none the less certainly is Müller's influence observable, in the general, sectional arrangement of material; and in the contents of certain chapters (*e. g.*, I. 4). In psychophysics, Dr. Külpe, who has been for some years chief assistant in Wundt's Institute, has had a very exceptional experience, both pedagogical and experimental. The former has stood him in good stead in the composition of I. Chap. 1 (On the Analysis of Sensation); a chapter which is nothing less than masterly. The latter has rendered his judgment singularly good, in the most diverse departments of investigation. The vexed question as to the nature of centrally excited sensations (reproduction and association), he approaches as only one who has himself experimented in the matter can: for the section on feeling he had laid a foundation in his dissertation *Zur Theorie der sinnlichen Gefühle*: tonal fusion he is qualified to discuss by his unusual musical gifts and education: the theory of temporal association he has already handled, in a series of articles in the *Philosophische Studien*: and finally, his *Habilitationsschrift* dealt with the doctrine of will in modern psychology. Apart from these special researches, he has taken part in all the Leipzig *Arbeiten* of recent years.

A book which took shape under such conditions we should expect to be good, and Dr. Külpe's text-book is thoroughly good. Only in one regard must I confess to a feeling of disappointment: the author, despite a nominal adherence, has departed very widely from the Wundtian doctrine of apperception. His work cannot, therefore, be regarded as an intermediary between Wundt's *Vorlesungen* and *Grundzüge*; the book which shall lead from the former of those to the latter is still to be written. One can only hope, in the interests of education, that it will be written shortly. Other deviations from Wundt's system I shall notice later. There is none so important as this, though there are two or three others of primary significance.

The Introduction (pp. 1-29) falls into three sections: on the concept and problem of psychology; on its methods, and on the assistance derived by it from other sciences; and on psychological classification and literature. Psychology is a natural science; it deals with experiential data (*Erlebnisse*), in their dependence on

the experiencing (*erlebenden*) individual. Its direct methods are the introspective and experimental; its indirect, the memorial and linguistic. The definition will, I think, be admitted to be an improvement upon those generally current. Whether memory and language can constitute methods is arguable, but Dr. Külpe makes out a strong case for them.

The body of the work consists of three parts, which treat (in Wundtian fashion) of the elements of consciousness, of the ordinary combinations of these, and of their more permanent combinations to form "states of mind." "Consciousness" is only a collective term for the sum of all *Erlebnisse* in their psychological aspect. Elementary among these experiences are two processes: sensation and feeling. These may be conjoined in two ways: by fusion and by combination. In the fusion, the constituent processes retire before the unity of the total impression; in the combination, the constituents are, in the whole, as evident as (or even more evident than) they would be in isolation. The perfect sensation has four attributes, under each of which it may be investigated: intensity, quality, duration and extension.¹ The feeling is characterized by intensity, quality and duration. It is plain that fusion occurs when qualities combine, while time and space relations remain the same;² whereas combination implies spatial or temporal difference. I think, with Dr. Külpe, that the two types of compounds are to be kept distinct, and that each requires a special theory (statement of conditions). Wundt prefers to regard fusion as the most intimate form of association. The difference can hardly be termed a radical one at present, since but little advance has been made towards a psychological theory of either process.

Part I. (Elements) comprises two sub-parts. The doctrine of sensation occupies just 200 pages; the whole book contains 471. Here is proof positive (if any were still needed) that experimental psychology has not stopped short at the simplest mental processes, in confessed inability to cope with the more complex. The doctrine of feeling occupies something over fifty pages. Little more than half of the work, therefore, is taken up with the consideration of the elements of mind. And a good deal of that which is included in this half might have been relegated to a later portion of the discussion, *e. g.*, the criticism of ideational associations in Chap. 4.

It is impossible here to estimate the special chapters in any detail. Chapter 1 (Analysis of Sensation) gives an admirable account of the "psychophysical measurement-methods," of sensibility and sensible discrimination. The classification of the methods is the most logical and coherent of any as yet propounded. In fact, the writing of this chapter is itself sufficient to give the author a high rank among psychophysicists. The beginner who assimilates its contents (and the subject is not an easy one to master, as all teaching psychologists know), working through and experimentally verifying the illustrations appended to the symbolical exposition of each method, will have served no inconsiderable apprenticeship in the science. In the chapter on sensation-quality, there is made an attempt to estimate the number of distinguishable qualities of tone and brightness.³ The sub-cutaneous sensibility is divided (accord-

¹ The cutaneous sensibility has all four; the visual has quality, duration and extension; the other modalities, intensity, quality and duration.

² Intensive fusion may occur in the case of the paired sense organs. Here there need be no prior fusion of stimuli.

³ No such attempt is made for colors. Of course, "color" is a fusion of brightness and color-tone. But if we start out from arbitrary saturation-values; determine their number (*e. g.*, in the solar spectrum); and then determine the number of less and

ing to the rule which classifies sensation by reference to the bodily sense-organs) into the muscular, articular and tendinous. It is certainly well to avoid the slippery term "movement-sensations." And there seems to be nothing objectionable in the ascription¹ to the tendons of a quality of strain; to the muscles of that of contraction (usually experienced in the exhaustion-complex), and to the surface of the joints, of that of locally signed pressure (cf. the skin). If there are missed in this chapter references to English research (e. g., to the audition-theories of Rutherford and Waller), it must be remembered that the work is a German text-book, written for German students; and that, where the space-limitations are so strict, a selection has necessarily to be made. Chapter 3 is concerned with the intensive sensibility and sensible discrimination, and with the theory of the latter (Weber's law).

The fourth chapter (Centrally Excited Sensations, Reproduction and Association) is a really important contribution to systematic psychology. It is curiously Herbartian in terminology. Setting out with a rejection of the associationist dogma, "Nihil est in memoria, quod non prius fuerit in sensu," the author (1) states the problem of "recognition;" (2) inquires into the properties of centrally excited sensations; (3) subjects the associationist doctrine to a severe criticism; (4) seeks to determine the motives and tendencies, the basis and accuracy of reproduction; (5) enumerates the general conditions of centrally excited sensations; and (6) attempts a psychological "theory" of their origin. There is no "quality" attaching to centrally excited sensations, other than the qualities possessed by those principally excited; still less do the central sensations or any of them constitute a new modality. But memory is not on that account by any means identical with the reproduction of the remembered; and there are indications that the central sensations do not stand in a simple relation to the peripheral. Direct recognition consists, psychologically regarded, in a special centrally exciting efficiency (*Wirksamkeit*) of the "known" impressions or memory-images, and in a corresponding mood (*Stimmung*). The known, as such, is pleasant; the unknown, unpleasant. The known falls into place, into connection and relation; the unknown remains for some time isolated, is related by effort.² The development of Dr. Külpe's theory must be followed in the book itself. It is not easy reading, and the beginner would do well to familiarize himself with the ordinary associationist position, with some less penetrating discussion of "recognition" (e. g., Höffding's³ or Wundt's in the *Vorlesungen*, which is very like the author's, but more popular), and with certain of the experimental memory-researches, before attempting its digestion.

The doctrine of feeling (pleasure-pain) is expounded in a single chapter of eight sections. The author, as I have indicated above, goes further than Wundt, and makes feeling (rightly, I think, though I should prefer the abstract term "affection") an independent mental process, of equal rank with sensation. The two current

greater saturations distinguishable within each of the values so obtained:—and if we regard saturation-values and illumination-values as identical:—the attempt is not hopeless, though the result is only hypothetical. The number of color-qualities proves to be about 40 50 000.

¹ Which, however, goes beyond Dr. Külpe's treatment.

² To the adult mind, nothing, of course, is absolutely "unknown." At least a word will be associated to its impression.

³ Höffding, though his theory is referred to (without a name) in the text of the chapter, is not mentioned in the list of references, p. 230.

methods of affective investigation are happily termed the serial (Fechner) and the method of expression (Mosso). Cautiously but quite definitely formulated is the view—surely correct—that it is intensity of stimulus which is effective for the pleasure-pain movement; the quality of stimulus doing no more than shift the limen of affection (or the central indifference-point) to right or left upon the abscissa of Wundt's well-known diagram. Theories of feeling are suggestively tabulated, and that of Wundt finally accepted.

Part II. (Conjunctions of Elements) also comprises two sub-parts. Fusion (some sixty pages) and combination (ninety pages) have each three chapters devoted to them. (1) The account of tonal fusion is based on Stumpf's (*Tonpsychologie*, II.). I notice several deviations from Stumpf's views, and cannot doubt, from my own experience, but that many others would have appeared, had Dr. Külpe been able to discuss the *Tonpsychologie* more in detail. A psychological theory he wisely does not attempt. (2) The sections on visual fusion (color-tone and brightness, based partly on Hillebrand's important paper, denying the attribute of intensity to visual sensation), etc., contain a criticism of Ebbinghaus' theory of vision. (3) Emotion and impulse are considered as representing fusions of sensation and feeling. Whether we regard the author's special conclusions and analyses as correct or not,² one point seems clear, that no exposition of systematic psychology, in book or lecture form, can in future neglect the doctrine of fusion in general, and of sensational fusion in particular. Just as the psychology of sensation has been elaborated, must the psychology of sensation-complexes be elaborated. When we see, our visual impressions are fusions; when we speak, our auditory impressions are fusions. There is enough opportunity for experimental work here to occupy at least a generation of psychologists.

The combination chapters discuss (1) the two psychological spaces of touch and sight. Dr. Külpe gives up Wundt's theory of the influence of eye-movement in the "construction" of the third dimension. Unwisely, it seems to me. That area is "given" we should all admit; every tactual and visual sensation is extended. But if the depth-idea is not original but associative (pp. 36, 373, 383 ff.), can not we get at it best in terms of eye-movement? For the rest, psychophysical "extension" and the Stumpf-James "bigness" (cf. p. 387) are quite different matters.³ (2) Of the duration of sensations not much can be said. Of our estimation of intervals, and its conditions—thanks mainly to Meumann—a good deal. The recent revolution in the time-sense psychology will be familiar to everyone. I need not resume the author's paragraphs. He might well have given the topic more space. (3) Under the head of association proper are reviewed the phenomena of contrast (color and brightness), and of action ("reaction"). Ideational association had already been considered in I. 4. Wundt's reference of contrast to the law of relativity is rejected; Dr. Külpe tends towards a peripheral theory. My own experiments on binocular contrast appeared to me to point to the necessity of a central hypothesis; but nothing certain can at present be said. The author's theoretical analyses of the two forms of the simple reaction have already been published.⁴ The compound reactions are here treated from a similar point of view.

¹Dr. Külpe has recently stated his views of fusion in the *Zeitschr. f. Psych. u. Phys. d. Sinnesorgane*, V., pp. 360 ff.

²I may, perhaps, be allowed to refer to a tentative note in *Mind*, N. S., II., 6, pp. 285 ff. which I hope shortly to republish in enlarged and revised form.

³Cf. Ladd, *Psychology*, p. 326.

⁴*Phil. Stud.*, VI., VII.

Part III. (States of Consciousness) gives twenty-odd pages to attention, five to self-consciousness and will, and five to sleep, dreams and hypnosis. The treatment of attention we must examine somewhat fully.

There can, I take it, be no doubt that, for Wundt, conation (whose quality is apperception) is a "conscious" process. Apperception fuses with all the remaining conscious content; but it is itself content,—psychology deals with nothing beyond content. It can not be disentangled from the concrete to anything like the degree to which affection can,—to say nothing of the practically isolable sensation. But it is content, and we consciously-experience (*fühlen*) it as the quality "active" or "effortful." The diffused excitation consequent on the explosion of a frontal-lobe cell is paralleled by a mental process; just as the explosion of a sensory-area cell is so paralleled. If this is not Wundt's position, in the *Psychology*,¹ the *Ethics*, and the *System*, then words must surely have changed their meanings.

Dr. Külpe *a changé tout cela*. Let us put the passages together. (1) Pp. 219, 220. Apperception is a term covering "undeniable facts of consciousness." The activity of will is the expression of the totality of previous experiences. *Only a small part of it*² is conscious; the rest lies below the limen of consciousness. (2) Pp. 273-275. The elementary quality of will, effort (*Streben*), is reducible to definite sensation-qualities. (3) Pp. 300, 220. The "unconscious" is characteristic for the activity (*Wirksamkeit*) of fusion and attention. The "unconscious" is an "effective, but in itself not perceptible constituent of a composition of elements." The "unconscious" covers physiological processes, to which no mentality runs parallel.—The expressions are not univocal. That in giving the psychophysical conditions (theories) of certain mental processes, physiological processes, not in themselves consciously-experienced by us, must be taken into account, no one would deny (*cf.* Müller's recent work on memory). I should not affirm, either, that apperception could be "perceived" (*wahrgenommen*). The verb is only applicable to the presentation of sensations. But that there can be an unconscious constituent in a complex of conscious elements seems, in terms of the definition of psychology, impossible. The author is sailing perilously near to von Hartmannism, with all its unpsychological implications. (4) Pp. 438 ff. The "small part" of will, which is conscious, is not will at all. Introspection shows no "new conscious act" in attention; we have only "effort" (a sum of organic sensations); there is no "special content: internal activity." On p. 446 *Aufmerksamkeit* and *Inhalt* are opposed; *cf.* p. 464 (*die Inhalte selbst . . . die Apperception*). The principal conditions of the appearance and persistence of attention are to be looked for outside of consciousness. Attention is not derivable from the posited elements of mind. The psychophysical process of attention is an inhibitory one. "If we combine the view that there must exist a special central organ for the operations of attention with the view that all these operations may be regarded as inhibitory processes, we obtain Wundt's apperception-theory in its most recent form." We have *Hamlet* with *Hamlet* left out: Wundt's

¹In the 4th Ed., as well as the previous editions. *Cf.* Külpe, p. 461, with the *Phys. Psych.*, II, p. 274. Dr. Külpe's loyalty to Wundt has led him to retain the latter's phraseology; and this retention can not but prove regrettably misleading.

²My italics.

³The adjective "special" is misleading. For Wundt, the *Thätigkeitsgefühl* (= conscious experience of effort = apperceptive content proper) is a general content; but it is special, as differing from sensational or affective contents.

theory minus the "consciously-experienced activity, which is characteristic of the whole process of attention!" Except for the postulation of the organ of apperception (and I admit that the "except" is a large one), Dr. Külpe seems in full agreement with Professor Münsterberg. (5) Pp. 462 ff. The will-process need not be conscious. There is not necessary a third, qualitatively definite element, beside sensation and feeling. The facts of will are referable partly to the laws of reproduction, partly to apperception (i. e., to unconscious physiological inhibition-processes).

Plainly enough, there is a great gulf fixed between the two theories.¹ Dr. Külpe has, for purposes of psychology, sworn allegiance to the heterogenists; though he reserves the physiological ground to autogeny. Perhaps in a second edition he will clarify his views, and give up the confusing Wundtian terminology. As things are, he seems at times to recognize a consciousness which is outside of and beyond conscious content; and to be attempting to give the appearance of fullness to a capacious old bottle, with but little new wine at his command.

In other respects, the treatment of attention is as exhaustive and clear as that of any other subject discussed in the book. I do not, however, think that it is pedagogically advisable to defer the consideration of this process till the very end of a text-book on psychology. In fact, there are several alterations in the arrangement of the contents of the work which might be proposed. The greater part of I. i. 4 should, in my opinion, be relegated to II. ii. 3. Section 40 (on the simple quality of will) does not belong in its setting. A I. iii. might have been introduced, for the consideration of apperception, without begging the question of its *Gefühlsein* or of its elementariness.² To explain impulse, etc., without apperception is not good psychology.³ And minor changes might be suggested.

In estimating the work as a whole, one looks round for something to compare it with. I can find nothing but Höffding's "Outlines." There is some similarity between the two volumes. Both are published as text-books; both are compressed and matter-of-fact in style, and far from easy reading; both contain real contributions to psychology, and are not mere compilations. But there the analogy ends. Dr. Külpe's *Grundriss* stands alone as the first published complete guide to experimental psychology. And we must judge its quality to be worthy of the place in history which this fact must, of itself, assign to it.

The present review has left a vast amount of debatable matter (contained especially in the sections entitled "conditions" of such and such processes) entirely untouched. But it has already outrun its limits. It needs only to be noticed, in conclusion, that the book is well printed, on cream (not on white) paper; and that, besides the table of contents, it contains a valuable index.

E. B. TITCHENER.

Pain, Pleasure and Æsthetics. An essay concerning the Psychology of Pain and Pleasure with special reference to Æsthetics. By HENRY RUTGERS MARSHALL. Macmillan & Co., New York.

The new inductive psychology, which began with laboratory experiments upon the senses, reaction-times, and the psycho-

¹ For deviation from Wundt on a point of detail, which seems due to a misunderstanding of him, see p. 464.

² Which latter, however, seems granted; p. 452, § 75, 1.

³ Cf. Mind, I. c., and Külpe's own analysis, for the justification of this. He has, of course, no room for a fusion of affection with conation; cf. p. 446.

physic law, has been for a decade drifting on toward the study of the active powers of will, and there are many very recent signs that it is entering the still larger field of feeling and emotion. When it does so, it will cover the entire ground of man's psychic life. As the problems have deepened, the old materialistic bias of these studies has decreased, until there now seem promise and potency of deeper insight even into man's religious life. Mr. Marshall's book is thus most opportune, and contributes so much clearness, confusing as is his plan of arrangement, that it must be read by every one interested in the subject. He has made himself well acquainted with the vast and varied literature of the subject, save only the works of Oppenheimer — which probably appeared just too late—and of Dr. Henry Head, both of whom discuss the problem of pain in a way that is very important for his theory. Besides being severely scientific, they base their work more entirely upon anatomical and pathological data than Mr. Marshall approves.

We have read Mr. Marshall's book from cover to cover, and are much indebted to him. His fundamental position is that pleasure and pain are not the basis or raw material out of which all mental life is developed, because were this the case pleasure and pain would be used up, like raw material, in the product; and if mind was made out of them, it would show traces of their duality. Neither does he think pleasure and pain to be *sui generis* and apart, like special senses; his view is that they enter as differential qualities into all mental states, and that either of them may belong to any act or element of consciousness. If they are special qualities, they may come to all mental phenomena. Mr. Marshall's classification of instinct-feelings, of which the emotions are complexes and coördinates, is clear and convenient. Joy is a complex psychosis of coming advantage; dread, of disadvantage. Sorrow is loss of advantage; relief, of disadvantage. Over against these four passive are four active feelings—love, a complex psychosis tending to go out to beloved objects; fear, tending to flee from disadvantage; anger, to drive it away; and surprise, which is a concentration of effective action on a single object. To these last four he adds a tendency to imitate, and another to please or attract advantage. States of pleasure and pain, or algedonic states, to use the author's convenient new term, color all and do not have the wide neutral or untuned interval between them which Wundt—whom the author thinks is coming around to his general view—urges. Due scope is given to the nutritive factors which Grant Allen first brought into prominence. The description of each emotion is interesting and comprehensive. Although emotion is said not to originate in reflex or other movement or attitude, due attention is given to the latter. We could, however, but wish that so competent an author could have included the fascinating topic of sign-language, and perhaps even Delsarte, in his field of view. The object of art and pedagogy might be conceived as the enlargement of pleasure-fields—to use another happy conception of the author—and the frequency and prolongation of pleasure-states. Algedonic æsthetics are thus related to pedagogy and to ethics, and racial pleasure-getting is equivalent to racial effectiveness. This view does not favor utilitarianism nor egoistic hedonism. Even the bitterest restrictive pains should not be eliminated, for this would be death of the higher entity. The relatively permanent pleasure-field of revival is for each person the æsthetic field to which he refers in making judgments.

LETTERS TO THE EDITOR.

DR. FOREL, OF ZÜRICH.

TO THE EDITOR:

The following short account of the work of Dr. Forel is gladly sent in response to your request:

Outside of Switzerland, Dr. Forel's reputation is based largely on the fact that he is one of the greatest authorities on hypnotism, but a more intimate acquaintance with his work here brings other phases of it into equal prominence.

In appearance Dr. Forel is large and commanding, and shows in every movement the spirit of activity that is one of his most marked characteristics.

The medical department of the University of Zürich has attained a good reputation among German universities, and when vacancies occur, effort is made to secure the best men from abroad to fill them. A few years since Dr. Forel was called from Munich to occupy the position of professor of psychiatry in Zürich. In connection with this office, he gives two courses of lectures during the year, one on general psychiatry, and one on hypnotism, and also holds semi-weekly clinics. His lectures are very popular and well attended, and in the course on psychiatry he touches on many general subjects, which show how broad and far reaching his interests are. The opening lecture states his *Weltanschauung*. He is a pantheistic monist, believing in the existence of God as a universal force, of which life and matter are manifestations. He denounces with great vigor all anthropomorphic ideas of a personal God, and explains what is commonly called sin as being the result of a weak *Keimplasma*. He gives one lecture of the course on Weismann's theory of heredity, which he accepts. Among the most interesting of his lectures this year were one on brain localization, and one on disturbances of speech.

The clinics are attended by medical students, and, by special permission, by students of psychology. To the latter, these clinics furnish a fine opportunity for the study of pathological psychology, as Dr. Forel never fails to make the psychological significance prominent in the diagnosis of the cases examined. Among the rare cases presented this year was that of a patient suffering from paraphria, with general paralysis of the muscles of the eye, on the same side as the brain trouble, causing the paraphria. Only one other case of the kind has ever been known. Also a case of negative hallucinations of memory, the possibility of which was discovered a few years since by Bernheim, and several interesting cases of auditory and motor hallucinations.

In addition to his official position as professor of psychiatry, Dr. Forel is director of a large insane asylum in Burghölzli, a picturesque village about two miles from Zürich. Here he holds his clinics,

and here is his laboratory, which contains the largest collection of pathological brain material in Europe. He is most cordial to all who visit the asylum for scientific purposes, and very free in offering opportunities for study in his laboratory. Dr. Forel himself is making some special investigations in brain localization, and is quoted as authority in scientific works on brain anatomy. As director of the asylum, Dr. Forel is noticeable for the philanthropy which enters into all his relations with the insane. He studies to promote the comfort of even the most abandoned cases, and often patiently listens himself to the details of slight disagreements among them. His regimen for the patients is liberty and work, in so far as possible.

Dr. Forel's investigations in hypnotism are well known. His work on hypnotism is much superior to that of Moll, and is probably the best hand-book on the subject yet produced. He is also one of the editors of the *Zeitschrift für Hypnotismus*, published at Berlin. In his theories of hypnotism he belongs to Bernheim's school, although he opposes all dogmatism on the subject. He insists on accurate scientific methods in hypnotic research, and denounces the large class of psychologists who condemn hypnotism on *a priori* grounds, without having any experimental knowledge of it. His method of inducing hypnosis is short and quick. He causes the subject to look him fixedly in the eyes. Then, holding his thumb and finger before the two eyes of the subject and near, he bids him look at them, and drawing them down and away, the eyes diverge, and at the same time close, and the subject is asleep. On subsequent occasions a mere "*schlafen Sie*" is usually sufficient. The percentage of hypnotizable subjects Forel puts very high.—I think from 90 per cent. to 100 per cent. among normal persons. Despite the fact that an insane asylum is the most unfavorable place for the practice of hypnotism, Dr. Forel furnishes remarkable examples of its psycho-therapeutic value, and striking illustrations of the power of post-hypnotic suggestion. One cannot but think, however, that his success in the latter is due largely to the influence of his own strong personality. One feature of his use of hypnotism is his economy of labor in the insane asylum by its means, a course which has been opposed by his confrères, but the practical advantages of which they have been obliged to admit. One occurrence of the past year will illustrate this. An insane woman was expected to give birth to a child, and as on several previous occasions she had caused the death of her infants, the closest watch was required. Instead of providing night watchers, Dr. Forel hypnotized a waiting maid, and put her to sleep in an adjoining room, telling her to sleep quietly, but to awake and call the doctor should the woman be taken ill in the night. The waiting maid slept, undisturbed by the ordinary screaming of the insane woman, but awoke the moment that medical aid was required, and the life of the infant was thus saved.

In the city of Zürich Dr. Forel is well known as the president of the Temperance Society, a prominent member of the Society for Social Purity, and the leader in a movement to procure funds for the establishment of a People's Palace. He advocates total abstinence from alcoholic drinks with great vigor, and does not allow any use of alcohol in the insane asylum. He never fails in his clinics to emphasize the fact that a large proportion of the cases of insanity result from the prevalent use of *Schnapps* in Switzerland, and one of his most prominent lectures is a vigorous setting forth of the effect of alcohol on the brain tissue.

This short outline of Dr. Forel's work would not be complete without reference to his investigations in natural science. Among other studies in this direction, he has worked for years in collecting material in regard to the habits of ants. His library is full of books on the subject, and his published articles about ants and their habits are authoritative.

MARY MILLS PATRICK.

Zürich, April 20, 1894.

A CORRECTION.

EDITOR AMERICAN JOURNAL OF PSYCHOLOGY:

Sir:—In the last number of the AMERICAN JOURNAL OF PSYCHOLOGY, pp. 228-9, Mr. T. L. Bolton courteously gives me credit for suggesting the rhythm instrument there pictured and described. In doing so, however, he has fallen into error. The rhythm instrument is but a slight modification of a beat instrument used long ago by Prof. Alfred M. Mayer (*American Journal of Science*, 3rd Ser., VIII., 1874, 241; and XLVII., 1894, 5).

E. C. SANFORD.

The editors of *Mind* request that all MSS. from America, intended for publication in that journal, be forwarded to Prof. E. B. Titchener, Cornell University, Ithaca, N. Y., instead of to Mr. Stout, as has hitherto been the case.

THE AMERICAN
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COMPARATIVE OBSERVATIONS ON THE INDIRECT
COLOR RANGE OF CHILDREN, ADULTS, AND
ADULTS TRAINED IN COLOR.¹

BY GEO. W. A. LUCKEY,

Fellow in Psychology at Clark University, Worcester, Mass.

The work already done in indirect color vision has been largely summed up in the original investigations of A. Kirschmann⁽¹⁾,², A. E. Fick⁽²⁾, C. Hess⁽³⁾ and A. Fick.⁽⁴⁾

Kirschmann, with others, has shown that only a small part of the retinal surface is sensitive to the fundamental color impressions; that the retina is sensitive to impressions of blue much farther from the centre than to any other color impression, or giving the eccentric range of the different colors, he finds (beginning with the color having the greatest range and proceeding toward the centre) blue, yellow, orange, red, green, purple, violet.

He finds, further, that "The action of the peripheral retina varies greatly, in different directions, from the centre, and that the ranges of all colors extend farther in the nasal and

¹This investigation was made in the psychological laboratory of the Leland Stanford, Jr., University, and the writer desires in this connection to thank Dr. Frank Angell and Prof. Earl Barnes for helpful suggestions and criticisms, and also Mr. J. C. Hammel, and others, for kind assistance in the laboratory.

²The small figures in parenthesis refer to the bibliography at the end of the article.

upper parts than in the lower and temporal parts of the retina." He also finds that the size of the colored objects has much to do with the extent of the range.

A. E. Fick finds, in going over an investigation of Charpentier,¹ that there is considerable mutual assistance of separate neighboring retinal spots in the perception of color and of light. He also finds that the centre retina is more sensitive to color and form impressions, while the eccentric retina, as we pass toward the periphery, is more sensitive to light and motion impressions, but much faster to some colors than to others; and that intensity has influence on the perception of light, color, form and number, but just how great is not determined.

The paper of Hess, published in 1889, is a very careful re-investigation of peripheral color vision, both spectral and pigment colors having been used, and its results partly support and partly supersede those of previous experimenters. He finds equal color ranges for red and green, and for blue and yellow, when these colors are of the hue of Hering's original colors (*Urfarben*).

A. Fick's study, to which reference has been made above, concerns itself with the theory of color vision, and only touches indirectly upon the work which the writer has in hand.

The present investigation has been like that of Kirschmann, in being a study of the eccentric color range, but different, in being a comparative study of the eccentric color range of individuals differing in age, sex and previous color training.

After nearly two months of preliminary testing of both the perimeter and a flat surface apparatus (campimeter), it was concluded that the former possessed more advantages, with fewer disadvantages, and for this reason all the results recorded in this paper, excepting those on the upper and lower vertical meridians, were made on the perimeter. In the writer's opinion the errors arising from unequal illumination in different parts of the perimeter² are more than counterbalanced by the variation in angular magnitude in the case of the campimeter, especially when, as in the case of these experiments, the perimeter was illuminated equally from the sides, from behind and from a skylight; all windows being slightly to the rear of the observer. In fact, the results obtained on either apparatus were almost identical up to about 30° from the fixation point, but for greater distances the results given by the

¹ Arch. d'opht. Juli-August, 1886.

² Kirschmann⁽¹⁾ S. 600.

campimeter were from 1° to 15° less than those given by the perimeter, varying with the angle. It is difficult to see how accurate results can be obtained on the campimeter, when the angular magnitude is greater than 30° until we know more definitely the effect of size and intensity on color vision.

The only meridians in which the illumination of the perimeter was thought to vary were the upper and lower vertical meridians, and as the color range in these is small, the campimeter was used for them.

The perimeter apparatus consisted of a darkened steel rod, made in the form of a half-circle, fastened to a black wall by a screw at its middle point. This rod could therefore be adjusted in any desired meridian, the screw on which the perimeter turned being the point on which the uncovered eye of the observer was fixed. The observer was seated in an adjustable chair which carried a head rest. In this latter his head was placed, with his eye on a level with, and just two feet away from the fixed point, or at the centre of the curve of the perimeter. The only difference between the campimeter and the perimeter above described was the substitution of a dark rule, eight feet long, for the semi-circular rod. This was fastened to the wall in the same way and graduated to inches and projected degrees.

The colors used consisted of Bradley & Co.'s colored cylinders (familiarily known as the Hailmann beads). The cylinders were 12 mm. long and 12 mm. in diameter, and were perforated with a small opening from end to end, by means of which they were easily fixed on the end of a darkened steel rod 2 mm. in diameter and 1 m. long, used for moving them along the perimeter. The colors were first brought into the field of vision from the periphery by the operator, and the exact place on the perimeter noted at which the color could be recognized by indirect vision. The color was then placed at the centre and gradually moved toward the periphery until it lost all color, all changes in color, and the places at which they occurred, being also noted. One eye of the observer was continually covered, while the other was kept constantly on the fixed point during the time the color was moving to or from the centre. The latter was permitted to turn about, however, during the time the experimenter was recording the past result or was adjusting a new color. Six colors were used—red, orange, yellow, green, blue and violet; more colors became confusing and were not named with accuracy. Purple and violet could not be accurately distinguished in indirect vision even by those most practiced in color discrimination, *e. g.*, teachers

of painting. Two persons who had had considerable experience as art teachers said they could not tell, as violet was being brought toward the centre, whether it was going to be violet or purple, and were only sure when it was within a few degrees of the centre. Possibly this may be accounted for by the fact that we have little or no experience in color discrimination in the peripheral parts of the retina, and color sensations there are not only different from those at the centre, but have also not become grouped or classified. The above is only true, however, of tints and shades of the same color or of neighboring color mixtures, for, as Kirschmann⁽¹⁾ has observed, a spectral red, and a red carrying a little blue become noticeably different as they pass toward the periphery, the former changing to orange or yellow, the latter to blue. The same may be said of the other colors.

The colors used were supposed to be true imitations of the corresponding spectral colors, and were perhaps as exact imitations as it is possible for pigment colors to be. Fresh cylinders were used for each individual tested, so as to prevent all danger of change by use. Three double tests (*i. e.*, three tests with the color approaching the centre and three with color receding from the centre) were made with every color in every one of the eight meridians, and the average (range) of these tests taken as the one nearest correct.

The results obtained by the present investigation, in so far as they represent ground traversed before, agree with the results of Kirschmann⁽¹⁾, Fick⁽²⁾ (⁴), Raehlmann⁽⁵⁾, and others, in the following facts: (1) The ranges for the different colors were from 1° to 2° greater when the colors were moved from the centre toward the periphery than when they were moved from without toward the centre. (2) There are certain meridians in which the colors can be seen and recognized much further than in others. The upper nasal half of the retina is sensitive to colors farthest toward the periphery, while the under temporal part is sensitive to color over the least area. (3) The colors seem to arrange themselves in a certain definite order, according to the distance they can be seen in indirect vision. Beginning with the color having greatest range and proceeding toward the centre, we have first blue, then yellow, orange, red, green, violet. (4) The colors seem to fall into two rather noticeable groups—the blue-yellow group and the red-green group. Blue and yellow do not coincide in range but fall nearer together than in Kirschmann's investigation. The same is true of red and green. (5) As shown by Raehlmann⁽⁵⁾ all the colors used entered the field of color vision as either blue or yellow (*i. e.*, yellow or orange). Blue and yellow

give no sensation of color when seen beyond their range, unless different shades of gray may be called color. Green is generally seen first as yellow, and a few degrees nearer the centre is recognized as green. Red is generally seen first as yellow, then as orange, then as red, though often it is seen first as orange then as red. Orange is generally seen first as yellow, and violet is always seen first as blue.

The following facts may be inferred from other investigations, but I believe have not been stated: *e. g.*, blue is the most stable and permanent of all the colors. It is never mistaken for any other color, enters the sensitive color field as blue and remains a blue throughout the entire field. Green, although its range is very much less, comes next to blue in permanency; while it is generally seen outside of its range as yellow, yet there is quite sure to be found a certain definite place on every meridian within which the same observer is sure of the color. Yellow is perhaps the most variable: for example, in all the tests made on the same individual the range for blue never varied more than 3° , and the subject always felt sure when blue entered the field, whether he expected it or not. In green, the variation (with the same observer) was never beyond 4° , but he was not quite so sure as with blue. With yellow, on the contrary, the observer often varied from 1° to 10° , and never felt quite sure that the yellow might prove to be orange, or red, or even green.

Another interesting fact is that violet is seen as blue a degree farther than the blue itself, but is not recognized as violet until within the range for green.

The variation between yellow and orange, as mentioned by Kirschmann¹ was not confirmed by the present investigation, as yellow showed a greater range than orange in every one of the eight meridians, and came as near to blue in one meridian as in another.

The purpose of the present investigation was, however, less to study the general subject of peripheral color vision than to answer the following questions:

Can children see colors in indirect vision as far as adults?

Does sex have anything to do with difference in the range?

What influence has color education on the range?

As this study was for the purpose of establishing the comparative range, great care was taken that all the conditions should remain as nearly constant as possible. All tests were made on uniformly clear days, between 2 and 4 P. M. As soon as the observer showed signs of fatigue or

¹S. 609.

inattention, the work ceased and was resumed at another time. The writer made all the tests himself (assisted by Mr. J. C. Hammel, a fellow student in psychology), in order that there might be uniformity in the manner of presenting the colors, although the observer was always kept ignorant of the order or of the color approaching. The difficulties which occur here are, for the most part, similar to those which occur in all experimental work in psychology and need not be mentioned. There was one difficulty, however, which at times became very annoying, especially in the work with children, and as the writer has failed to find it mentioned by others, a little space will be devoted to it here.

The eye of the observer is supposed to remain fixed on a small surface at the centre (5 mm. in diameter in these tests) during the time the colors are approaching or receding, but as the color enters the field of vision the attention is diverted from the fixation point to the approaching color, and as the attention becomes diverted the eye often unconsciously follows it a little. The same is true when the colors are receding. In this way it often occurs that the eye has changed from 1° to 10° , or even 15° in children, without the observer being conscious of the fact. Some way of overcoming the errors which would result from this change of vision is necessary, and as the observer is not conscious of the fact he cannot be trusted to tell when his eye has changed. To avoid the above error it was found necessary for the operator to watch constantly the eye of the observer and not to record any results in which the eye wavered from the fixation point. Such a control would obviously be inapplicable to the dark room experiments that others have preferred.

Now for the answers to the questions. First: Can children see color in indirect vision as far as adults? The writer first obtained the color range of ten adults who were students in the university, but found later that he could not get so many children of any one age, and selected from the adults six whose color range seemed to vary least. He then obtained the color range of six thirteen-year-olds, and next the color range of six children who were seven-year-olds.

In Tables I, II, III and IV are presented the results of these investigations. The figures indicate the number of degrees from the centre point at which the colored objects could be correctly seen, and represent the external visual field. When referred to the retina the meridians must, of course, be reversed, the outer horizontal meridian would, on the retina, be the *i. e.*, nasal meridian, etc., etc. The average results are shown by diagrams in the plates at the end of the article. These diagrams

TABLE II.

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THIRTEEN-YEAR-OLDS, CLASS II.

	MALES.						FEMALES.						Av Range for every Meridian.															
	A			B			C			D				E			F											
	R	L	R	L	R	L	R	L	R	L	R	L		R	L													
Outer Horizontal,	55	52	57	55	45	54	55	59	54	54	57	57	54	55	78	82	65	73	82	83	75	70	70	71	75	69	74	74
Outer Lower Oblique,	44	46	45	48	45	54	53	50	53	49	50	48	48	49	55	66	47	46	70	70	58	52	60	64	65	64	59	60
Lower Vertical,	30	30	30	31	30	30	35	33	25	29	27	31	29	31	37	38	33	31	43	43	33	34	43	43	39	39	39	39
Inner Lower Oblique,	34	28	28	30	35	30	28	30	25	26	31	26	30	28	35	32	36	35	35	35	33	39	39	32	33	35	34	34
Inner Horizontal,	32	25	30	28	29	28	33	28	26	28	27	32	29	28	37	44	38	43	34	38	35	36	40	40	34	36	36	39
Inner Upper Oblique,	25	26	25	26	30	30	28	25	27	23	26	28	27	26	35	38	38	38	35	37	32	33	41	42	34	33	36	37
Upper Vertical,	20	22	18	23	22	20	26	25	21	20	23	26	22	23	32	34	32	30	37	33	30	30	30	31	30	30	31	31
Outer Upper Oblique,	28	26	27	30	30	31	33	35	40	35	32	33	32	32	37	43	38	38	38	43	37	37	40	40	38	45	39	41
Average Range,	33	32	32	34	33	34	36	36	34	33	34	35	34	34	44	47	41	42	45	48	42	40	46	47	44	45	44	44
Sex Average,	33						34.5						43.4						44.5									

The External Visual Field for Yellow.

This above chart represents the average results of three double tests of each individual through all the meridians as indicated. The letters A, B, C, D, E, F, represent the different individuals of each class.

SEVEN-YEAR-OLDS. CLASS I.

	MALES.									FEMALES.						Av. Range for every Meridian.													
	A			B			C			D			E				F												
	R	L	R	R	L	R	R	L	R	R	L	R	R	L	R		R	L											
	R	L	R	R	L	R	R	L	R	R	L	R	R	L	R		R	L											
Outer Horizontal,	30	30	31	30	35	36	30	32	27	28	30	31	30	31	56	50	54	48	46	47	46	46	46	45	45	47	49	47	
Out. Lower Oblique,	26	26	26	27	30	30	29	27	29	25	25	25	27	27	30	30	37	38	34	35	30	\$4	36	33	33	32	33	34	34
Lower Vertical,	19	20	20	20	17	17	21	21	18	19	19	19	19	19	20	20	23	20	23	25	25	26	20	22	22	23	22	23	23
In. Lower Oblique,	19	18	18	17	18	17	20	16	20	22	17	18	18	18	23	24	25	22	22	22	25	24	26	22	24	24	24	23	23
Inner Horizontal,	21	22	21	18	20	18	15	17	19	25	19	19	20	20	23	24	25	22	24	23	24	24	28	26	25	24	25	24	24
In. Upper Oblique,	20	19	16	16	16	20	17	16	17	20	18	16	17	18	22	25	28	27	25	23	22	23	22	22	22	22	23	24	25
Upper Vertical,	15	15	12	12	15	15	13	13	14	16	13	14	14	14	17	17	20	17	19	18	21	20	19	19	17	18	19	18	18
Out. Upper Oblique,	17	20	16	18	22	21	16	18	22	23	21	19	19	20	25	27	25	28	24	23	27	28	23	27	28	23	22	24	25
Average Range,	21	21	20	20	22	22	20	21	20	22	21	20	20	21	27	27	30	28	27	27	29	29	28	27	29	27	27	29	27
Sex Average,	20						20.5						27.2						27.8										

THIRTEEN-YEAR-OLDS. CLASS II.

	MALES.						FEMALES.						Av. Range for every Meridian.		MALES.						FEMALES.						Av. Range for every Meridian.									
	A			B			C			D			E			F			A			B			C			D			E			F		
	R	L		R	L		R	L		R	L		R	L		R	L		R	L		R	L		R	L		R	L		R	L				
Outer Horizontal,	33	35	35	36	33		33	35	31	37	39	35	34	35		34	36		60	60	53	52	55	50	54	55	51	50	52	54	+	53	+			
Out. Lower Oblique,	30	27	31	26	32		29	31	30	32	30	32	32	31		29	35		35	42	34	35	36	42	42	41	40	42	35	32	37	39				
Lower Vertical,	20	20	20	20	19		18	19	19	16	20	17	21	18		20	25		22	26	26	25	28	24	23	23	32	31	30	27	+	27	+			
In. Lower Oblique,	20	18	18	19	21		20	20	19	20	17	22	21	20		19	24	28		24	28	29	24	27	30	28	26	27	25	26	26	+	27	+		
Inner Horizontal,	22	20	20	21	20		20	21	20	20	20	21	22	21		21	27	30		32	32	25	30	30	31	27	33	28	27	28	+	30	+			
In. Upper Oblique,	19	21	20	19	21		20	19	17	20	17	20	19	20		19	30	29		34	28	28	29	28	27	25	26	26	27	28	+	28	+			
Upper Vertical,	15	16	15	16	17		18	17	16	17	15	16	16	16		16	26	20		21	20	22	23	21	21	20	20	23	22	21	21	21	+			
Out. Upper Oblique,	21	20	20	21	22		19	22	20	21	23	22	24	21		21	35	30		30	33	33	30	35	32	28	30	25	32	31	31	31	+			
Average Range,	23	22	22	22	23		22	23	22	23	22	23	24	23		23	32	32		32	32	32	31	32	33	32	31	32	31	31.7	32	+				
Sex Average,	22						22.7						32.2						31.																	

The External Visual Field for Red.

The above chart represents the average results of three double tests of each individual through all the meridians as indicated. The letters A, B, C, D, E, F, represent the different individuals of each class. R, right eye; L, left eye.

TABLE IV.

SEVEN-YEAR-OLDS. CLASS I.		ADULTS. CLASS III.											
MALES.						FEMALES.						Av. Range for every Meridian.	
A			B			C			D			E	
R	L	F	R	L	F	R	L	F	R	L	F	R	L
23	27	25	27	25	25	24	28	21	23	20	20	23	25
23	24	23	24	22	19	23	25	21	19	21	21	22	22
14	14	15	17	15	13	18	18	16	17	17	17	16	16
15	14	11	13	14	13	13	13	12	15	15	14	13	14
17	18	13	18	15	15	14	14	15	18	15	13	15	16
17	16	12	13	13	14	14	13	14	13	13	13	14	14
11	11	11	12	12	11	11	12	13	11	13	11	12	19
15	16	14	15	17	17	14	15	18	17	17	14	16	16
17	17	15	17	17	16	16	16	17	16	16	16	17	17
Sex Average,						17-						16.5	
												25.5	
												25.6	

THIRTEEN-YEAR-OLDS. CLASS II.

MALES.		ADULTS TRAINED IN COLOR. CLASS IV.											
MALES.						FEMALES.						Av. Range for every Meridian.	
A			B			C			D			E	
R	L	F	R	L	F	R	L	F	R	L	F	R	L
32	31	35	32	33	33	32	33	33	32	33	31	33	32
27	27	28	28	28	25	31	28	30	28	25	29	28	28
17	18	18	17	18	16	16	18	16	19	18	19	17	18
18	17	17	18	19	15	18	17	16	18	17	17	17	18
18	20	17	19	17	19	20	18	20	18	20	18	18	18
16	16	18	17	20	20	16	18	18	17	16	18	17	18
14	15	14	15	17	14	17	17	16	15	14	14	15	15
19	18	17	17	20	16	17	17	19	20	17	16	18	18
20	21	21	21	23	20	21	21	22	21	21	21	20	21
Sex Average,						20.5						20.7	
												24.5	
												25	

The External Visual Field for Green.

*This column shows representative the average results of three double tests of each individual through all the meridians as indicated. The letters A, B, C, D, E, F, represent the different individuals of each class. G, H, I, J, K, L, M, N, O, P, Q, R, S, T, U, V, W, X, Y, Z, represent the different individuals of each class.

also represent the external visual field, and must be reversed in order to represent the retinal color fields. In the tables and diagrams the results of only four colors (R. Y. G. B.) have been represented, as these seemed sufficient to illustrate the point and less confusing than more colors would have been.

On examining these tables one is impressed with the general similarity of the fields in persons of different ages. The general form of the visual field is, in all cases, somewhat elliptical, but more circular in youth. The extent of the visual field is evidently larger in the adults, and so uniformly is this the case that it seems safe to answer the first question, "Can children see colors in indirect vision as far as adults?" in the negative.

Representing the average range for the adult eye for the four colors as 100, the average for the thirteen-year-olds would be represented by seventy-seven, and the seven-year-olds by sixty-one. This, expressed in retinal surface, would be in the ratios of 100, fifty-nine, and thirty-seven respectively. The greatest difference in the range was found with the blue and the least with the red. The order, beginning with the color showing the greatest difference and ending with the one showing the least difference, is blue, yellow, green, red. There is, however, one exception to this order in case of the seven-year-olds, where yellow presents a greater difference than even blue. (See table.) Perhaps this can be accounted for in the hesitancy of the children to speak as soon as they received a sensation of yellow. The observer was always requested to speak as soon as he received any sensation of color whether it should prove to be true to the color approaching or not, but the children soon learned that what seemed to be yellow, might, on approaching nearer, prove to be orange, red or green, and would often hesitate until they felt quite sure that it was yellow. The writer endeavored to overcome this cautiousness but was not entirely successful.

The question that now arises is: Why cannot children see color in indirect vision as far as adults? The discussion of this problem would require a paper in itself and must be deferred; however there are a number of points which present themselves. There are just as many degrees in the child's eye as in the adults, and so far as is definitely known, the retinal layer extends as far forward in the one as in the other. The difference in the convexity of the crystalline lens favors the child. We can hardly account for it through inattention, for the thirteen-year-olds seemed to be able to give as close attention to the approaching color as the adults, yet they

could recognize color only three-fourths as far. Again, the seven-year-olds could see objects 78% as far as adults, while they could see color only 61% as far. Although it is difficult to decide what the true cause is, yet the writer believes that the cause must be looked for in the order of the color development itself, whatever may be regarded as the true theory of color vision. Beginning with the visual color field, as seen by the adult, and passing backward toward and through the visual field of the child, we notice that the field not only decreases in extent but also becomes more circular. The same is true of the general visual field, *i. e.*, the visual field for light and shade, only the latter field decreases much less rapidly, or, in other words, shows a much greater proportional range in children than the color field. Now, should we continue still farther backward, and this law hold true, we should finally reach a point in the child's development where the eye ceased to be sensitive to color impressions; and from previous observations on young children, the writer would place this point not earlier than the fourth week of life, if as early as that. Perhaps no one will doubt the statement that the child becomes sensitive to light and shade much sooner than to color as color. A very young child may be attracted by a bright color, not on account of the color but on account of the light it contains. The above observations show that light and shade either develop faster in children than color, or else start sooner to develop, for while the range for light and shade of the seven-year-olds, as compared with adults, stands seventy-eight to 100, color, for the same individuals, stands as sixty-one to 100, linear measure. The writer very much questions whether his own children were able to perceive blue before they were eighteen months old, while they knew and seemed to enjoy red, orange and yellow very much earlier. If it be true that blue is the last of the colors to be perceived by the child, it becomes interesting to know why in after life blue has the greatest range of all the colors.

Judging from all the evidence at hand it seems probable that the child inherits from past generations an ever increasing color tendency, but nothing more, that he must come in contact with the real colors, or, in other words, the different color stimuli must play on the retina in order to develop this color tendency into a real mechanism for the discrimination of color, and that this mechanism begins to develop in earliest childhood, develops slowly and is finally completed in adult life. Further discussion of this point must be reserved for a future paper.

The second question was: Does sex have anything to do with difference in the range? There is a prevalent belief that woman has a better color eye than man, *i. e.*, greater inherent power of seeing and discriminating color, and that there are more color-blind men than women. The writer doubts, however, whether these beliefs are founded on facts. At least a carefully arranged and conducted experiment on 200 children (a number too small, perhaps, for accurate judgment) showed practically no sex differences in color vision. In the present experiment on visual range the data are entirely too limited to warrant any definite conclusions in the matter of sex, but the figures at least do not show that woman possesses any color superiority over man. In Tables I to IV, we find in Class III, designated adults, where there were three male and three female observers, that the average range for the males with blue was 47.4° , for the females, 45.7° ; the range of yellow, for males, 46.3° , for females 44° ; the range of red, for males, 27.8° , for females, 27.2° ; the range of green, for males, 25.5° , for females, 25.6° , or representing the average range of all colors for males at 100%, the average range for females would be 97%. Also in Class I (the seven-year-olds), where all the conditions were equally favorable to both sexes, we find again the males leading by 2%, or representing the average range of the boys for all colors as 100%, the average range for the girls would be represented by 98%. In Classes II and IV we find the conditions less favorable, for in each of these classes there were only two males and four females. Besides, in the class of thirteen-year-olds, where we should expect, if anywhere, to see a difference, the average age of the boys was nearly a year less than that of the girls, and this of itself may have been sufficient to account for the difference in the range in favor of the girls. Representing the average range of the boys as 100%, the average range of the girls would be 103%. In Class IV, where there were two men and four women, the average range favored the men by only 0.1%, or again representing the average range of the men as 100%, the average range of the women would be 99.9%.

These figures, so far as they show anything, show that man possesses not only equal power of recognizing the fundamental colors, but also equal retinal surface sensitive to color.

Let us now turn to the third and last question: What influence has color education on the extent of the range? To the answer of this question the writer brings the results of his observations on twelve different individuals, six of whom were selected from students of the university who had had

no special training in colors, and six others of equal age selected from the students and instructors of the Art Department who had had three or more years of special training in color.

In selecting the first class the author relied on his own judgment, but in selecting the second class he relied principally upon the judgment of Prof. Brown of the Art Department, who himself kindly submitted to the test, and endeavored to recommend only persons who showed special talent in color discrimination and in color harmony. The one class is indicated in the tables as "Adults, Class III," the other, as "Adults trained in color, Class IV."

Perhaps the best and only sure way to answer this question would be to select a number of individuals of the same age and of equal color range, giving to half of them three or four years of special color training, while the others were educated along other lines, without particular reference to color, and then take the color ranges again and compare. But in the absence of data gathered under these conditions, the present carefully made observations are of interest.

Table I shows that with blue the ranges for both the trained and the untrained were practically the same. With yellow, the range was greater for those untrained; while with red there was a decided difference in favor of the trained. The range for green was slightly greater in those untrained. Representing these in percentages and indicating the range of the untrained each time by 100%, we have the following results: The average range for blue in the untrained, 100%, in the trained, 100.2%; the average range for yellow in the untrained, 100%, in the trained, 97%; the average range for red in the untrained, 100%, for the trained, 115%; the average range for green in the untrained, 100%, in the trained, 97%. If the average range for all colors in the untrained were indicated by 100%, the average range for the trained would be 101.8%. This difference, although favoring those trained in color, falls within the individual variations of the same class, and is too small to be taken into account in tests of this kind. And judging both from the figures and the many impressions received during the time the tests were being made, the writer is forced to conclude that color education, as generally understood, has no influence on the color range. But is not this conclusion opposed to the one reached in answer to the first question? If correctly understood it is not. There are two kinds of color education, the direct and the indirect, the conscious and the unconscious, and it is on the indirect and unconscious sort of education, in which all share alike, that the widening of the color field depends.

That these incidental color experiences are necessary to the development of the range is shown by the general form of the color range itself. On the meridians which the nose and eyebrows shield from the color impressions, the retina is sensitive to color over only a few degrees as compared with the rest.

We mean something quite different, however, when we speak of direct color education. When an individual receives color instruction the colored objects are not brought to play on the periphery, but on a small spot at the centre of the retina, and here all color education or training, in the general acceptance of the term, takes place.

There is, therefore, no reason for thinking that those trained in direct color vision ought to see farther in indirect vision than the untrained.

Of other points of interest in this comparison of the trained and untrained may be mentioned the marked difference in the range for red in favor of the trained, of yellow in favor of the untrained, and the greater variety of shades seen by the trained, as well as the greater uncertainty in naming the true color. What color training really does, is to increase the number and variety of the color tones consciously recognized, but it does not increase the amount of retinal surface sensitive to color, and the greater the variety of color tones seen by an individual, the less accurate he becomes in naming the fundamental colors by indirect vision.

Another rather interesting fact was the not unusual occurrence of small color-blind surfaces in eyes otherwise apparently normal. These surfaces varied from 2° to 10° in breadth. Such a color-blind area can be seen in the Tables I to III, by referring under "Class IV" to the outer upper oblique meridian of the right eye of "F." In this case the color-blind area was so situated as to reduce the limit of vision on this meridian several degrees as compared with the other eye. The persistence and irregularity of these spots in some individuals was quite marked.

Again, in the examination of a person who was red-green color-blind, there were found small unequal areas in the eccentric retina of both eyes (near the normal limit for red and green), where all the fundamental colors could be correctly distinguished.

It appears, then (to sum up), 1st, that children cannot see colors as far in indirect vision as adults, but as compared with adults they show a greater proportional range for black and white than for color.

2nd. Difference in sex seems to make no perceptable difference in the extent of the color range.

3d. Color training does not seem to increase the color range (except perhaps in the case of red), but makes itself felt in a greater variety of shades and tints to the colors as seen in indirect vision, and less accuracy in naming the fundamental colors by indirect vision.

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STUDIES FROM THE PSYCHOLOGICAL
LABORATORY OF CORNELL UNIVERSITY.

COMMUNICATED BY E. B. TITCHENER.

VI.

TASTE DREAMS.

As long a moot question, to what extent dreams are caused by peripheral stimulation, and how large is the part played in them by the centrally excited idea. There is now to be a practically general agreement in the view that the field of hallucination must be minimized in favor of illusion. The influence of external stimuli upon the process of dreaming is probably universal. Thus the *Eigenheit* of the retina has been called upon to explain the pre-eminence of the visual dream ideas over those of the other

forms of all kinds, whether peripherally initiated or centrally initiated, imply the presence of a certain trend or direction of consciousness. The events of the day will suggest to the mind suggestible in certain principal directions.² It would seem that the "suggestion" must be stronger in the case of dreams which are mainly or exclusively centrally initiated, and the central excitation will only be possible where the process of waking thought is simply continued over, after a brief or less of interruption, into the dream life. The *psychic* or *Traum* will, therefore, naturally be the rarer type.³ A sensation following from an external impression shall suggest associated ideas along the line of least resistance is a matter of course; and the determining "suggestion" will not be particularly intensive. Somewhere between these

² Cf. *Vorlesungen*, 2d. Ed., p. 352; *Phys. Psych.*, 4th Ed., II, p. 115. Ladd, *Mind*, N. S., I, pp. 299 ff. *Die Schlaf- und Traumzustände der menschlichen Seele*, p. 213. *this JOURNAL*, V, p. 319. *Etc.*

³ Cf. Calkins, *l. c.*, pp. 331, 2. Wundt, *Phys. Psych.*, II, p. 540.

⁴ So, it is probably seldom found "pure." Cf. Calkins, *l. c.*, 4. I am not referring in the present paper to dreams caused by drugs.

two forms will come the dreams which result from auto-suggestion, from the "will" to dream or not to dream in a particular manner.¹ Here the validity of the volition will be altogether dependent upon circumstances.

Most of our dreaming is in terms of vision. Auditory dreams, especially those in which the auditory ideas are verbal, probably stand high in the order of frequency. Tone-dreams seem to be of rare occurrence. I have certainly dreamed in tonal ideas: *e. g.*, the *Preislied* in the *Meistersinger*. But I have no record, and autosuggestion has failed to induce a musical dream.² Dreams in terms of touch appear to be usually colored by cutaneous pleasure-pain, generally pain.³ Temperature ideas are not uncommon.⁴ Dreaming in terms of the organic sensations is, perhaps, only surpassed in universality by visual dreaming. We have dreams involving the respiratory sensation complex (suffocation, flight, etc.), the static sense (looking or falling from a height, etc.), sex, sensations from stomach and intestines, from the bladder (dreams in which the idea of water plays a part), and from the heart, muscular and movement complexes (resistance, fatigue, etc.), and so on.⁵ Such dreams are subject to a very curious objectification, which usually takes the form of translation into sight or hearing.⁶ Of course, in most instances, the dream ideas of the less frequent senses are found together with the more common visual or auditory ideas. Tones are sung by some person seen, heat is sensed amid certain visual surroundings, etc.—Taste and smell remain.

Wundt remarks that dream hallucinations of taste and smell occur but seldom.⁷ One reason for this is, probably, the difficulty of taste reproduction. "Memorial images of taste impressions are complications, in which the taste sensation proper is of but minimal intensity. It can be altogether replaced by movement sensations, for the reason that these (correlates of movements of mimetic expression) differ for different taste stimuli."⁸ So, too, the memorial representation of smell is composed principally, if not exclusively, of three disparate factors: the visual image of the odoriferous object, the sensation of movement in the nose (inspiration), and the

¹Cf. Nelson, this JOURNAL, I, p. 376.

²Wallaschek, *V. f. Mus. Wiss.*, 1892, pp. 233 ff.; Wundt, *Vorlesungen*, I, c.; Calkins, pp. 319, 322.

³Cf., *e. g.*, the cases cited by Ladd, *Psychology*, 1894, p. 412.

⁴In my own experience. Cf. Calkins, p. 319.

⁵Schermer, *Das Leben des Traumes*, p. 187.

⁶Wundt, *Phys. Psych.*, II, p. 539.

⁷In der Regel fehlen. *Vorlesungen*, p. 358.

⁸*Op. cit.*, p. 310.

touch-temperature complex occasioned by the inspired air. At the same time, this weakness of the true memory image furnishes no valid reason against the cropping up of the vicarious complex idea in the dream series. Taste-smell fusions form a fairly large part of waking conscious content; and the associative suggestiveness of smell impressions is well known.

Miss Calkins¹ found two gustatory presentation dreams in a total of 335 dreams; and four olfactory and no gustatory representation dreams in a total of 298 dreams. In the abstract of Professor Murray's paper, "Do we ever dream of tasting?" in the *Proceedings of the American Psychological Association*² there seems to be a confusion between the classification of dreams as presentative and representative (Calkins) and as illusions and hallucinations. The "representative" dreams include both illusion and hallucination ideas. A dream is no less a dream, because the peripheral sense organ is stimulated during sleep. As we have seen, "whether the central tract . . . can be excited by disturbances in the neighboring tracts without any peripheral stimulation" is a question which may be answered by a theoretical affirmative in the case of all the senses; but it is very doubtful whether, if we had accurate knowledge of the conditions, we should not find illusion to be the dream material in practically every instance; visual, auditory or what not. That is, it does not seem justifiable to single out the taste center as not centrally excitable, *because* it is so very easily excitable peripherally; the same holds of vision:— but there is every reason for supposing that the end organs of taste, like those of vision, are somehow concerned in the suggestion and formation of the dream idea.

During the present year I have collected five good cases of taste dreaming, no one of which is that of a presentation dream (Calkins).

1. On the evenings of January 22, 23 and 24 of the present year, I attempted to induce taste dreams by auto-suggestion. Every precaution was taken to avoid the occurrence of a presentation dream; the mouth thoroughly washed out, etc. There was no indigestion. The first two nights I was unsuccessful; but on the third a perfectly good taste dream occurred. It contained visual, auditory (speech), tactile, muscular (movement of self and others), temperature, affective (both pleasurable and painful), and conative elements, beside the gustatory. The taste dreamed of was that of

¹*L. c.*, pp. 319, 321.

²New York, 1894, pp. 20, 21.

English school plum cake. All the elements of reproduction were present; the visual and motor idea of breaking off fragments from a slice, the tactile sensations from their crumbling in the mouth, taste and aroma. The dream continued beyond the taste part of it. Jotted down immediately after waking, the dream record comprised 300 to 350 words.¹ The dream was of the morning class.² It was, however, a true sleeping dream.³ I had arranged to be awaked somewhat earlier than usual, in order to prevent the confusion of dozing with sleep proper, and was on the morning of January 25 aroused from sound sleep. On waking, I had the normal saliva taste in the mouth, which appeared on re-testing to be perfectly free from food fragments. The associative connections between the dream and events of the waking life were traceable with rather exceptional completeness. This may have been due, in part at least, to the fact of autosuggestion.

2. The second instance appears also to belong to the class of suggested dreams. It was recorded by the Rev. A. Beede, Alfred, Me., who had seen a notice of Professor Murray's paper in the *Philosophical Review*, and "resolved to watch for an opportunity of verifying" the occurrence of taste dreaming in his own experience. The dream took place in the night of Feb. 15, 1894. The mouth was clean. There was, perhaps, a very slight indigestion. The dream contained visual, tactile, muscular (movement of self), affective (both pleasurable and painful) and conative elements, beside the gustatory. The taste dreamed of was that of fresh strawberries; two good, one over-ripe. Reproductive elements present were: picking of the fruit and placing it in the mouth, taste (pleasant and unpleasant) and aroma, two acts of swallowing and (?) one of spitting out. The dream continued after the taste part of it. The letter of communication contains 350 to 400 words. The dream was of the night class. On waking, the taste in the mouth was, perhaps, not quite normal; this, like the presence of slight indigestion, is doubtful. The associative connections between the dream and events of the waking life were traceable with very considerable completeness.

It does not seem necessary to transcribe the remaining three dreams in detail. Each dream is reported by a different observer; and I have every reason to believe the reports trustworthy. No one of the three was autosuggested.

¹Nelson, p. 399.

²Calkins, p. 318.

³Nelson, p. 353.

These five are all "representative" dreams; and with the possible exception of two, as "hallucinatory" as dreams can well be. If the evidence be still regarded as unconvincing, I would propose that trial be made of autosuggestion. This can, of course, be done without any lapse into that form of the "psychologist's fallacy," against which Professor Murray cautions dream observers.¹

Postscript. Since the above paper was sent in to the Editor, Sept. 14, I have received accounts of three more taste dreams; two from new observers. No one of them was suggested. Since that date, also, there has appeared Professor Ribot's article, *Recherches sur la Mémoire affective* (*Rev. phil.*, Oct., 1894), which confirms many of my arguments.

VII.

ON THE QUANTITATIVE DETERMINATION OF AN OPTICAL ILLUSION.

(Continued.)

By R. WATANABE, PH. D.

On page 418 of the current volume of this JOURNAL, Mr. Knox writes, apropos of the dotted-line and point-distance illusion, as follows: "Binocular bisection of horizontal distances is not subject to any constant error; binocular bisection of verticals is subject to the constant error of over-estimation of the upper part of the field of vision. We should, therefore, expect to find the *m. v.* of our vertical Δ 's greater than that of our horizontal. The results [do not verify this expectation] . . . This is curious. We are unable to offer any explanation of the result."

Further experiments upon the illusion in question were made, in the hope of elucidating this difficulty. Every precaution that could be thought of was taken to ensure accuracy and avoid the intrusion of complicating factors. Mr. Knox' experiments were exactly repeated, with a single modification. Whereas, on his cards, the point-distance was constant, and the dotted-line variable, on our own the reverse was the case. We imagined that if this alteration in the nature of stimulus brought about any alteration in judgment, the latter would be of such a kind as to be readily determinable for itself; and that this determination, itself an interesting side issue, would not interfere with the realization of the main object of the new experiments. On the other hand, the stimulus altera-

¹P. 21.

tion might prove to be without influence upon the judgment process; in which case we should at least obtain the negative result.

Series were obtained from three subjects: Messrs. Knox (*K.*; see pp. 416, 419), Pillsbury (*P.*; see pp. 417, 419, 421), and Read (*R.*). The following tables correspond in every respect to those of pp. 415 ff.:

TABLE I.

Reagent *K.* Vision normal. Method (*c*) predominant. General and special practice. Unit = 1 mm.

SERIES.	<i>R.</i>											
	<i>C</i> =25	<i>n.</i>	<i>m.v.</i>	<i>C</i> =30	<i>n.</i>	<i>m.v.</i>	<i>C</i> =35	<i>n.</i>	<i>m.v.</i>	<i>C</i> =40	<i>n.</i>	<i>m.v.</i>
<i>C</i> - <i>V</i>	27.18	3	0.62	32.12	3	0.62	36.62	3	0.78	42.62	3	1.03
<i>V</i> - <i>C</i>	25.25	3	1.66	32.00	3	1.25	36.95	3	0.80	42.00	3	0.91
$\frac{V}{C}$	26.29	3	0.82	31.95	3	0.63	37.16	3	0.63	42.16	3	1.08
$\frac{V}{C}$	26.71	3	0.90	31.37	3	0.73	36.95	3	0.64	41.74	3	0.79
<i>Hor.</i> Δ	+1.21			+2.12			+1.78			+2.31		
<i>Vert.</i> Δ	+1.50			+1.66			+2.05			+1.95		

TABLE II.

Reagent *P.* Vision Normal. Method (*b*) predominant. General and special practice. Unit = 1 mm.

SERIES.	<i>R.</i>											
	<i>C</i> =25	<i>n.</i>	<i>m.v.</i>	<i>C</i> =30	<i>n.</i>	<i>m.v.</i>	<i>C</i> =35	<i>n.</i>	<i>m.v.</i>	<i>C</i> =40	<i>n.</i>	<i>m.v.</i>
<i>C</i> - <i>V</i>	25.08	12	0.81	30.89	11	0.56	35.78	12	0.66	40.70	12	0.79
<i>V</i> - <i>C</i>	25.40	12	0.69	31.65	12	0.58	36.58	12	0.80	41.92	12	1.06
$\frac{V}{C}$	26.48	9	0.57	32.84	9	0.65	37.81	9	0.72	43.60	9	0.91
$\frac{C}{V}$	26.44	9	0.86	32.40	9	0.68	37.37	9	0.85	42.61	9	0.83
<i>Hor.</i> Δ	+0.24			+1.27			+1.18			+1.31		
<i>Vert.</i> Δ	+1.46			+2.62			+2.59			+3.10		

TABLE III.

Reagent R. Vision normal. Special practice only. Method mixed.
Unit = 1 mm.

SERIES.	R.											
	C=25	n.	m.v.	C=30	n.	m.v.	C=35	n.	m.v.	C=40	n.	m.v.
C-V	27.10	6	1.27	31.39	6	1.60	37.26	6	1.39	41.89	6	1.17
V-C	27.49	6	1.08	32.83	6	1.32	37.53	6	1.48	42.49	6	1.41
$\frac{V}{C}$	27.37	6	1.27	32.29	6	1.51	37.66	6	1.57	42.41	6	1.47
$\frac{C}{V}$	26.72	6	1.37	31.25	6	1.33	36.83	6	1.22	41.74	6	1.33
Hor. Δ	+2.29			+2.11			+2.39			+2.19		
Vert. Δ	+2.04			+1.77			+2.24			+2.07		

Remarks.¹ (1) The illusion holds for every observer.

(2) Vertical Δ 's are larger than horizontal, in these twelve comparisons, in six cases; smaller in six. But in none of these contrary cases does the difference of the two Δ 's amount to half a mm. (Differences are: Table I—0.46, 0.36; Table III—0.25, 0.34, 0.15, 0.12.) Moreover, four of them come from the least practised reagent, R., who began with horizontal judgments. Mr. Knox' conclusion under this head is, therefore, confirmed by our results.

(3) The main object of the present investigation has been stated above. Do we find any light thrown upon the matter by the present figures? We have:

Table I. $r\ 0, = 2$ (.79, .63; .97, .93), $w\ 2$ (1.72, 0.86; .93, .68).

Table II. $r\ 0, = 4$ (.75, .71; .57, .66; .73, .78; .92, .87), $w\ 0$.

Table III. $r\ 2$ (1.17, 1.32; 1.29, 1.40), $= 2$ (1.46, 1.42; 1.43, 1.39), $w\ 0$.

In all, $r\ 2, w\ 2, = 8$. Mr. Knox obtained $r\ 6, w\ 7, = 7$; or, if his Table I be included, $r\ 8, w\ 8, = 8$. Massing, therefore, we get $r\ 10, w\ 10, = 16$. We do not insist upon the absolute relations of these figures,—apart from the fact that massing, even in two such comparable cases as these, is psychologically unjustifiable. Nor do we fail to note that of

¹See pp. 418 ff.

our own = 8, six have what tendency to differ they do have in the direction of w ; of Mr. Knox' = 8, seven have such a tendency. But we feel safe in formulating the following proposition: *so far as Mr. Knox' and our own experiments extend, there is strong evidence that, in presence of the dotted-line and point-distance illusion, the illusion of over-estimation of the upper half of the field of vision disappears*; the evidence being couched in terms of the $m. v.$ in vertical and horizontal quantitative determinations of the former illusion. We are not at present prepared to suggest any explanation of this fact. The fact itself holds, whether we employ the method with knowledge or the method without knowledge, and whether the reagent be practised or comparatively unpractised in *Augenmass* experiments.

(4) The values of $\frac{\Delta}{r}$ are:

- I. Hor.: $\frac{1}{20}, \frac{1}{14}, \frac{1}{19}-\frac{1}{20}, \frac{1}{17}$. Vert.: $\frac{1}{17}, \frac{1}{18}, \frac{1}{17}, \frac{1}{20}-\frac{1}{21}$.
 II. Hor.: $\frac{1}{104}, \frac{1}{23}-\frac{1}{24}, \frac{1}{29}-\frac{1}{30}, \frac{1}{30}-\frac{1}{31}$. Vert.: $\frac{1}{17}, \frac{1}{11}-\frac{1}{12}, \frac{1}{13}-\frac{1}{14}, \frac{1}{13}$.
 III. Hor.: $\frac{1}{11}, \frac{1}{14}, \frac{1}{14}-\frac{1}{15}, \frac{1}{19}-\frac{1}{20}$. Vert.: $\frac{1}{12}, \frac{1}{10}-\frac{1}{17}, \frac{1}{15}-\frac{1}{16}, \frac{1}{19}-\frac{1}{20}$.

(a) Table I is taken from the same reagent as Mr. Knox' Table III. The values of its limina are probably vitiated for the reason alleged on page 419. (b) Table II is taken from the over-practised reagent *P*. (See page 419.) Here, as in Mr. Knox' Table IV, the vertical Δ 's have suffered much less by practice than have the horizontal. (c) Table III, from a previously unpractised reagent, confirms Mr. Knox' general formula (p. 419) a good deal better than his own Table I, from a similar reagent, does. (d) The valuelessness of $C = 40$ mm. is indicated by Table III. We should not expect to find evidence of it in the other two tables. It is, perhaps, hardly necessary to make the explicit statement that these supplementary experiments were not at all expected to throw light on the magnitude of $\frac{\Delta}{r}$. The two reagents *K* and *P* were wholly unsuitable for such a purpose. On the other hand, the reasons that disqualify them for that investigation do not come into account for the main issue, discussed under (3). And it is, at least, satisfactory to note that there is nothing in the fractions which makes against Mr. Knox' conclusions. Thus, Tables I and II alike make the vertical $\frac{\Delta}{r}$ greater, on the average, than the horizontal; and the variation from this rule, in Table III, is so slight as

to be readily explicable in terms of the order of special practice (*cf.* Table I, p. 419). (*e*) Did the reversal of the *C* and *V*, as compared with those of Mr. Knox' investigation, influence the process of judgment? (i) The opinion of the reagents *K.* and *P.* was to the effect that it did not. (ii) If we compare the fractions obtained by the old and new methods, we find that those of the latter case are:

Table I. *Hor.*: $\langle \langle \langle \rangle \rangle$; *Vert.*: $\langle \langle \rangle \langle$,

Table II. *Hor.*: much $\langle = \langle \langle$; *Vert.*: \langle just $\langle = =$, as compared with those of the former. This general lessening was to have been expected, other things equal, from the increase of practice. Had the interchange of *C* and *V* had any influence, it would, we think, have been one in opposition to this tendency to lessen. For *a priori*, if there is any question of relative ease or difficulty, it should be easier to estimate when the dotted line varies (Mr. Knox' procedure) than when the point distance is the variable (our own). In the former case, an extension difference carries with it a quality difference, a greater or less number of dots: while, when the extension of the point-distance alters, no qualitative change is involved. The fact that the practice-lessening of the fractions is so little counteracted, therefore, in our results, tends to confirm the verdict of introspection. (iii) Moreover, the horizontal figures of Table III show, as has been pointed out, a very good agreement with Mr. Knox' formula. On the whole, then, we would answer the question of this paragraph in the negative. (*f*) Since the appearance of Mr. Knox' paper, there has been published in the *Zeitschr. f. Psych. u. Physiol. d. Sinnesorg.*, an attempt at a quantitative treatment of another optical illusion—that of the arrow head and feather (*F. Auerbach: Erklärung der Brentanoschen optischen Täuschung*, Vol. VII, pp. 152 ff.). The numerical results (p. 159) are not comparable with those given in the two present papers; the point investigated being not the quantitative variation of the illusion with variation of absolute magnitude of lines or distances, but its increase with increasing length of the limbs of the limiting right angles. But attention may be called to certain remarks of the writer's, bearing upon the general question. (i) The illusion varies with the *visual habits* of the reagent (p. 155). We have had no opportunity of testing this, in Auerbach's way. But the statement receives indirect confirmation from Mr. Knox' conclusions, p. 419 of this JOURNAL. (ii) Increased concentration of vision and attention diminishes the illusion (p. 155). This holds of the arrow head and feather illusion, for the explanation of which the influence of indirect vision is called into account, to a greater extent than for our own.

At the same time we have seen that a similar result may be obtained by familiarity and practice. (iii) It is necessary, for determination of the limen of difference, to avoid knowledge of the actual relations of the distances compared on the part of the reagent (p. 159). This point has also been insisted on by Mr. Knox. (iv) Judgment should be as immediate as possible; since it is apt to fluctuate, if the stimulus is present for any length of time (p. 159). Cf. the length of Mr. Knox' experimental series (p. 414). The method employed both by Auerbach and ourselves being a form of minimal changes, the necessity of immediacy of judgment is a matter of course.

VIII.

THE CUTANEOUS ESTIMATION OF OPEN AND FILLED SPACE.

BY PROFESSOR C. S. PARRISH.

A comparison of the spatial functioning of the cutaneous and visual sensibilities must always possess an especial psychophysical interest. The study of sensational intensities culminates in Weber's Law; that of sensational quality leads to a whole number of alternative psychophysical theories; the determination of the temporal attributes of sensation is one means of approaching the problems of the so-called time-sense; that of its spatial attributes, the first step towards a psychological space construction. But, whereas every sensation is possessed of duration, quality, and (with the exception of the visual series) intensity, a space attribute attaches exclusively to the sensations of sight and pressure. This fact, which seems at first sight to simplify the space problem, in reality renders that problem unusually difficult of solution.

Opinions differ very widely as regards the sensational factor in psychological space, as regards the interaction of eye and skin in its construction, and as regards the attributes and aspects of the cutaneous sensibility itself. It may, therefore, be well to give here, at the outset, a brief *credo*, not with any intention of dogmatizing, but merely with a view to clearness and intelligibility.

We believe, then, that the development of the eye, as a space organ, far outran that of the skin. That tactual space was, accordingly, built up under the influence of, and remains almost invariably subject to that of vision. Nevertheless, that there are two psychological spaces, and not one space. We consider, further, that the mechanical cutaneous sensi-

bility has (*pace* Dessoir) only one quality, that of pressure. That the cutaneous local signature is physiological only, although the retinal—as is indicated by many facts which cannot here be adduced—is psychological. And that in its ordinary functioning, the skin co-operates with the three deeper lying sensibilities, the tendinous (with its quality of strain), the articular (with its quality of pressure, and possibly with its own local signature), and the muscular (with its peculiar quality which only becomes seriously involved in fatigue or exhaustion). Finally, we ascribe to the cutaneous and visual sensation an attribute of extension, which we regard as co-ordinate with intensity, quality and duration, and which is by no means to be confused with the “bigness” or “massiveness” predicate of one school of nativistic psychologists.

After this preface we may approach the special problem which heads this paper. Almost unexceptionally, the eye regards a filled space as greater than an empty space objectively equal to it. What is the attitude of the skin to such spaces? It has recently been maintained that pressure *plus* movement functions, in this regard, as does the eye.¹ But one of the explanations propounded for the visual illusion is couched in terms of movement: it being argued that though the resting eye is also subject to the error, it is only so subject because, at some time or another, it has moved. The argument is paralleled by many others of the chapter of psychology which deals with visual perception, and need not be further commented on. Now, if the resting skin (*sit venia verbo!*) were deluded equally with the moving, then, although visualization might be called in to explain the fact, we should still be in presence of a phenomenon telling with more or less of force against the movement theory. If, however, the resting skin, in spite of visualization, and in spite of its own constant movement in the past, should prove to be not deluded, the movement theory is so far supported. Should the illusion be actually reversed, we must look for the conditions of such reversal in the special psychophysics of the organ.

The problem, then, resolves itself into that of obtaining comparative space judgments from the resting skin. If the skin, and the skin only, is to be appealed to, stimulation must be liminal. In this case, however, judgment will be uncertain, and comparison difficult. Since in ordinary life the static functioning of the skin is almost invariably correlated with a similar functioning of the deeper lying sensibilities—since, *i. e.*, normal stimulation is almost always supra-

¹Dresslar, this JOURNAL, pp. 332 ff. The view of this author is criticised below.

Method.—We imagined, at the outset of the investigation, that a very large number of experiments would be needed, if any satisfactory result was to be obtained. We therefore employed a modification of the method of right and wrong cases. Instead of taking too little-different stimuli, as that method requires, we proposed to compare each block with every other block, recording the judgments as r , w , $=$ and $?$ [r being used on the optical analogy, when the cutaneous judgment made a filled larger than an objectively equal empty space]; while, to avoid *Einstellung*, we did not confine ourselves to one pair of blocks in each series of experiments, but intermixed the comparisons at random. But the uniformity of which we were in search made its appearance so quickly, decidedly and unmistakably, that this original plan was not carried out. We have, consequently, only a relatively small number of experiments taken by this method, and those we propose to submit in detail.

In the following Table the first column gives the blocks compared, the numbers (as stated above) signifying the number of points in each block; while in the others, each of which is accredited to a different reagent, the signs ($>$ or $<$) gives the judgment of relation recorded N times out of n experiments. Thus, " $2:3 > 4:6$ " means that the two-point distance was judged greater than the three-point distance four times out of six experiments, by the particular reagent. Of the seven subjects, one only, ($Ti.$) had had general as well as special practice. His results, although not numerous, are the most reliable. The other six reagents were specially practised for the purposes of this investigation. It should be stated that during the first half (approximately) of these experiments, the application of the second stimulus was not to the exact part of the skin stimulated by the first, but to a line just alongside of it; during the second half the successive applications were made at precisely the same place. Absolutely no difference in result could be discovered; and irradiation makes this intelligible. Both sets of experiments have, therefore, been drawn upon in the composition of the Table.

Remarks.—(1) We notice at once that, for the resting skin, a filled distance is, on the average, shorter than an empty distance objectively equal to it. This holds for every reagent. In the earlier stages of practice, when visualization was especially insistent, there occurred sporadic cases to the contrary effect: the filled distance appeared longer. But such cases disappeared as practice progressed; and introspection referred them very definitely to the influence of the visual idea.

TABLE I.

	B.	Ha.	H.	O.	Ol.	T.	Tt.
2 : 3	> 4 6	> 2 2	> 2 4	> 5 6	> 4 5	> 7 11	> 4 4
4a	" 5 6	< 4 5	" 7 13	" 1 2	" 5 6	" 5 8	" 4 2
4b	" 3 4	—	" 3 4	" 5 5	" 4 5	" 3 3	" 3 3
5	" 4 6	> 5 6	" 4 6	" 5 7	" 3 5	" 5 7	" 5 6
6	" 3 4	" 3 3	" 4 6	" 4 4	" 3 3	" 4 6	" 4 6
7	" 2 4	" 3 4	" 3 4	" 4 4	" 2 3	" 9 10	" 10 10
8	" 4 6	" 1 2	" 5 5	" 6 7	" 5 5	" 3 4	" 3 4
9	" 2 4	" 2 2	" 4 4	" 5 6	" 4 4	" 4 4	—
3 : 4a	" 3 4	< 4 5	< 3 5	" 2 4	" 5 6	< 4 4	—
5	" 6 9	< 4 6	> 3 4	" 6 8	" 4 4	> 5 8	" 4 7
6	—	" 3 4	" 3 6	" 6 8	" 3 4	" 5 6	—
7	" 1 2	" 7 8	" 4 8	" 5 5	" 5 7	" 8 11	" 4 4
8	" 1 2	" 3 5	" 3 6	" 2 4	" 3 4	" 4 5	" 2 2
9	" 1 1	" 3 6	—	" 2 3	" 2 2	" 6 8	" 3 4
4a : 5	" 4 7	" 2 3	" 2 4	" 3 3	" 2 2	" 1 2	—
6	" 1 2	" 1 2	< 4 6	" 6 8	" 2 3	" 3 4	—
7	" 5 6	" 4 6	> 5 7	" 3 3	" 2 2	" 4 8	—
8	< 1 2	" 1 2	" 5 8	" 4 4	" 3 4	" 3 4	" 2 2
9	> 2 3	" 1 2	—	" 2 3	" 2 3	" 2 4	—
4b : 5	—	< 2 2	—	" 2 3	" 2 3	" 2 4	—
6	—	—	—	" 3 4	" 2 4	" 2 3	—
7	—	—	" 2 3	< 2 3	< 2 3	" 4 4	—
8	" 1 2	—	< 1 2	> 2 3	> 2 3	" 2 3	—
9	—	—	—	" 2 3	" 2 3	" 2 4	—
5 : 6	" 2 3	" 2 2	> 3 4	" 2 3	—	" 2 2	—
7	" 5 8	> 4 5	" 2 3	" 5 6	" 3 4	" 7 7	" 3 4
8	" 1 2	> 3 4	" 3 5	< 5 8	" 1 2	" 2 3	" 2 3
9	" 2 4	> 2 4	" 1 2	" 3 3	< 2 3	" 6 8	" 5 6
6 : 7	—	" 1 2	" 2 2	> 2 3	" 2 3	" 3 4	" 2 2
8	—	—	" 2 2	" 3 4	" 2 3	" 4 4	—
9	—	—	" 3 4	" 2 3	" 2 3	" 3 6	—
7 : 8	—	" 2 4	—	" 2 3	" 2 3	" 4 6	—
9	—	< 2 2	—	" 3 6	< 2 3	" 4 7	—
8 : 9	—	—	—	" 4 8	> 2 4	" 4 5	—

(2) There are a few exceptions to this rule. To understand them we must ask at once—taking the confirmatory evidence of Series II for granted—for the *reason of the reversal of the illusion*, in its transference from optics to haptics. That reason is given in terms of introspection by the reagents. All alike asserted that the points in the filled line were sensed as "bunched" or "crowded" together. The space between two points can be fairly accurately apprehended—we are not speaking, of course, of objective accuracy—but a space which is more or less filled "shrinks" together, and may be reduced to what are comparatively very small proportions. Doubtless, for the majority of the subjects, the first judgment was more influenced by visualization than was the second. But this is

an error common to almost all cutaneous experiments. And, though it may have aided the reversal of the illusion, it certainly could not have produced it. Moreover, the reagent *Tv.* has been able, by practice, pretty completely to separate the visual from the cutaneous judgment,¹ and the reversed illusion holds quite strongly in his case. The "bunching" or "crowding" is psychophysically explicable in terms of irradiation.

(3) Now for the exceptions. (a) $2 < 4a$ for *Ha.* Reference to the scheme will show that $4a$ may be regarded as an open line, doubly bounded at either extremity. This double bounding would make the point-impressions especially intensive. Now it proved to be a constant error in these experiments—one which evidenced itself in the practice series, in which experimenter was being educated as well as experimentee—that *increased intensity of pressure meant a judgment of increased length of line.* We did not attempt any quantitative evaluation of this error; the error itself was eliminated by the acquisition of facility and accuracy in the handling of the blocks. But we suggest that the error, in a modified form, may account for these exceptional judgments. (b) $3 < 4a$ for *Ha.*, *H.* and *T.* This is, again, easily accounted for. 3 is a filled line; $4a$ may be sensed as a doubly bounded open line. When this is the case, the judgment $<$ will follow, in terms of the cutaneous illusion. (c) $4a < 6$ for *Ha.* This we can only explain by supposing that the dots in the middle of 6 were crowded together in sensation, the terminal points being thus left free. (d) $4a < 8$ for *B.* Again, we have possibly a similar explanation. The two 4's of 8 are crowded, leaving the centre space free; $4a$ is more irregularly filled for sensation. Introspection gave a confirmatory result in both these cases. (e) $4b < 5$ for *Ha.* The two blocks are so similar, that any accidental and variable factor may have conditioned this judgment. (f) $4b < 7$ for *O.* and *Ol.* Here, again, the blocks are of like patterns. Perhaps the central dots of 7 were massed, leaving the ends free. (g) $4b < 8$ for *H.* and *T.*; cf. (d) above. (h) $5 < 6$ for *Ha.* The former is more uniformly filled. (i) $5 < 8$ for *Ha.*, *O.* and *Ol.*; cf. (d) and (g) above. (k) $5 < 9$ for *O.* This cannot be explained by reference to the blocks. (l) $7 < 9$ for *Ha.* and *Ol.* Nor can this.

We would call attention to the facts: (i) that these are very few exceptions, *when the fewness of the experiments in*

¹Proof of this statement will be advanced later, in articles by Dr. M. F. Washburn and Mr. W. B. Pillsbury, dealing respectively with the influence of the visual idea upon cutaneous space judgments, and upon cutaneous localization.

general is borne in mind ; (ii) that most of them are explicable in terms of the illusion itself ; and (iii) that they are by no means co-ordinate. For the discrimination of certain of the blocks, pretty thorough *practice* and very constant *attention* are necessary. Judgment becomes at once uncertain if *fatigue* has begun to set in. Not only is an accidental increase of pressure liable to be interpreted as an increase of length, but *vagueness or insecurity of judgment* (due to exhaustion, inattention, etc.) was also found to be so interpreted. Yet, in face of the fewness of the experiments and of all these sources of error, we see that *2 is judged greater than every other block*, except in one set of judgments from one reagent, with the misleading block 4a. We may remark, also, that the evidence from the experiments is stronger than that from the table ; since there occurred cases—not many, it is true—in which the judgment contrary to $>$ is not $<$, but $=$. Such cases have not been specially treated by us.

SERIES II.

In the second series of experiments a line was compared with a point-distance. The line was the impression obtained from the application of a strip of hard rubber, $\frac{1}{100}$ inch in thickness. The point-distance was given with the æsthesiometer figured on p. 422 ; the bulb being left unemployed, and the pressure regulated by practice. This was necessary, since we had no rubber strips, but only points, attached to the shaft of the instrument. The method followed was that of right and wrong cases. Here, again, the experiments, though not numerous, speak with complete decisiveness for the reversal of the optical illusion.

The first column of the table gives the reagent ; the second, the length of the filled line, in mm. ; the third the point-distances with which it was compared—the difference between each point-distance and its next successor being 1 mm. ; the

TABLE II. Unit = 1 mm.

<i>R</i>	Line.	Limits of Pt.-distances.	<i>n</i> .	Equivalence.
<i>B.</i>	28	28—23	15. 20. 20. 20. 40. 40.	Between 24 and 23
<i>Ha.</i>	20	20—15	30. 15. 35. 30. 35. 100.	15
<i>T.</i>	28	28—24	20 throughout.	Between 25 and 24
<i>O.</i>	28	28—23	17. 20.	" 24 " 23
<i>Ol.</i>	28	28	20 throughout.	" 23 " 22

fourth, the number (n) of experiments made with each point-distance; the fifth the point-distance which proved to be subjectively equal to the constant line-stimulus.

I. e., an open space of 24 mm. is equal to a filled line of 28 mm., and one of 15 to a filled line of 20. The different values of the line were taken owing to the fact that the limen of twoness for *B.*, *T.*, *O.*, and *Ol.* lay considerably higher than for *Ha.*

Table III shows the results of lines 2 and 5 of the above Table more in detail, and proves the point made just now—that the experiments speak very decidedly for the reversal of the optical illusion.

TABLE III a.

TABLE III b.

Reagent <i>Ha.</i> Rubber line = 20 mm.			Reagent <i>Ol.</i> Rubber line = 28 mm.		
Point Distance.	Judgment.	Per Cent.	Point Distance.	Judgment.	Per Cent.
20	>	88	28	>	100
19	>	83	27	>	97.5
18	>	85	26	>	90
17	>	75	25	>	90
16	>	80	24	>	77.5
15	=	88	23	>	77.5
			22	<	72.5

Further experiments might still further regularise the per cents.; they could hardly do more.

Conclusion. Literature.—We think the conclusion to be pretty obvious, that for the resting skin a filled line is shorter than an open space objectively equal to it. We have already suggested an explanation of this fact, in terms of irradiation, and specially directed visual association.

It has been asserted, quite recently, by Mr. Dresslar, that the illusion for touch is identical with that for sight. We would offer the following remarks: (*a*) In Dresslar's experiments it was not an open space that was compared with a filled, but a uniformly filled space (surface of a smooth card) which was compared with a discontinuously filled space (punctured card). It will be necessary to make experiments, both on active and passive touch, in this way: that a really open space be compared with a discontinuously filled

space. Till this has been done, the work of this experimenter must remain equivocal. (b) In active touch, the deeper lying sensibilities are involved to a much greater extent, and much more definitely from the qualitative point of view, than in our own experiments. We find no reference to this fact in Dresslar's introduction. (c) Whether we experiment with active or passive touch, the perception of movement is implied, and this is of itself amply sufficient to arouse the visual analogy. It seems hardly credible that Dresslar should not have come upon the visualization error in the course of his investigation. But we have not found any reference to it in his article. (d) Even for touch, as distinguished from pressure, there is evidence against Dresslar's results. James (*Principles*, II, p. 250—wrongly quoted by Dresslar as 242) declares that if the finger-tip be moved over a smooth and punctured card surface, the distances being objectively equal, the *filled* (i. e., punctured) *space is shorter*. Loeb (*Pflüger's Arch.*, XLI, p. 122—quoted by Dresslar as 121), it is true, found that an unevenly coated wire, drawn between finger and thumb, appeared longer than a smooth thread of equal length but somewhat less diameter, drawn at the same velocity. Loeb, however, calls especial attention to the effect of friction in this case. While therefore, his conclusion so far confirms that of Dresslar, the two experiments are not strictly comparable, nor is Loeb's at all exact. James is characteristically deficient in his description of his own experiments, but, so far as these are reliable, they stand in direct contradiction to Dresslar's. (e) A confirmation of our own results for the resting skin will be found in James' *Principles*, II, pp. 141, 142. The experiment cited is, however, very rough, and no numerical determinations are given. (f.) Dr. Nichols also supplies confirmation in his book, *Our Notions of Number and Space*, pp. 97, 105, 106, etc. . . . We believe that this investigation, as well as that of Mr. Dresslar, must be very carefully scrutinized and tested, before its results can be accepted. But this is not the place to enter upon a general criticism.

So far as it goes, our own conclusion, that for the resting skin the filled line is shorter, distinctly supports the theory that the key to the corresponding optical illusion is to be looked for in movement. It will be interesting to see whether more accurate experiments than those quoted upon active and passive touch confirm this view, or point to the necessity of its modification.

[Both of the above sets of experiments were well under way before Mr. Dresslar's paper appeared. Dr. Nichols' book, received by the *Philosophical Review*, August 4, was not seen till after their conclusion.—E. B. T.]

NOTE TO STUDY NO. V.

1. Certain correspondents, among whom is my friend Dr. Meumann, of Leipzig, have pointed out what they judge to be a defect in the Washburn aesthesiometer (see p. 422), and what must also be regarded as a defect in the very similar Jastrow model. It is this: that there is no guarantee of the simultaneity of the two impressions. I would urge that even if this be granted, the new instrument is better than the old, for it at least ensures constancy of pressure, which that did not. But I think that more can be said in its defence. Its form makes it easier to handle; and pressure made by it can be more readily controlled as regards the time factor, the attention not being distracted by the necessity of pressure regulation. And it certainly does not lie in wait for the experimenter with a constant error, as the sensibilometer does. If the skin, in the place worked on, is perfectly flat or of symmetrical curvature, the bulb might with advantage be held in a fixed support, and the part played by the hand be confined to that of regulating the time of impression and of release from stimulation. But where this is not the case, regulation of simultaneity by hand seems desirable. I do not know of any other control than those of the vision of the experimenter, and introspection of the experimentee. Of course the rubber points could be made to pass through the bar that holds them, and their length as regards one another be regulated by the skin curvature in the experimental series; but this alteration would introduce one of the faults of the sliding-scale form of the instrument—and would not the controls, after all, be then precisely what they are now?

2. Dr. E. W. Scripture and myself have devised an improved arm-rest for the new model kinesimeter. Cuts of this and of the instrument itself will appear in the next number of the JOURNAL.
E. B. T.

THE DAILY LIFE OF A PROTOZOAN: A STUDY IN COMPARATIVE PSYCHO-PHYSIOLOGY.

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The life of an animal, as we attempt to study its physiology, appears to consist of a fabric of interwoven rhythms. Circulation, respiration, daily activity and rest, as well as reproduction and to all appearances many processes of nutrition, muscular contraction and the transmission of nerve impulse, all have come to take the form of waves or rhythms, which differ greatly in period and at certain points are interdependent. The waves of external nature, into the midst of which an animal's life is cast, no doubt tend to cause rhythmic responses on the part of the animal. This is well exemplified in the evident relation between seasonal and lunar periods and reproductive rhythms, and in the rest or activity of day and night. That external changes are not the sole determinants of physiological processes is amply demonstrated, however, by the fact that different animals living under similar environment, possess widely different rhythms. Certain controlling factors must, therefore, be sought for within the animal itself, and clearly these must be closely related to physical structure and endowment. In other words, we could not expect an organism to respond to stimuli unless it possesses mechanisms by which the stimuli may be felt. Or, conversely, if an animal responds to changes in external nature we must suppose the existence of mechanisms for their perception, although specialized structures may not have been demonstrated. To ascertain to what extent physiological processes are in fundamental character rhythmical and to be able to learn approximately what normal rhythms are, will require continuous observation of a series of animals, each for a considerable period. A series of such observations would naturally begin with the simplest animals, the protozoa.

While taking other rhythms into careful account, the present research had for its primary object a study of the rhythm of rest and activity in one of the protozoa. Since in these simple organisms, consisting of but a single cell, there are found all the important physiological, and, for all we are able to observe, types of all the psychic processes which take place in the life of one of the higher animals, it would seem possible to observe directly all the steps in the fatigue of gland, muscle or nerve which are demonstrated in more complicated bodies by indirect methods. We should be able to see, during a period of rest, zymogen granules forming and being stored up, the body grow, and possibly the nucleus increase in size. Following this, if the life of a protozoan is similar to that of higher animals in these respects, we should have a period of activity, in which new food is secured, while the body is emptied of its formed materials. If the life of a protozoan is found to be cast on rhythms similar to those of higher animals, the fact will be most remarkable, since their physical equipment is so different. If protozoan rhythms prove to be strikingly dissimilar, in what ways may such peculiarities be correlated with differences in structure?

The work here described was done in the physiological laboratory of Clark University, in the fall of 1893. For much assistance in construction of apparatus, we wish to express our thanks to Mr. J. R. Slonaker.

Among the numerous protozoa available, the *Vorticella*¹ was chosen for two reasons. First, being attached permanently by its stalk, a specimen can be retained in the field of a microscope for days without difficulty. This is, of course, a prime condition of the experiment. Second, the animal is active, its movements are well defined and easily observed and some of them appear to be clearly automatic, others, purposeful and selective. These movements may be classified as follows:

AUTOMATIC.

1. Contraction of vesicle.
2. Ingestion of food balls.
3. Ejection of detritus.

PSYCHO-REFLEX.

1. Contraction of stalk, with attendant closure of bell.
2. Vibration of peristomal cilia.
3. Sorting of particles by the the sensory cilia, the driving of food toward the mouth, and the driving away of waste particles.

¹Descriptions of the *Vorticella* are so readily accessible in all manuals of biology and zoology that no attempt to describe it here is deemed necessary.

Objection may be raised to placing ciliary activities upon the psychic side, and it is true that the action of cilia in various parts of the human body could not be considered of such character. On the other hand, the work of the cilia in Vorticellæ seems to be at every point more complicated than that of ordinary cilia. By their movements currents are set up with the apparent purpose of drawing food within reach. When a particle is touched by the cilia, an act of choice is apparent, and in accordance to this choice the particle is carried toward the mouth or whirled away. This process would seem to indicate no less conscious wakeful action on the part of Vorticellæ than the seeking of prey and the feeding of animals in general. No account is taken of the extension of the stalk, since on the view that this is due entirely to elasticity of the cuticle it is a purely mechanical action.

Apparatus was devised to record the occurrence of all these phenomena. It consisted of a continuous-roll kymograph with eight capillary glass pens arranged vertically across the paper. The lowest pen was connected with an electromagnet and the hands of a clock so as to register hours and minutes. Two other pens registered temperature and barometric pressure, and the five remaining were attached to tambours in such a way that the observer could record various activities by a touch of the fingers of the right hand, without taking his eye from the microscope. The first recorded contractions of the stalk; the second, contractions of the vesicle; the third, ingestion of food particles; the fourth, ejection of detritus; the fifth, reproductive phases. The observer's left hand remained free to adjust the focus of the microscope.

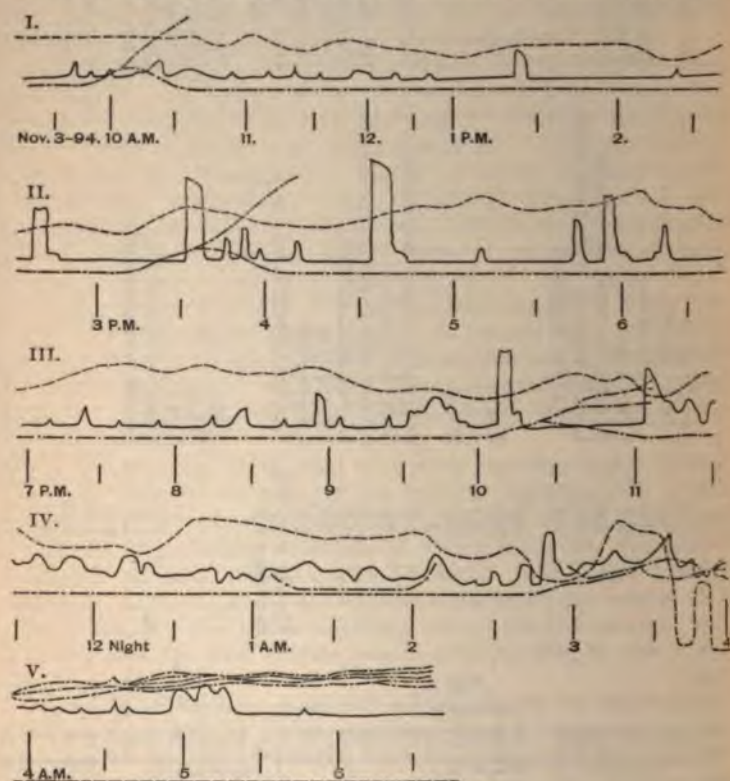
The microscope used was a Zeiss, apochromatic series, ocular 6, objective 4 mm., which gave a magnification of 375 diameters. In order to keep the Vorticellæ under conditions as normal as possible, a stream of water from an aquarium, in which various plants were growing, was kept flowing under the cover-slip. This was accomplished by means of a glass syphon, drawn to a capillary point, placed at one side of the cover-slip, and a filter-paper drip applied to the other side.

The first Vorticella observed corresponded to the species *gracilis*, as indicated in W. Saville Kent's "Manual of the Infusoria." It was observed, without a moment's intermission, between 8 and 8.30 and between 9.30 A. M., Nov. 3 and 6.30 A. M., Nov. 4, and every contraction of the stalk or vesicle registered upon the kymograph paper. The following table shows its life history for the whole period of observation. The times given in the first column are usually

Date.	Time.	Contraction of Vesicle per min.	Contraction of Stalk.	Remarks.
Nov. 3,	8.00 A. M.	3	8 in 30 mins.	No rhythm.
"	8.30 "	2.8	1 in 8 (about).	Division begins.
"	9.30 "	2.6	1 in 8 (about).	Two vesicles visible.
"	10.00 "	2.6	1 per min.	Division complete.
"	10.13 "	2.6	Several in rapid succession.	
"	10.33 "	2.6	Less frequent and less violent	
"	10.35 "	2.6	after division.	
"	11.35 "	2.6		
"	11.55 "	2.6		
"	12.00 M.,	3.2	Rare.	
"	12.15 P. M.	2.4		
"	12.52-1.50	2.4	None.	
"	2.00 P. M.	2		
"	2.50 "	2.4	Several in rapid succession.	No stimulus apparent.
"	3.15 "	2.2	None in 25 mins.	
"	3.33 "	2.8	2.8 per min. (about) for 5 mins.	Second division begins.
"	3.42 "	2.8	1.5 per min. (about).	Two vesicles visible.
"	4.11 "	2.8	1.5 per min.	Division complete.
"	4.50 "	2.8	1.5 per min.	No observable stimulus.
"	5.00 "	Slowly varying	Varying irregularly, rare.	Gas light. No visible change in contrac-
"	to	between 2.2		tions of stalk or vesicle.
"	10.00 "		None.	Third division begins.
"	10.21 "		None.	Subdivision of daughter cell begins.
"	10.35 "		None.	Further subdivision of daughter cell begins.
"	11.00 "		None.	Daughter cells detached.
"	11.05 "	3.2	Rapid contractions.	Free-swimming zoid in contact.
Nov. 4,	1.15 A. M.	2.7	Feeble contractions.	
"	2.10 "	2.4		Vesicle much swollen.
"	2.12 "	1.6		Free-swimming zoid becomes attached.
"	3.06 "	1.68		
"	3.15 "	3		Vesicle suddenly divides.
"	3.25 "			The two vesicles and that of the free-swimming zoid all break into one which con-
"	3.46 "	1.5		tracts five or six times, at the rate of 1.5 per minute. After this no more
"	3.52 "			contractions of vesicle can be seen. Following this a curious succession
"				of partial divisions and reunions occurred, lasting until 6.30 A. M. See Figure.
"	5.44 "			Last contraction of stalk.

those at which greatest changes appear in the rhythms recorded.

The course of this *Vorticella*'s life may be seen graphically represented in cut below. The lines in this figure are plotted directly from the kymograph record, but to avoid complication the coördinates, excepting designations of time in hours and half hours, have been omitted. The continuous line represents stalk contractions. For somewhat more than half its extent it is at its lowest level, indicating that no contractions occurred during that time. Elevations are drawn proportionate in height to the number of contractions occurring during the time traversed. The broken line indicates in a similar way frequency of vesicle-contractions, the higher the



Experiment 1, *Vorticella gracilis*. The continuous and broken lines represent frequency of stalk and vesicle-contraction respectively. The line of dots and dashes indicates reproductive phases. For further explanation, see Table above.

curve, the more frequent the beats. It reaches its zero point when the vesicle ceases to contract. The line of dots and dashes represents reproductive phases, its rise and division indicating increase in size and division of the Vorticella. The time at which one of the bells detaches itself from the stalk upon completion of divisions is marked by the termination of the corresponding line. Conjugation is indicated by the approach and union of a similar line.

No account has been taken of movements of the cilia, for the simple reason that they are not a variable in the animal's life. During the whole period of observation the cilia were working incessantly, drawing particles toward the mouth, sorting them, ingesting food and driving away excreta and debris. An apparent exception to this statement occurs at the instant of a stalk contraction, when the bell is also contracted into a sphere and the cilia are drawn in; but during this operation there is only time for the cilia to fold in and open out again, and hence there is nothing which could be construed as a period of rest. The same statement applies to the results of ciliary activity, viz., ingestion of food and ejection of detritus. In other words, so far as our observation goes, there is practically continuous action of cilia and, in consequence, a practically constant stream of particles both into the body and out from it.

As stated above, the temperature and barometric pressure were traced upon the kymograph paper along with the Vorticella's activities. This was done in order to ascertain whether these physical factors had any influence similar to their influence upon the tissues of higher animals. These tracings are omitted from the chart and from further consideration, because no hint of any connection or causal correspondence could be made out. As far as temperature is concerned, sudden change from ice water to water at room temperature (20-22 C.), or vice versa, never was observed to act as a stimulus sufficient to occasion stalk-contractions, nor did the rhythm of the contractile vesicle appear to be influenced in the slightest degree. This would seem to indicate that a vorticella is not endowed with even the rudiments of temperature sense. It is more difficult to disprove the influence of barometric pressure.

In all, fourteen experiments were made, similar to that just described, lasting from a few hours to five and a quarter days. Observation in all these experiments, especially the longer ones, was not continuous, though it was frequent during both day and night. The presence of several species of Vorticella and of Carchesium was utilized to add variety to the experiments. Farther than showing the physiological fact that the

rhythm of the contractile vesicle is somewhat different in the different species and tends to vary in a similar way under the same conditions, these experiments are simply confirmations of the first.

In Experiment 3, *V. gracilis*, observation was continued during the entire process of conjugation. Not the least change in the movements of the cilia, the taking in of food, etc., could be noted. Stalk contractions were frequent. During the hour in which the process was completed (9.42-10.48 A. M.) the vesicle-contractions gradually decreased in frequency from 8 to 2.6 per minute. Shortly after conjugation (11.09 A. M.) the stalk remained closely contracted and the bell detached itself and its movements could be no longer followed.

In later experiments by attending more carefully to food supply and by preventing as far as possible the growth of mould and bacteria and keeping the stream of water under the cover-slip as clean as possible, we were able to keep the *Vorticellæ* in apparently much more normal condition for a longer time. In spite of all efforts and precautions, however, mould and bacteria sooner or later overran the field and either killed the *Vorticellæ* or compelled them to migrate. We attempted to obviate this difficulty by sterilizing the water supply, and by boiling and covering antiseptically, at the same time giving in the place of their normal food a pure culture of yeast plants. This attempt resulted in an interesting demonstration of the educability of *Vorticellæ*. At first they took this, to them, newly discovered food with great avidity, filling their bodies to distention with food vacuoles of the yeast. In a very few minutes, however, the entire meal was ejected with volcanic energy. Not a single torula was allowed to remain in the body, and for several hours at least—how long the memory lasted was not determined—the individual could not be induced to repeat the experiment.

Experiment 11 was continued for two days and was terminated by accident. Experiment 14, upon *V. campanula*, lasted five and a quarter days and leaves no ground for doubting the truth of our main conclusion, viz., that a *Vorticella* works continuously and shows in its life no period of inactivity or rest corresponding to periods of rest in higher animals. In other words, a *Vorticella* never sleeps.

During the five days, frequent observation both day and night failed to detect any considerable relaxation of apparent effort or attention. The cilia worked uniformly, drawing in food particles and sorting them. In fact all efforts to surfeit the tiny animals with food produced no appreciable effect in satisfying their apparent hunger. Division occurred

nently, but only in rare cases did this cause any noticeable relaxation of other work. Occasionally for a few moments during the act of division the bells became nearly spherical, their cilia worked feebly and in one instance ceased vibrating altogether. This had the appearance of a momentary "rest" but its occurrence was a rare exception to the rule of continuous work throughout the process of division. No change of conjugation occurred in this experiment and this suggests a point of importance that has not as yet received attention. Under certain conditions a *Vorticella* passes what is known as the encysted state, in which the bell becomes spherical, detaches itself from the stalk and secretes a cyst. It now is said to "rest" or "lie dormant" through a period of such unfavorable conditions as dryness or cold, and under circumstances favoring activity again return, it bursts the cyst, not as a single zoid, but as a number of small free swimming zoids. The stage of rest or encystment is thus a period of reproduction. Each of the minute *Vorticellæ* encysts itself, develops a stalk and grows to the normal size of the species. Just the bearing which this phase of a *Vorticella*'s life has upon the problem of rhythmical periods of rest and activity our experiments do not determine. So long as they yield any evidence, they support the view indicated above, which is generally adopted, that conditions unfavorable to life cause this mode of reproduction, encystment and encystment. Encystment is a means of protecting the animal from changes in its environment which would otherwise prove fatal. Upon this supposition, as long as conditions of life remain favorable, a *Vorticella* might continue to live and work and reproduce by division indefinitely without the intervention of a "resting" stage. Encystment is therefore of the nature of an enforced "rest," a period of inactivity imposed by exceptional external circumstances; therefore has no bearing upon the problem in hand. During the course of this experiment, careful tests were made of the *Vorticella*'s sensitiveness to vibrations of sound and light. No one can watch a *Vorticella* for an hour without being struck by its exceedingly delicate sense of touch. The slightest jar is instantly answered by a quick contraction of the stalk, and particles scarcely visible under the microscope are sorted with the greatest apparent precision. No less striking throughout all the experiments was a *Vorticella*'s insensibility to all other stimuli. No reaction could be elicited by changes in light or to sounds of any kind so long as they were unaccompanied by perceptible jarring of the microscope. Musical sounds of all qualities and volumes were tried, but without effect. Bright sunlight was

flashed upon the creatures from total darkness; this was varied by interposing colored glasses, red, green, blue and violet, between the mirror and the stage of the microscope; each light was allowed to act for minutes at a time and was also tried in a succession of quick flashes, but not the least evidence of sensation could be detected. Thus, so far as we can judge, the universe must consist for a *Vorticella* of a series of touches, possibly also of tastes and smells; but not to any extent of sights and sounds.

Correlating now what we have learned concerning the activities of this animal with the anatomical structures at its command, we remark first that a *Vorticella* consists for the most part of a mechanism for digesting food. Supplementing this is a motor mechanism beautifully adapted for securing it. Material thus obtained and assimilated causes the body to grow to a certain size, but when this limit is reached the body divides instead of enlarging indefinitely. A prime condition of the creature's life must be ability to distinguish food from what is not food. This it is able to do sufficiently by the sense of touch, and the ciliary mechanism which mediates this sense is precisely similar to tactile and sensory "hairs" as they exist throughout other parts of the animal kingdom. But like any other animal a *Vorticella* must be able to escape from its natural enemies.

The experiments afford evidence ample to prove that this is the chief purpose of the stalk and its contractions. Several earlier experiments were suddenly terminated by a "monster" appearing in the field and snapping off the *Vorticella*'s bell. In a number of cases contraction of its stalk actually pulled the bell out of a devourer's mouth. The particular enemy observed was a minute white worm hardly more than visible to the unaided eye. A necessary preliminary to later experiments consisted in carefully teasing all these animals out of the preparations. The sense by which a *Vorticella* is made aware of the approach of its enemy is touch. At least a *Vorticella* was never observed to react until its cilia were actually touched.

With food in abundance, capable of yielding a continuous supply of energy, it is a strange physiological paradox that all animals should not be able to work continuously. For any of the higher animals, at least, this is not possible. A certain amount of activity produces fatigue, and fatigue makes necessary a period of rest. Fatigue is commonly explained upon two assumptions. The first of these is that decomposition products arising from activity of the tissue are not removed quickly enough to avoid poisoning or clogging the organism. The second assumption is that highly organized ma-

als are consumed during action more rapidly than they formed. So generally are these processes present in the tissues of animals usually studied that we are apt to consider fatigue as a universal characteristic of living matter. It seems only reasonable, however, that protoplasm may be renewed as fast as used under favorable conditions of nutrition, and that with equally good facilities for the removal of composition products, these may not accumulate in amounts sufficient to interfere with activity. So far as we are able to interpret the significance of our own experiments, this is the case of things in a *Vorticella* under favorable conditions of life.

MINOR STUDIES FROM THE PSYCHOLOGICAL
LABORATORY OF CLARK UNIVERSITY.¹

Made under the direction of

EDMUND C. SANFORD, PH. D.

VIII. A STUDY OF INDIVIDUAL PSYCHOLOGY.

BY CAROLINE MILES.²

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Great as have been the contributions of the laboratory to recent psychology, many most fascinating and important problems as yet resist experimental solution. For the study of these the investigator is thrown back upon introspection and observation, and, so far as his introspection is to have extraneous confirmation, upon the questionnaire.

To ask questions is easy, but to make the questionnaire an instrument of precision is very far from easy. It has more ways of going wrong than the chronoscope, and is in as great need of careful study. To say nothing of the general difficulty of selecting truly cardinal points for questioning about, and the special rhetorical difficulty of framing questions that shall be perfectly clear as to the information required without at the same time prejudicing the answers to be received, there yet remains the difficulty of assigning their proper weight to the answers received. How much dependence, for example, can be placed on the inner observations of people of intelligence, but untrained in introspection? How fully does what can be recalled at the time of answering represent the total experience of the answerer on the matter in question? What allowance must be made for influences that might unconsciously mold the answers, — aversion to displaying anything of the inner life on one hand and egotistic interest in one's own experiences on the other, distrust of the questioner or desire to

¹ Continued from Vol. V, p. 389.

² Received as a private pupil during the winter of 1893-94.

and well in his estimation? Some of these sources of error must be avoided, some must be recognized and allowed for and one must forbid the use of the method except under uncommon circumstances. On some of these points the questionnaires already put forth by various investigators have thrown light, and the following report of a study by this method is presented as much for what it may contribute to the psychology of the method as for the facts that were elicited by it. The questions asked were as follows :

(1) How do you know your right hand from your left? (2) Do you ever hesitate for a moment as to which it is? (3) Are there any two things that you persistently tend to confuse, such as *ei* and *ie* in spelling, or the Guelphs and Ghibellines in history? (4) Have you mastered any such tendencies and how? How do you recall a forgotten name?

(1) How do you concentrate your mind with all your might on some one thing, *e. g.*, in playing the parlor game of "mesmerism"? (2) How do you force yourself to work when you do not want to? (3) How do you pay attention to a dull lecture?

How do you go to sleep when sleepless?

(1) What things were you afraid of as a child? (2) Were you ever frightened by these things? (3) How did you overcome your fear?

Mention a good ghost story, *i. e.*, something that gives you the creepy feeling supposed to characterize ghost stories

Mention several concrete instances of things that have made you angry—ten if possible.

(1) What is your favorite color, *i. e.*, what color appeals to you most apart from any colored thing—merely as color sensation?

(2) Why do you like this color? (3) Has this color any association with persons, places, music, poetry, emotion, odor, taste?

Did you express yourself in any art form before eighteen years of age?

What were your favorite games when a child?

(1) What is the earliest thing you are sure you can remember?

(2) How old were you?

Mention some story that has made you weep—the most pathetic you can think of.

Mention a funny story, incident, joke or scene in a book or play—the funniest you know, if possible.

What characters in history, fiction or life were ideal to you when growing up?

If you had just one sermon to preach what would be your text?

These questions were asked of 100 Wellesley women during the winter of 1893-94. The persons were taken at

This game has several forms and many names. The essential feature of it, however, is that one member of the company withdraws while the remainder select some object which he is to find or the act which he is to perform on his return. When he re-enters the company endeavor to assist him by intent concentration of mind on the object or act selected.

random and the number included seventy-one students and twenty-nine members of the faculty. At first the questioning was verbal and the entirely naïve answers were noted down by the writer. These results were then examined and two months later other questions suggested by them were added to the original set and a printed copy was sent to the same 100 persons with the request that new answers be written without regard to the former answers. Ninety-seven persons responded, and the following report is based on a tabulation of these written answers. Comparison with the first naïve answers has also been made when it promised anything of interest.

The questions were asked in irregular order, but will be discussed in five rough groups: I. Habits of Discrimination and Memory (*A* and *B*). II. Method of Concentrating Attention and of Getting to Sleep (Extreme Distraction of Attention) (*C* and *D*). III. Emotions and Preferences (*E*, *F*, *G*, *H*, *I*); *E* strictly belongs to group IV, but for purposes of comparison with *F* it has been placed as it stands. IV. Recollections of Childhood (*J* and *K*). V. Miscellaneous Questions More or Less Unsatisfactory (*L*, *M*, *N* and *O*).

I.

Habits of Discrimination and Memory.

A. (1) Question: How do you know your right hand from your left?¹

Replies on second questioning (97 cases): Thirty-three (33) mentioned some association, such as writing or eating, the position of the heart, some actual difference in the hands, as a ring or scar, or the conditions in which the respondent was when she first learned her right hand. Twenty-seven (27) replied that there is a distinct difference in feeling, which they describe as readiness, skill or strength. Thirty-seven (37) called their method merely "instinct," and could not define the difference further.

Replies on first questioning (100 cases): Thirty-seven (37) mentioned an association, thirteen of them mentioning some actual difference in the hands. Thirty-three (33) said it was a feeling of difference, three stating that the difference extended to the whole side. Thirty (30) called it "instinct," or replied "because I was told," without further specification. The greatest change in the records of the two questionings is the decrease of those that tell by a feeling of difference and the increase of those that tell by "instinct."

¹ For this question the writer is indebted to Dr. Clarence Blake of Boston.

Such a change is not surprising considering the indefinite character of the feelings of difference reported, a difference in readiness is hardly to be distinguished from an instinctive difference in the loose way in which the word was used. The three persons that fail to appear in the second questioning belonged one to each class.

(2) Question: Do you ever hesitate for a moment as to which it is?

Replies on second questioning (97 cases): Fifty-seven (57) replied Never. Forty (40) replied Yes. This is probably too small a proportion for those liable to this confusion. Of the forty that replied Yes, nearly a quarter (9 — all students) gave such instances as drill and setting tables, as in Wellesley domestic work, but not all of the students questioned (71) had had drill or setting tables, and some therefore had missed these opportunities of finding their liability to confusion.

A comparison of the answers to (1) and (2) shows an interesting relation between the way of telling the hands and liability to confusion.

Of 33 who tell by association:	24 are sometimes confused.
Of 27 who tell by a feeling of difference:	10 are sometimes confused.
Of 37 who tell by "instinct:"	6 are sometimes confused.

This relation furnishes another evidence of the greater reliability of muscular as compared with associative memory, and of unconscious as compared with conscious memory of either sort. The difference, however, is probably a good deal less than the figures seem to show, for the persons that suffer confusion and so have to recall consciously which their right hands are would be on that account the more able to give a definite account of how they know, when presented with a questionnaire. Both tendencies work in the same direction: those that are uncertain are familiar with how they find out, and those that find out by conscious means are apt to be uncertain.

Of the 97 persons asked, 43 remember how they learned and 54 do not. Twelve now tell the hands by recalling the way in which they learned; eleven of these belong to the forty-three who remember and one, whose knowledge came in some other way, to those who do not remember.

The fact that the method of learning still persists as an association may be due to a very good memory or it may be due to learning very late. Nine of those who distinguish by an association mention learning after they started to school. The average age of the earliest memories (see question *K* below) of the two classes seems, however, to favor the former hypothesis. The age to which the earliest memories of those

who employ association belongs averages three years; that of those who know by instinct averages three years nine months; that of those who know by difference in feeling averages three years seven months.

The tendency to confuse the right and left hands called attention also to the tendency to confuse other pairs of things that have few points of difference or in which from any reason the distinction seems arbitrary.

(3) Question: Are there any two things which you persistently tend to confuse, such as *ei* and *ie* in spelling, or the Guelphs and Ghibellines in history?

Replies (97 cases): Seventy-five (75) were conscious of more or less tendency to confusion, some saying that they confused everything that could be confused. Twenty-two (22) were not conscious of any such tendency. The list of examples given by the seventy-five may be classified as follows, beginning with the most arbitrary distinctions: (a) *Spelling* and other confusions almost as arbitrary: e. g., directions, turning a screw. Spelling, however, afforded most examples, such as double letters, *el* or *le*, *sion* or *tion*, etc. (b) *Words of similar sound* but different meaning: e. g., statue and statute, Calvary and cavalry. (c) *Mathematical formula* which must be used in computations as words are used in sentences. These involve a train of reasoning too long to go through each time they are used, and are, therefore, distinguished arbitrarily in memory: e. g., $\sin (x+y)$ and $\sin (x-y)$. (d) *Confusions in science*: e. g., do acids or alkalis turn litmus paper blue? Of two organ pipes of the same length, which gives the higher note, the open or the closed pipe? These involve a process of reasoning, but it is not so long as the mathematical formulæ and is more concrete, so the memory can be aided by associations with experiments and by mental images. (e) *Confusions in history*: e. g., did the Yorkists or the Lancastrians wear the red rose? Did Roger or Francis Bacon write the "Novum Organum?" With increasing knowledge of history these facts cease to be mere matters of memory and it becomes impossible to confuse them. Taking into account also the fact that the number of confusions decreases steadily from the most arbitrary to the least so, from spelling which has almost no necessary associations, to historical names which abound in them, the conclusion is obvious that the tendency to confusion varies inversely as the fullness and variety of the associations that are started by the ideas in question.

This list of things confused was further examined to find the nature of the confusions made by those who distinguish their hands in the three typical ways.

Of 33 who distinguish by association:

28 were liable to confusions of the sorts mentioned.

10 gave instances in classes (a) and (b).

18 gave instances in classes (c), (d) and (e).

Of 27 who distinguish by feeling:

13 were sometimes confused.

9 in spelling.

4 in other cases.

Of 37 who distinguish by instinct:

24 were liable to such confusions.

21 in spelling, class (a).

14 in classes not purely arbitrary, (c), (d) and (e).

The first and third of these groups seem to show that the instinctive method of deciding is less valuable in the later acquired forms of discrimination. This agrees with the general biological principle that instinctive action proves valuable in relatively simple conditions to which the organism is well adapted, but must be replaced by conscious action when the conditions become complex and adaptation less perfect. Most of those who have *no* confusions are among the class who distinguish their hands by a difference in feeling, who, perhaps, carry spelling and formulæ and even historical associations in motor terms.

(4) Question: Have you mastered any such tendencies, and how?

Of 68 who answered this question:

29 employ an arbitrary association.

15 employ reasoning, *i. e.*, they think of the meaning of two terms or the derivation.

6 use arbitrary memory—some call it sheer force of will, others strain of attention, with some it becomes positive muscular strain.

18 have not succeeded in mastering their confusions.

A comparison of the records of the first and second questionings for questions (3) and (4) shows that fifteen persons find new confusions and seven omit old ones; and that nineteen seem to have changed their method of mastery, eight find arbitrary memory unsuccessful, seven find confusion not overcome, four are scattering—rule instead of arbitrary memory and *vice versa*.

B. Question: How do you recall a forgotten name?

Replies on second questioning (97 cases): Eighty-eight (88) recall by some sort of association. Two (2) are conscious of a strain toward vacancy. One (1) calls up a mental image of the name [tries to visualize it?]. One (1) is unsuccessful by any method, and five (5) cannot tell how they recall a name. A further classification of those that work by an associative clue, gives the following results:

Of 88 making use of some sort of association:

51 seek by association with the person to whom the name belongs, the circumstances under which it was first heard and the like.

24 by the initial letter, or the place on the roll.

12 by the sound of the name.

1 by some peculiarity of the spelling.

It seems probable that the common habit in trying for a lost name is first to recall the image of the person whose name it is, or the circumstances in which the name has been heard, or some other complex image in which the name is a part. This may happen almost unconsciously. If this method fails, appeal is made to some one of the more conscious methods, in which attention is directed to some remembered part or mark of the name, in the hope that this will bring up the rest. The fifty-one above make use of the first method, going from whole to part; the remaining thirty-seven use the more artificial method and go from part to whole.¹ The initial letter is apt to be the part recalled both because of its prominence as a capital letter and because of its being the first in the series that make up the name.²

The chief difference between the records on this point, in the first and second questionings, is the decided gain in the group that makes use of the first method. In the first questioning the groups were nearly equal; in the second, a small class who had depended upon some peculiarity of the leading consonant or vowel, length, rhythm, color or a vague indefinable feeling of recognition, almost entirely disappeared into the class that recall by association. This change may very likely have been due to better observation on the part of the answerers, induced by the attention which the first questioning called to the matter. It may also have been due to a disinclination to specify particularly a second time.

II.

Methods of Concentrating Attention and of Getting to Sleep.

C. (1) Question: How do you concentrate your mind with all your might on some one thing, *e. g.*, in playing the parlor game of "mesmerism?"

The naïve answers on this point in the first questioning were of three types:

¹It has already been found in association experiments that transition from whole to part is more frequent and probably easier than transition in the contrary direction. (Cattell and Bryant, *Mind*, Vol. XIV, 1889, p. 241.)

²Experiments on memory span have showed the superior persistence of the first member of the series used: *cf.* Bolton, this JOURNAL, Vol. IV, p. 378 f.

(a) Those who were conscious of some physical strain which aided in concentration, *i. e.*, a feeling of exerting will-power in mentally repeating the command "Do so-and-so;" or a tension of the vocal organs in repeating over and over the names of the single object which the "mesmerized" person is desired to find; or a tension in the head, or an effort to keep the body quiet.

(b) Those who are more conscious of a mental image of the object or of the act to be performed. A few see the object in all its parts or relations, and one sees the word.

(c) A very small class are conscious of auditory sensations, *i. e.*, they hear the word, either with or without repeating it.

A few others can give no idea how they do concentrate.

The written answers of the second questioning confirm this result, but the first class is much larger than the others. Repetition seemed clearly a means of concentration. The third class almost entirely disappears, and the number of those who cannot tell increases.

(2) Question: How do you force yourself to work when you do not want to?

Replies on first questioning (100 cases): Sixty-one (61) think of the end to be attained, duty to self or to some other person, the necessity of getting done promptly, consequences of doing or leaving undone. Twenty-four (24) mentioned some physical device, *e. g.*, sitting up straight, reading aloud, reading over and then stopping to repeat, or some bodily comfort conducive to "cramming." Fifteen (15) could say only "I just go to work."

The sixty-one give the usual testimony as to voluntary attention; in forced work the present physical discomfort is hidden by the pictured future happiness or unhappiness. It also appears that, when duty and inclination conflict, the fear of evil consequences is a more powerful, or at least a more frequent, motive than the hope of good to be attained.

Replies on second questioning (97 cases): Forty-five (45) mention duty, necessity, fear of consequences, three thinking of the object of the work and one of the reward. Twenty (20) speak of physical tension. Eighteen (18) have some special method of going to work, and fourteen (14) cannot tell.

Among the methods mentioned are: Close attention to details, removal of all external distractions, reading aloud, timing one's self, not allowing one's eyes or hands to wander, feigning an intense interest, and imagining one's self another person who is not tired or who wants to work. One speaks of artificial stimulation with coffee.

(3) Question: How do you pay attention to a dull lecture?

Results of second questioning (96 cases): Sixty-four (64) have some conscious method. Ten (10) think of losing an opportunity, of politeness, etc., *i. e.*, make listening a matter of conscience. Five (5) feel physical strain. One (1) never finds a dull lecture, and sixteen (16) do not listen if they find one.

The answers to this question bring out another phase of attention. Not physical strain, nor moral consciousness is prominent, but the means of getting hold of the subject-matter that is presented. The number of those who do not listen includes ten members of the faculty who are, of course, beyond the stage of compulsory attention to dull lectures and sermons. The smallness of the number of students remaining is doubtless due to the fact that the question was explained to mean: How do you pay attention to something you are to be examined upon hereafter?

A classification of the methods employed by the sixty-four who have conscious methods gives the following table:

Of the 64 that have some conscious method:

- 16 pay close attention to the words, repeating them after the speaker if necessary.
- 14 feign an interest. This includes taking the attitude of interest.
- 12 make an outline. Some mentally, some on paper.
- 11 look steadily at the speaker.
- 6 assume a critical attitude.
- 4 try to get into rapport with the speaker.
- 1 imagines each sentence is the last.

Two things are strongly marked in this table. The first is the appeal from the sensory and receptive functions to the active and more directly controllable motor functions, from simple hearing to repeating of the speaker's words, the taking of an attitude of interest, the formation of an outline. The second is the turning to details, to the relatively concrete, either in the sensory form of the individual words or sentences of the speaker or his person (groups of 16, of 11, and of 1) or in the more intellectual forms of the outline and the critical attitude. The fact, also, that attention can be encouraged by feigning interest is worth regarding, not only for its implications as to the nature of emotions in general, but also as a contribution to mental tactics. All of the methods, however, probably involve more or less fully both the mental and the physical attitudes of attention.

A comparison of the answers to questions (1), (2) and (3), received at the first questioning with the later written answers, shows in (1) more consciousness of physical strain,

and in (3) more discovery of method. In (2) there is little difference, the changes balancing each other.

D. Methods of Getting to Sleep.

Question: How do you go to sleep when sleepless?

Replies on first questioning (100 cases): Seventy-seven (77) report some method, twenty-three (23) report none (twelve because they are never sleepless and eleven because when they are sleepless no method succeeds).

Of 77 who report methods, 9 mention a physical method;

68 are mental and are as follows:

- 19 try counting.
- 10 repeat poetry.
- 8 have various methods of thinking of nothing.
- 6 feign the state of sleep.
- 4 make up stories.
- 4 imagine that they are rocking on the ocean.
- 4 try to think of something pleasant.
- 3 imagine sheep going over a stile.
- 2 make pictures of peaceful or monotonous scenes.
- 2 try to think of something dry [uninteresting].
- 2 confine attention to some one thing.
- 1 listens in imagination to a brook she has often heard when going to sleep.
- 1 listens to the ticking of her watch.
- 1 repeats the Greek verbs.
- 1 counts her breaths.

Many had more than one method, and many mentioned both physical and mental methods.¹

In the first questioning there was also no report as to whether the answerers were troubled by sleeplessness or not. The printed questions asked for physical and mental methods and added: Are you often troubled? The replies (97 cases) showed twenty-two (22) who were often sleepless, nineteen (19) who were rarely so, fifty-six (56) who were never so. A further classification of the methods employed by these different groups gives the following table:

Of 22 who are often sleepless:

- 6 find a physical method more successful, *e. g.*, more fresh air in the room, eating, lying on the chest, perfect relaxation, etc.
- 4 must get up to read awhile.
- 3 count.
- 3 repeat something.
- 1 imagines humming sounds.

¹Compare the list which Wordsworth gives as ineffectual in his sonnet on "Sleeplessness."

"A flock of sheep that leisurely pass by
One after one; the sound of rain and bees
Murmuring; the fall of rivers, winds and seas,
Smooth fields, white sheets of water, and pure sky;
I've thought of all by turns, and still I lie
Sleepless!"

- 1 imagines an agreeable company.
- 4 are not successful by any method.

Of 19 who are rarely sleepless:

- 4 try counting.
- 2 have methods that suggest hypnotism, following an imaginary line up and down or watching a mental image of interweaving lines and concentric circles.
- 1 reads.
- 1 repeats something.
- 1 performs a simple arithmetical problem.
- 1 is unsuccessful.

The other 9 try stories, air castles, imagining the influence of ether, or merely anything pleasant, only one mentioning a real physical method; the others seeming to be merely memories of how it is to go to sleep.

Of 56 who are never sleepless:

- 9 think of nothing.
- 7 mention physical relaxation.
- 7 count.
- 7 think of something pleasant.
- 3 repeat something.
- 10 have no conscious method.

The other 13 have methods which are special instances of thinking of something pleasant, but involve regularity of movement, or simple mental imagery.

The writer's conjecture, formed after examining the first collection of methods, that the best of them is the feigning of sleep (chiefly a physical method) was confirmed by the relative frequency of physical methods among those that are really sleepless. Counting, repeating poetry or Greek verbs, rocking on the ocean, listening to a watch or to the imaginary sound of a brook, while they are rhythmical and would go with regular breathing, seem to imply a little more attentive activity for their execution. Those who make stories or pictures, or think of something dry, or concentrate attention on some one thing, seem to feign dreaming or to remember the semi-conscious reverie that precedes falling asleep. This also is confirmed by the classification, for such methods are tried by those rarely or never sleepless. Both these methods and the feigning of sleep proceed on the psychological principle that a given mental state will tend to reproduce itself entire when enough of its constituent parts are reproduced.

Of the psychical causes of sleeplessness the chief is inability to cease thinking about what has engaged the attention before going to bed; it is a "cramp of attention." The great art in getting to sleep on the other hand is the art of attending to nothing, the art of general distraction of attention. This makes the methods employed in getting to sleep an interesting counter picture of the methods of concentrating attention in study or on a dull lecture. In the first there is general

passivity, in the second activity. To attend, the muscles are contracted and an erect posture is assumed, explicit movements are made (reading aloud and repeating what has been gone over, making notes of what is said by the lecturer); to get to sleep, the muscles are relaxed, a recumbent position is taken, movements are avoided or suppressed (the counting or reciting poetry is generally silent or semi-articulate). In attending, the effort is to establish trains of thought connected with the matter in hand (making outlines, taking a critical attitude, looking steadily at the speaker, and trying to put oneself into rapport with him); in getting to sleep, the effort is to establish trains unconnected with the intrusive thought (reading, counting, reverie, repeating poetry or verbs). Over against the tendency to concentration on details in the first may be set in the second the tendency to rhythm (which gathers particulars into groups), though rhythm has a yet stronger reason for its presence in that it reduces the demands on attention to a minimum. In the first, future consequences are a spur, in the second everything but the present is excluded; and further instances would be easy to find. The contrast, however, is not absolutely perfect, and there are even points of identity, because, while attention is concentration as opposed to general distraction, it is itself distraction from everything except the thing attended to. In attending there is the exclusion, so far as possible, of sensations unconnected with the matter in hand, in getting to sleep the exclusion of all sensations; in attending the more or less complete suppression of non-assistant movements, in getting to sleep the suppression of all movements. Both are sought by imitation of the characteristic attitudes of mind and body.

A comparison of the results of the first and second questionings as wholes shows that in the two months that intervened physical methods had in six instances proved more successful than mental; in four instances building air castles, imagining something pleasant, etc., had proved better than no method; in four other instances reading, physical relaxation, etc., had proved better than counting; two persons had found no method successful.

III.

Emotions and Preferences.

E. (1) Question: What things were you afraid of as a child?

The answers received on the second questioning are classified in the following table. The number of things feared

exceeds the number of persons questioned, because most persons who confessed to any fear confessed to more than one.

Classification of things feared:

- 31 feared darkness.
- 31 feared animals. Dogs and cows were mentioned most often, geese and turkey gobblers several times.
- 24 feared a class of creatures which many of them said caused repulsion rather than fear, *e. g.*, snakes, spiders, worms, mice, cats, etc. Many in this class seemed abnormally fearful, and were terrified, also, by floating feathers, tearing cloth, and by all white, fuzzy things.
- 18 feared human beings—drunken, dead, insane, strange tramps, and rude boys.
- 9 feared imaginary evils, *e. g.*, witches, Satan, the end of the world, being buried alive, earthquakes, nightmare.
- 3 spoke of thunderstorms or of all strange noises.
- 2 were afraid of everything, one of whom was sure it was due to a prenatal influence.
- 6 were afraid of nothing.

Taking the list as a whole it is easy to trace in it two types of fear, the artificial fears induced by painful experience or the suggestions of elders, and the instinctive fear of the unknown. From the latter type, perhaps, ought to be separated yet a third, namely, fears that arise reflexly on sense impressions of special kinds, especially voluminous sensations. Examples of this would be the fears of snakes, spiders and insects and of fuzzy things, so far as these are tactual, the fears of tearing cloth, of thunder and other loud noises, and perhaps in some cases the fear of darkness—probably the most voluminous visual sensation in the experience of a child.¹ These are, however, at first all more or less strange experiences and later are seized upon by suggestion, so that in any individual case classification would be difficult, if not impossible. In the more conscious fears there is a common

The following extract from the autobiography of Laura Bridgman seems to show something like an instinctive touch-fear, though, of course, strangeness was also a factor. The experience belongs to a time before her eighth year, and so, before means of communicating with her had been secured. "My father used to enter his kitchen bringing some killed animals in, and deposited them on one of the sides of the room many times. As I perceived it, it made me shudder with terror because I did not know what the matter was. I hated to approach the dead. One morning I went to take a short walk with my mother. I went into a snug house for some time. They took me into a room where there was a coffin. I put my hand in the coffin and felt something so queer; it frightened me unpleasantly. I found something dead wrapped in a silk handkerchief so carefully. It must have been a body that had had vitality. I did not like to venture to examine the body for I was confounded. There stood some person on one side of the floor very calm, gazing upon the dead, and they touched its clouded eye and stroked it as if the tears were shedding along his face."

element of helplessness, in the dark one is robbed of his chief sense; ghosts, witches, Satan, represent an unknown and invisible power; earthquakes and the end of the world are catastrophes against which no power is availing.

The answers received on this head in the first questioning were unreliable, because the respondents did not at once recall the things they feared most, and those of the second questioning have alone been regarded in this discussion.

(2) Question: Were you ever frightened by these things?

This question might be expected to decide in individual cases whether the fear was acquired by personal experience on the one hand, or by instinct or suggestion on the other. The answers to this question were inconclusive, about as many fears of each kind appearing in the lists of those who had not been actually frightened by them as in the lists of those who had.

(3) Question: How have you overcome your fear?

Most of those who had not actually been frightened spoke of the fear as outgrown. A large class still feared snakes, insects, cows, horses, drunken and insane people. A few said they reasoned themselves out of it; one said pride overcame it, another, that better health caused it to disappear naturally. Much the greater number of fears caused by actual fright had not been overcome; repulsive things formed the largest class of these. Twice as many childish fears persist when caused by fright as when no cause can be remembered and they are believed to be instinctive.

F. Question: Mention a good ghost story, *i. e.*, something that gives you the creepy feeling supposed to characterize ghost stories.

Replies on second questioning (97 cases): Eighty (80) told stories which they thought creepy. Twelve (12) could tell none, were not sufficiently impressed to remember them. Five (5) told stories with a humorous turn at the end, which seemed to be the thing for which they were remembered, not for the mysterious part. The stories mentioned were all read to find the element which gives the creepy feeling. Many, of course, involve several elements of the fearful, but such a classification of elements as can be made gives the following table:

Of 80 stories which were thought creepy:

32 involved something unexplained, *i. e.*, one-third of all the replies were real ghost stories.

22 were stories of insanity.

16 were stories involving moral horror, as well as other sorts of fear, *e. g.*, Dr. Jekyll and Mr. Hyde.

19 were stories of murder, torture, snake stories, or stories about finding a corpse unexpectedly.

While none of the categories under *F* exactly match those of *E* and while *F* asked for stories of one kind of fear only, some general points of resemblance are interesting. The stories of murder, torture, snakes, and the like in *F*, are analogous to the sense fears in *E* and perhaps depend more immediately on sense imagery for their effect than the others. Stories of insanity and the fear of the insane are of common origin. The real ghost stories involve darkness and its fears together with those of the unknown and of mysterious power. An entirely new kind of fear appears in the moral horror group, a mark perhaps of the adult audience for which such stories are written. The feeling inspired by "Dr. Jekyll and Mr. Hyde," or by Mrs. Shelley's "Frankenstein," swallows up the mere physical sensations and makes them instruments of a moral repulsion. Hyde's external appearance and his crimes are repulsive to contemplate, but the story means little to him who sees only the bare incredible facts. Ibsen's "Ghosts" is repulsive as any idiocy is repulsive, but there is more than mere idiocy, there is awe before the forces of nature which make sin its own punishment. This is like the fear of darkness in that it is individual helplessness, but it is much more complex. It includes many kinds of sense-fears plus associations with moral ideals that do not exist for the young child. The ghost stories that affect one most are those in which there is a skillful accumulation and interweaving of all sense-fears. Among these ghost stories Poe's tales, Lytton's "The House and the Brain," and a story, originally from the German, called "The Gold Arm," were mentioned equally often. Stories of being watched by a pair of eyes peeping through a rent in a curtain or a crack in the floor received the next highest number of votes, and after them a story in *Harper's Magazine* for 1859, called "What was It?" An examination of the plots of these stories shows most interestingly how the artificial fear is worked up; fears of the sense types are common but generally subordinate, the fears of others are described and excite our own by sympathy or imitation, the whole scene of the story is gradually shifted from the ordinary world of daylight and known forces to a world in which man is the sport of mysterious and unknown powers. The appeal to the senses is never to all at once; a presence can sometimes be felt but not seen, sometimes seen or heard but not touched; sometimes it is only the effects of its acts which appear. The actual shudder of fear is generally the result of a special sensory appeal.

G. Question: Mention several concrete instances of things that have made you angry—ten if possible.

In order to secure a large number of answers no time of life was mentioned and thus the instances extend from childhood to the age of the average Junior in college. Those who wished to respond kept asking: "Do you mean vexed, or indignant, or simply *mad*?" The latter definition was adopted because it seemed least likely to be misunderstood. Of 100 persons asked only 34 gave lists of things that had made them "mad."¹ These lists included 247 instances of anger, but thirty-two instances were duplicated in the same list, *i. e.*, the same person unconsciously gave two or more instances when the cause was the same in both. The instances duplicated were as follows: Seven gave more than one instance of being angered by punishment; four by punishment because it was thought unjust; six by reflections (slurs) upon family, friends, country, etc.; five by arbitrary compulsion; two by pride in personal appearance; two by meddling; two by inanimate things; one by false accusation; one by carrying off of property. A classification of the 215 single instances remaining gives the following table:

Of 215 single instances of anger:

78 were due to causes which the writer cannot better name than injuries to sense of personal dignity; seventeen, remarks more or less insulting to members of one's family, dear friends, sex, church, or political party; sixteen, punishment, including scolding, punishment that was administered in the presence of others, etc.; seven, interference in one's personal affairs, *e. g.*, religious beliefs or friendships; six, being laughed at; four, being slighted; four, making a bad appearance in recitation; three, not being told what one thought she ought to know; two, being patronized; two, being gossiped about; two, being made a tool of; two, having manners corrected; two, comments on personal appearance; the rest of the seventy-eight are isolated instances, such as being told, "I told you so," having work taken out, etc.

73 are caused by sense of injustice;

41, injustice to self: twelve, unjust punishments; eight, when someone shirks her duty; seven, being contradicted—including positive accusation of falsehood; five, a broken promise; two, having to spend more time on lessons than seems just; one, being willfully misrepresented; one, being interrupted; one, not being allowed to explain; one,

¹ The smallness of this proportion is doubtless due in part to the difficulty of recalling things that have caused anger (normal people put such unpleasant things out of mind and in time forget them) and in part to disinclination to confess those that are recalled. The question is emphatically condemned as a question by this small number of answers and except for the goodly number of instances cited by the third that did respond would have hardly been worth discussing.

- being cheated out of something; one, being left at home; one, not being sympathized with; one, when matters serious to her were made light of.
- 17, injustice where self is not involved: eight, seeing someone else punished unjustly; four, hearing some one unjustly criticized; three, seeing animals ill-treated; one, seeing good nature imposed upon; one, seeing one person tell on another.
- 15, injury to property: eight, interference or injury to property, *e. g.*, having a notebook carried away; five, when a journal or letter was read; two, when a pet was killed.
- 33 were arbitrary compulsion, *i. e.*, to do one thing when she preferred to do another, or to refrain from something, or simply to "do as you are bid," *e. g.*, to do some kind of domestic work, to practice music, to apologize, not being allowed freedom to do work in one's own way.
- 25, physical annoyances: ten, being teased; three, "the mere sight of some people;" two, hurting one's self; two, familiarity from some people; two, having one's hair combed. Other examples are: pianos out of tune, being pushed in a crowd, being kept awake at night, etc.
- 6, disappointment, *e. g.*, not being able to learn something, or to find something when it is wanted, or missing a train.

Such a table as this, based on answers from only about one-third of those asked, can furnish only the most general indications, but there are traces of a few relations that are, perhaps not entirely accidental. The preponderance of mental and moral causes is clear and to be expected. The extent to which offences against personal dignity appear (and to the seventy-eight specified in the table should surely be added a large portion of the cases of injustice to self and of arbitrary compulsion) is doubtless characteristic of the angry emotions of people generally, and is not without pedagogical and ethical import. The small number of cases in which the anger was altruistic, testifies to a healthy egoism; we may be indignant at injuries to others, but our feeling rarely rises to anger unless others stand near enough to us to be covered by the *égoïsme à deux* by which a certain Frenchman has described love.

H. (1) Question: What is your favorite color, *i. e.*, what color appeals to you most, apart from any colored thing—merely as color sensation?

Replies on first questioning (100 cases): Thirty-eight (38) prefer some kind of blue; eighteen (18) some kind of red; twelve (12) yellow; eight (8) green; five (5) violet; one (1) white; one (1) dark brown; one (1) all dark warm colors; one (1) mere brightness of evening sky; and fifteen (15) have no preference.

Replies on second questioning (97 cases): Thirty-seven (37) preferred blue; twenty-two (22) red; ten (10) yellow;

nine (9) green; five (5) violet (heliotrope, lavender, purple); two (2) brown; two (2) gray; one (1) white; one (1) brightness of the evening sky; one (1) all dark warm colors; and seven (7) had no preference.

Blue is clearly the most generally preferred color and red stands next; after it follow yellow, green and violet. The most noticeable difference between the results of the two questionings is the decrease of those who have no preference and the gain of those that prefer red.

This order is confirmed by the report from a psychology class, each member of which was asked to write her favorite color on a slip of paper before leaving the room. This class of twenty-one reported as follows: Ten blue, four red, two none, one yellow, one gray, one violet, one green, one white.

Another class of forty-six was asked suddenly to put down the first color that came into their minds. Of forty-six persons: Nineteen wrote red, fifteen wrote blue, five wrote yellow, four wrote white, one wrote green, one saw the whole spectrum beginning with red, one saw a band of four colors beginning with red.

The explanation of color preferences was sought by asking the second question.

(2) Question: Why do you like this color?

In the first questioning:

Of 38 (blue): 30 give a reason.

Of 18 (red): 16 give a reason.

Of 12 (yellow): 10 give a reason.

Of the smaller classes all can tell why, and the reasons are most elaborate for the most unusual colors.

In the second questioning:

Of 37 (blue): 29 can give some reason.

Of 22 (red): 19 can give some reason.

Of 10 (yellow): 8 can give some reason.

Of 9 (green): 8 can give some reason.

All after green as before.

From this it appears that in both cases red has more conscious meaning than blue or yellow. The reasons given for liking blue are, delicacy, purity, tenderness, spirituality, infinity, calmness, faithfulness, immortality. Red is chosen because it is warm, deep, cheerful, loving, intense, passionate or quivering with pulsating life. Reasons vary in both cases with the shade chosen. Yellow is chosen for warmth, softness, happiness.

(3) Question: Has this color any associations with persons, places, music, poetry, emotion, odor, taste?

In the first questioning:

Of 38 (blue): 26 have some association.

Of 18 (red): 10 have some association.

Of 12 (yellow): 9 have some association.

All after yellow have some association.

In the second questioning :

Of 37 (blue): 26 have some association.

Of 22 (red): 13 have some association.

Of 10 (yellow): 6 have some association.

Of 9 (green): 7 have some association.

Blue is most associated with the sky, then with memories of childhood, with the sea, with music, and all the gentler feelings. Red is associated with strong feelings, strong characters, autumn, martial music. Both are frequently associated with persons. Yellow is associated with flowers and sunshine.

The conscious reasons are relatively more numerous in the case of red than in that of blue, but the explicite associations are less so. This would seem to mean, if the figures will bear any interpretation, that red is a color of somewhat more direct emotional meaning, and blue of somewhat more indirect meaning. The "direct meaning" of any color is probably dependent on early and forgotten association, and the indirect on later and better remembered association. If this is the case the direct emotional meaning of red would probably be a trace of the supposed preference of very little children for that color. How much the answers are influenced by such expressions as "true blue" would be hard to say, but there is not much trace of it in the "reasons" and "associations" specified. Early habits of dress may count for a good deal (more children seem to be dressed in blue than in red), but this still leads as a problem why mothers choose blue.

I. Question : Did you express yourself in any art form before eighteen years of age?

Replies on second questioning (97 cases) : Sixty-six (66) Yes ; thirty-one (31) No. Two (2) used some form or forms, but do not say which. A classification of the forms used by the sixty-four remaining gives the following table :

Of 64 who made use of some art form:

14 used verse (alone).

10 used stories (alone).

6 used drawing or painting (alone).

4 used music (alone).

14 used stories and poetry.

3 used stories and drawing or painting.

3 used poetry and drawing or painting.

2 used poetry and music.

1 used painting and music.

6, all arts but music.

1, all arts but painting.

Those who replied No seemed to take pride in the fact that they had been guilty of no such youthful folly. Most called their poetry rhymes. No attempt was made to get possession of the productions, but information was volunteered in some cases. Most interesting was one who wrote a tragedy at ten, which was acted on a little stage for the benefit of her friends; from ten to thirteen, an epic; at thirteen, sentimental and religious poems. A few mentioned telling stories as a favorite pastime, but said they did not write them down. A few mentioned a full journal as one means of expressing feelings from about twelve to fifteen. A few more who were asked in general conversation, said their poetry was religious and their stories sentimental—often involving their idealized selves as heroines.

IV.

Recollections of Childhood.

J. Question: What were your favorite games when a child?

The list of games falls easily into active, imitative and competitive groups. Classifying in that way gives the following table:

Of 92 who replied:

- 49 gave active games (including those of competitive activity):
 - 31, running and being pursued; specifically: sixteen, hide and seek; six, all running games (none named); four, tag; chase, London bridge, pussy wants a corner, blind-man's buff, fox and geese, hop-scotch, mulberry bush, wild Indians, etc.
 - 13, mere activity (?): horse, all boys' games, all out-door games, climbing trees, riding horseback, swinging, etc.
 - 5, games involving more thought: four, ball; one, hunt the thimble.
- 36, imitative games: thirteen, dolls; twelve, dramatic plays (invented from stories); four, keeping house; four, school; two, building bridges; one, church.
- 7, competitive games (involving thought): three, checkers; two, dominoes; two, puzzles.

Evidently far the larger number preferred out-door games, and among these most prefer the excitement of chasing and being chased. Hide and seek is the most popular game of all. Dolls come next in order—imitation of family life next after desire for pure activity. After these two games the remaining active and imitative games are almost exactly equal in number. In all cases where a number of games were mentioned the one given first was taken as that preferred.

K. (1) Question : What is the earliest thing you are sure you can remember ?

The replies on the second questioning (97 cases) may be classified as follows :

Of 97 who answered:

- 17 remember a birth or death in the family;
- 14 remember being frightened or hurt, *e. g.*, falling down stairs, being stung by a bee, being bitten by a dog, being punished, getting fingers pinched, feeling pain in eyes from flashes of light through the windows of a railroad coach.
- 12 remember an illness of self or family.
- 10 remember an emotion; of these, three remember a feeling of grief or disappointment at the death of a pet animal, when parents started to the Centennial, when a necklace was stolen; one remembers a feeling of anger, and two remember the feeling of pleasure on receiving a present.
- 9 some novel experience which seems to contain an assertion of individuality or at least a recognition of self as acting independently, *e. g.*, going to Sunday school alone for the first time, walking across the floor the first time, the first sentence, running away, or acting upon a new idea strictly her own, such as dressing up a dog or trying to make a chicken swim.
- 7 something connected with grandparents.
- 2 remember a visit. These might be regarded either as belonging with the development of individuality or with the memory of new scenes.
- 1 remembers her father's singing.
- 25 remember particular scenes which do not fall into any of the arbitrary classes above. Of these:
 - 12 have central figures, *e. g.*, a Christmas tree, a fire, a horse and carriage, Barnum's giant, a colored man, a sword, a kitten, a chicken, a new bridge.
 - 5 have no central figures but consist of a number of sense impressions, *e. g.*, scenes at the Centennial, playing in the garden, the family moving, a wedding.
 - 5 while remembering scenes do not specify one above the rest.
 - 3 cannot select any memory as the earliest.

Taking the table as a whole several tendencies appear. The preponderant direction of the mind of the child is shown by the fact that seventy show attention to the outside world and only twenty-seven to self. Even when the child thinks of himself he is more apt to regard himself as a victim of sensations than as an agent in bringing things to pass; in the twenty-seven cases only eight are of self assertion. Two cases of self-discovery are interesting enough for specific mention. One respondent remembers sitting on her mother's lap and hearing her mother explain to someone that she must postpone a journey because her little daughter was ill. This gave her a queer feeling of self recognition. The other says I remember "standing in the middle of the floor in the back bed-room and thinking that I was one of the people like those

around me and not just looking on at the world.' The ages in these two cases were respectively three years and between two and three years.

A superficial examination of the table seems to show that the child's world is a world of sensations rather than feeling (only five cases of emotion are specified) and chiefly of sensations of sight, but this is undoubtedly an error, for an emotion of some sort is evidently what made the experiences originally impressive. If in this later conscious recall sense elements predominate, it is because emotions are themselves not clearly attended to at the time they are experienced and not well recalled afterward. A child that is in terror of a dog is attending much more to the dog than to his own fears. The difficulty of recalling emotions has been fully recognized by psychologists. The preponderance of visual recollections may be due to a similar cause; sight is for most people the leading sense and, other things being equal, the focus of attention is turned upon what is seen. An interesting illustration of this general relation is found in the memories of illness where the things specified are the darkened room, the taste of the medicine, the candle lighted in the night, the mother's face bending over the sick child or the father's voice as he carried it, while the pain itself is not mentioned. The memory of pain, like that of emotion, is extremely colorless and imperfect.

(2) How old were you?

To this question eighty-nine replies were received, and the average was found to be 3.04+ years.

The different groups average as follows:

Memories of illness,	- - -	2.9+ years.
" " grandparents,	- -	2.6+ "
" " birth or death,	- -	3.3+ "
" " being hurt or frightened,	- -	2.8+ "
" " special scenes,	- -	3.1+ "
" " emotions,	- -	3.1+ "
" " self-recognition,	- -	3.2+ "

V.

Miscellaneous Questions.

As already indicated, the questions placed in this group proved less satisfactory than the others, and their results are chiefly valuable as warnings.

L. Mention some story that has made you weep—the most pathetic you can think of.

Replies on second questioning (90 cases): Thirty-nine (39) mention the death of some character in a novel; four

(4) misunderstandings; three (3) hopeless self-sacrifice; three (3) suffering for sin committed in ignorance; two (2) disappointment of dearest hope in life; seventeen (17) miscellaneous; twenty (20) can select none; two (2) never weep over books. The most interesting result of this table is the considerable number who specify cases not immediately connected with death.

M. Mention a funny story, incident, joke or scene in a book or play—the funniest you know, if possible

In response to this question many persons said they could not attempt to select the funniest story, because this was too dependent upon varying moods; if they should select one, it would be merely the last funny story they heard. Many who did attempt to choose showed plainly that they mentioned the one freshest in memory, and no classification of them is possible.

N. What characters in history, fiction or life were ideals to you when growing up?

The answers to this question, so far as ideals are taken from history and fiction, is little more than an indication of what children read when most impressible. There is a decided and natural preference for ideals of the same sex, but a few respondents were careful to state that their ideals were of the other sex. Where the ideal is found in real life, the tendency is to idealise the less-known rather than the well-known, the teacher rather than the parents, and the old rather than the young.

O. If you had just one sermon to preach what would be your text?

Many subjects that were given in reply to this question could not be classified because so many sermons might be preached from them. Some hesitated to answer the question because they thought it might be asked to get an insight into individual character, and give the questioner a clue for later use. The first questioning was made during the Christmas holidays, when the financial stress was causing great suffering among the poor, and social questions were much discussed. This seems to have given color to the answers—at least in the second questioning answers of this kind were less numerous. Some of those who could not choose excused themselves by saying that their subjects would vary with their own meditations and could not be relied on as characteristic, and this is doubtless true of many who chose.

It is not difficult to find reasons for the failure of these questions. In *L* it was clearly a mistake to ask for the *most* pathetic story—thus introducing an unnecessary effort of comparison—and to specify the shedding of tears. If the request

had been given the simple form of *F* it might have been a tolerably good question. The same is true of *M*. Both *N* and *O* are ill-conceived in the matters for which they ask. If full answers had been obtained, only uncertain inferences could be drawn from them, and *O* is worst of all, in asking for something that a part at least of the respondents were disinclined to give.

So much for the results of this set of questions. On the questionnaire as a psychological method a point or two may yet be added. In the first place what tests are there, if any, for the exactness of the answers received? M. Binet, in his *Psychologie expérimentale*¹ after referring to the accord of the observations as a general control, mentions as tests applicable in special cases, simple experiments like requiring a respondent who has reported strength of memory for timbre to give the orchestration of a portion of a well-known piece of orchestral music, or one who has reported colored hearing to give at sufficiently separate times his photisms for a list of words. If the orchestration is well given, or if the double records of the list agree, the statements are probably true. The test of double questioning can be applied to any set of questions, in which it is possible, by lapse of time, or in any other way, to make the second questioning independent of the first. The amount of concordance in the two replies is a direct measure of the trustworthiness of the answer as a representation of what the respondent thinks on the matter in question; it does not of course show whether he is mistaken or not. A certain light is thrown on this last point by agreement or disagreement among the respondents, and (provided that answers may be fairly expected from all, as in the case of the questions above), by the number of those who find themselves unable to answer the question. If as many as fifteen in a hundred cannot tell how they force themselves to work when disinclined [*C* (2)], some uncertainty may be assumed in the answers of a good many who did answer. And if, as in *G*, two-thirds fail to answer, from unwillingness to reveal the causes of their anger, or for any other reason, it is highly probable that some at least of the other third were influenced in the extent of their confessions. When a questionnaire is sent out broadcast to the general public, this criterion cannot be used, though the total number of answers received to the simplest question of the set might serve in place of the total number of papers sent out.

The double questioning carried out in the case of the

¹P. 141 f., Paris, 1894.

questionnaire above, shows some wavering in the answers, even though the interval of two months was probably too short for complete forgetfulness; in most cases, however, the principal ratios of answers were not materially changed, and it is believed that they are substantially true. The inability of some respondents to tell how they recall a forgotten name, or how they set themselves to work when disinclined, shows that these questions approach the limit of casual introspection. The questions in Group IV met with the most ready and satisfactory answers. The respondents seemed always sure that they were making truthful reports, the recollections were interesting to them, and they were glad to make any contributions to a better understanding of the child.

In general, the questionnaire seems to the writer more valuable for the suggestions it gives the questioner than for its strictly scientific results. Each group of this study has suggested some problem for further investigation by experimental methods.

IX. THE MEMORY AFTER-IMAGE AND ATTENTION.

By ARTHUR H. DANIELS, PH. D.,

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The memory after-image is familiar to all who busy themselves with psychology; and many others have casually noticed it in their ability to count clock strokes from the beginning, after a number have already passed, or to pick up the whole of a sentence whose beginning has been neglected. The experiments of this paper were undertaken to determine, if possible, the duration of images of this kind.

A prime difficulty is to separate the simple persistence of the image (due, perhaps, to the native retentiveness of the nervous substance) from its continuation in associative memory. It is possible to avoid the latter by experimenting with completely distracted attention; for what is received with complete inattention forms few associative bonds, or none at all, and runs its course of gradual extinction without interference. The state of the image during the fading out can be discovered by requiring the subject at a given signal to turn his full attention upon it and to endeavor to reproduce it. The degree of his success will indicate the condition of the image. It is probably impossible, under ordinary circumstances, to secure perfect distraction, and, even if it were secured, it might not pre-

vent the formation of all associative bonds. Absolutely perfect distraction was not secured in the experiments about to be described; they were made, however, under as near an approximation to that condition as the experimenters were able to secure.

As a means of distraction, the loud reading of letters pasted on a revolving drum and seen, one at a time, through a hole in a screen, was first tried, but proved itself less effective than the reading of interesting stories in a loud voice and with the greatest possible rapidity. The image used was that aroused by the pronunciation of a group of three digits. When the subject was well underway in his reading, the digits were announced by the operator, the three occupying about two seconds. After a determined interval, at a signal from the operator (a tap on the table) the subject ceased reading and endeavored to recall the digits that had been given. The intervals were 0, 5, 10, 15 and 20 seconds. One hundred trials each were taken at these intervals, on each of two subjects, *S* and *D*, each serving alternately as subject and operator. At beginning, both subjects were somewhat experienced in this sort of experimenting and the trials, on different intervals, were so mixed as to exclude as far as possible the effects of both practice and fatigue. In spite of strenuous effort to concentrate on the reading it was not always possible to prevent a return of the digits to consciousness before the operator's signal and this difficulty increased with the increase of the interval. A record of the cases in

TABLE I.
Involuntary Return of Digits.

SUBJECT S.

	5 Secs.	10 Secs.	15 Secs.	20 Secs.
No return,	47	29	22	19
One return,	50	67	66	54
More than one return,	3	4	12	27

SUBJECT D.

	5 Secs.	10 Secs.	15 Secs.	20 Secs.
No return,	40	36	15	15
One return,	56	56	60	75
More than one return,	4	8	25	10

which such a return occurred was kept and is summarized in the table above. Since 100 trials were taken for each subject,

with each interval, the figures represent percentages also. At the zero interval the reply of the subject followed immediately on the announcement of the last digit, and no time was given for the involuntary return here considered.

The strong tendency which this table shows for the image to thrust itself into consciousness, was probably due in part to the knowledge that its recall would presently be required; it testifies, in other words, to a partial division of attention. It is probably due in part, also, to the general tendency already often observed in both mental images and attention to a periodic rise and fall. When the original image of the digits was clear, the tendency to return developed almost at

TABLE II.
Decrease of Correct Replies with Increase of Interval.

	0 SECS. ¹			5 SECS.			10 SECS.			15 SECS.			20 SECS.		
	Total Number of Cases.	Number Correct.	Per Cent. Correct.	Number of Cases.	Number Correct.	Per Cent. Correct.	Number of Cases.	Number Correct.	Per Cent. Correct.	Number of Cases.	Number Correct.	Per Cent. Correct.	Number of Cases.	Number Correct.	Per Cent. Correct.
No return,	100	56	56	47	5	9	29	0	0	22	0	0	19	0	0
One return,				50	7	14	67	1	1	66	1	1	54	1	1

SUBJECT D.

No return,	100	96	96	40	24	60	36	10	28	15	0	0	15	0	0
One return,				56	40	71	56	17	30	60	7	12	75	7	9

¹With this record at the zero interval may be compared the following percentages of correct replies in the preliminary experiments, gotten by observers *D* and *A*, with no distraction (ordinary memory span) and with the distraction of reading letters from the revolving drum, in which case there was no interval between the pronunciation of the digits and the subject's response. The figures in parenthesis give the number of trials on which the percentages are based.

Subject.	NO DISTRACTION.			READING FROM DRUM.		
	8 Digits.	9 Digits.	10 Digits.	4 Digits.	5 Digits.	6 Digits.
A	100 (4)	78 (23)	31 (26)		53 (31)	7 (16)
D	80 (20)	63 (42)	8 (9)	74 (49)	27 (28)	26 (12)

once; if this was successfully resisted, the chances of complete exclusion were much increased. In what terms the return occurs—auditory or motor, or both—would be interesting to know, but cannot be stated with certainty; subject *D* is inclined to believe it motor in his case. Frequently, when there was no actual return of the digits, the subject was unmistakably conscious of their presence and their struggle for recognition, and by redoubled efforts at reading was able to prevent their re-entrance. This tendency to immediate and repeated return is undoubtedly of the greatest importance in ordinary memory. It is in this way that the original memory after-images are caught into the conscious memory trains and made recallable.¹ If no such return takes place a few seconds suffice to obliterate the image. A single return prolongs its existence, but for a few seconds only, as is clearly shown by Table II.

The table shows that under the conditions of these experiments the memory after-image does not last fifteen seconds in a reproducible condition, unless it is freshened by a re-entrance into consciousness. If it returns a single time it may outlast twenty seconds, but not often.

The table also shows a considerably greater persistence of the image in the case of *D* than in that of *S*, and the ordinary memory-span tests with *D* (see note to Table II) indicate a high retentiveness, but the chief cause was probably a less complete abandonment to the reading on his part. This was the impression formed during the experiments and is corroborated by the large number of *S*'s incorrect replies with the zero interval, where lack of retentiveness would play a small part. This can only mean that a certain minimum of attention is necessary for the original registry of the image, failing which its persistence is considerably abbreviated. During the experiments the subjects noticed differences in the vividness of the original registry, and found an extra effort at concentration necessary during the pronunciation of the digits, an effort which the operator also could often observe in the increased loudness of the voice of the reader. The announcement of the digits diverted a certain portion of attention to them, which, when the experiment was successful, was small and instantly withdrawn. The effect of this partial attention

¹The much greater persistence of what has been consciously in mind was also unexpectedly testified to by the errors made. Numbers accidentally met in the reading were apt, unintentionally, to take the place of forgotten digits in the test groups. This was also the case with numbers once used by the subject in replying. In one series of twenty groups, for example, the digit four was actually given five times, but occurred in the replies seventeen times.

is perhaps in the nature of a repetition, at least both subjects found it difficult to distinguish between repetition and good registry. The effect was most marked when the announcement of the digits occurred during a necessary pause in the reading. From all this it is therefore clear that the durations shown in Table II are in excess of what might be expected with perfect distraction, were that attainable. They represent a limit below which the duration of the simple memory after-image falls. With perfect distraction the subject ought to be unable, after a very brief interval, to say whether or not any digits at all had been announced.

A group of digits has, of course, a shorter duration than the single digits that compose it and is harder to reproduce. This is evident on a comparison of Tables II and III, in the latter of which is given the percentage of right answers for the digits in the first, second and third places. All replies (except those which had been preceded by more than one involuntary return of the digits) were used in making this table.

TABLE III.

Percentage of Right Answers for Different Parts of the Group.

SUBJECT S.

	NO RETURN.			ONE RETURN.		
	1st Digit.	2d Digit.	3d Digit.	1st Digit.	2d Digit.	3d Digit.
5 Seconds,	14	29	87	20	24	80
10 Seconds,	24	13	27	5	14	73
15 Seconds,	9	13	31	7	10	60
20 Seconds,	5	10	15	14	9	40

SUBJECT D.

5 Seconds,	70	70	95	78	78	95
10 Seconds,	30	41	83	62	39	76
15 Seconds,	20	6	46	31	21	75
20 Seconds,	10	10	5	29	17	66

The images of the separate digits evidently follow the same general course as the groups, but more slowly. Unless they get a recall, their chances of surviving fifteen seconds are, however, not very great. As regards the relative persistence of different parts of the groups, the table shows that the last

digit is more persistent and that there is little difference between the other two—a slightly greater persistence for the second in case of *S*, and for the first in case of *D*.

In conclusion, a few words may be added to show the relation of these experiments to those previously made by others. The term "memory after-image" (*Erinnerungsnachbild*) was introduced by Fechner,¹ who records observations on visual memory after-images. Exner² has described the same, under the name of the "primary memory-image," and gives examples for both sight and hearing. He remarks that the image vanishes in the course of a few seconds, if not caught by attention. Many experiments have been made on the simple memory span, with digits, letters or nonsense syllables. They have shown that, under favorable circumstances, groups of eight or ten members could be correctly reproduced. If the members of the groups were separated by one second intervals, this would show that in eight or ten seconds' time some of the members fall out of memory, and in even less time than that, for the errors are more frequent with the middle members of the group than with the first. The most elaborate experiments by this method were those made in Wundt's laboratory by Dietze.³ In these experiments groups of metronome ticks were compared with each other, on the supposition that accurate comparison is possible only when each group can enter consciousness as a whole. The size of the largest groups that can be accurately compared would then give the size of the greatest groups that can be taken into consciousness entire, *i. e.*, the groups in which the first member given is just fading out of memory when the last member enters. The supposition on which these experiments were made has been criticized, but apart from that they do not give sure determinations of the duration of the memory after-image, because of the very great effect of the rate at which the ticks are given and the tendency to rhythmical grouping of them. The experiments on memory-span have generally been made with full attention; Münsterberg,⁴ however, worked with attention distracted by solving arithmetical problems aloud, but used his results for other purposes than the measurement of the duration of the memory after-image. The experiments of Wolfe⁵ on the memory of

¹Elemente der Psychophysik, II, 491 ff.

²Hermann's Handbuch der Physiologie, II, ii, 282, quoted by James, Psychology, I, 646.

³Physiologische Psychologie, 4te Aufl., II, 288 ff.; for Dietze's full statement see *Phil. Stud.*, II, 362 ff.

⁴*Zeitschrift für Psychologie*, I, 1890, 99-107.

⁵Summarized by Wundt, *Phys. Psy.*, 4te Aufl., II, 431 ff.; for Wolfe's full statement see *Phil. Stud.*, III, 534 ff.

musical tones were aimed directly at the determination of this duration. In these experiments the ability to *recognize* a given tone was used as the test of the integrity of the memory-image instead of the ability to *reproduce* it which was used in the experiments of this paper. It is not surprising, therefore, that the duration found by Wolfe should be larger, extending to as much as sixty seconds. The fact that Wolfe's experiments were made with concentrated attention, and those of this paper with distracted attention, is also important, though single tones would not form many associative bonds, except perhaps with very musical subjects. The percentages of right answers are not directly comparable in the two studies, because of the greater opportunity for error with the groups of digits, but there is nothing overtly contradictory in them. The tendency of the digits to re-enter consciousness, observed by *S* and *D*, is undoubtedly the same that gives the periodic improvement of memory in Wolfe's curves.

X. ON THE LEAST OBSERVABLE INTERVAL BETWEEN
STIMULI ADDRESSED TO DISPARATE SENSES AND TO
DIFFERENT ORGANS OF THE SAME SENSE.

BY ALICE J. HAMLIN.¹

The figures commonly given by the text-books for this interval are taken from work done by Exner nearly twenty years ago and, so far as the writer is aware, since repeated only in part.² The object of the following experiments

¹Student at the Summer School, 1894.

²Exner, *Pflüger's Archiv.*, XI, 1875, 403-432. The statements in the text-books leave it open to the reader to infer that the figures are for single pairs of stimuli, (a single visual stimulus, for example, followed by a single auditory stimulus)—such at least was the conception under which these experiments were undertaken. When, however, Exner's original paper was examined, it was found that his method, except in the case of separate stimuli to the two ears, was such as to give him a series of pairs of stimuli instead of a single pair (pp. 406, 419-20, 423, 426). This may be represented diagrammatically as follows, letting (*a*) stand for auditory and (*v*) for visual:

(Auditory first).	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>	—	<i>av.</i>
(Visual first).	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>	—	<i>va.</i>

In the writer's experiments, on the contrary, single pairs (or triplets) of stimuli were used without exception, *e. g.*, either *av* or *va*. The importance of this difference will appear in the discussion of results below. The work of von Tschisch (*Phil. Stud.*, II, 603), of Angell and Pierce (this JOURNAL, IV, 528), and of Jastrow and

was a remeasurement of this interval for single pairs of stimuli under varying conditions of attention. An attempt has been made at the same time to notice accurately the subjective conditions, in the hope of finding by means of this introspection what the psychological basis may be for such discriminations. In most of the experiments only two stimuli were used, giving the following combinations: Eye and ear, eye and hand (electrical stimulation), ear and hand, right ear and left ear, right hand and left hand. The combination of right eye and left eye, upon which Dvůrák has made experiments, was undertaken by a fellow student and was consequently omitted. In a few experiments, stimuli to eye, ear and hand were used at once in a way which will be more fully explained below.

Apparatus and Method. The eye stimulus was always the flash of a small Geissler tube, the ear stimulus was generally the click of a telephone in the secondary circuit of a sliding induction coil (sometimes, however, the snap of an induction spark), and the hand stimulus a moderate induction shock in the tips of the middle and fore fingers. An almost unavoidable difficulty in using stimuli produced by induction apparatus is the variation in intensity. Reason will be given below for believing that moderate variations are without marked effect in such experiments as these. Nevertheless the variations were made as small as possible, and when the subject believed that he was influenced by them, the trial in question was discarded. The apparatus by which these stimuli were managed is fully described in the next of these "Minor Studies." It consisted essentially of a pendulum contact-breaker by which three electrical circuits could be broken at known and exactly adjustable intervals of time. These circuits in the further description will be called *a*, *b* and *c*. The range of possible intervals was from 0 to 44σ ; those used varied from 18σ to 44σ . The exactness with which the intervals were kept by the instrument as shown by

Moorehouse, (this JOURNAL, V, 239), is unlike that of the writer for the same reason as Exner's. In the *Revue Scientifique* (XXXIX, 585) Bloch gives the results of experiments on this matter, but is so meager in the description of his methods that it is impossible to decide whether his results are comparable with those of the writer or not. Gonnessiat (*Recherches sur l'équation personnelle*, Paris, 1892, pp. 138-140) has measured the interval by which a visual stimulus may precede an auditory, and yet both seem simultaneous. And finally, Dr. F. Tracy has in this laboratory measured the just observable interval between a sight and a sound by a method practically identical with that about to be described. His results have been kindly placed at the disposal of the writer, and for them her acknowledgments are due. The results have been inserted below in their proper connections.

chronographic tracings was amply sufficient, the mean variation in no case being as great as one part in 100. In the experiments with two stimuli, it was customary to place the apparatus for one (*e. g.*, the flash) in circuit *b*, and that for the other (*e. g.*, the click) by parallel wiring in both *a* and *c*. It was easy then, by a simple switch, to throw the latter apparatus from *a* to *c*. If it were in *a*, the order of stimuli was *a b* (*i. e.*, click leading, flash following); if in *c*, the order was *b c* (*i. e.*, flash leading, click following). The break-key of circuit *b* was fixed permanently in a middle position. In adjusting the apparatus the keys for *a* and *c* were set to make equal intervals on either side of *b*. Except in a few preliminary experiments, the subject and operator were in separate rooms.

When the operator was about to give the stimuli, he sent a ready-signal by a telegraphic key and sounder, and then broke the circuits for the stimuli by dropping the pendulum. As soon as the subject had received the stimuli, he returned his judgment of their order in the same way. After a series of twenty judgments there was usually an interval of rest, or the operator and subject changed places. One hundred judgments (five groups of twenty each) were taken in nearly every case before the conditions were varied. The Method of Right and Wrong Cases was used, the subject being given the stimuli an equal number of times in each order; with the click and flash, for example, click-flash fifty times was mixed irregularly with flash-click fifty times. In cases of doubt the subject was required to guess. Of those who served as subjects, *S* was practiced both in general psychological experimentation, and in this particular kind of work; *D* had had general practice, but *Si* and *H* had had neither.

I.

Experiments with Unforced Attention.

The first thing to be undertaken was a study of the matter under normal conditions of attention. The general results of experiments on this point can be most briefly reviewed in connection with the following tabulated record :

TABLE I.

Group I. *Stimuli addressed to disparate senses ; attention at a balance.*¹

STIMULI.	Interval.	Subject.	Date.	Number of Tests.	Percentage of correct replies.	
					Flash first.	Click first.
Flash and Click.	*18σ	H	A 6	100	70	40
	29	S	A 9	"	73	67
	"	"	"	"	74	72
	* " "	H	"	"	66	42
	44	"	A 10	"	75	52
	"	"	"	"	84	62
					Flash first.	Shock first.
Flash and Shock.	44σ	S	A 11	100	73	86
	"	H	A 11-12	"	62	62
					Click first.	Shock first.
Click and Shock.	*18σ	S	A 7-8	100	40	74
	* " "	H	A 1-2	120	48	78
	30	Si	J 22-25	70	72	81
	31	H	J 21	100	67	87
					Snap first.	Shock first.
Snap and Shock.	44σ	S	A 11	100	62	96
	"	H	"	"	62	62

The experiments of Dr. Tracy, above referred to, were all of this type. The conditions were practically the same as those above described, except that the room in which the subject sat was partially darkened. His subjects were somewhat experienced in

HAMLIN:

Group II. Stimuli addressed to different organs of the same sense; attention at a balance.

STIMULI	Interval.	Subject.	Date.	Number of Tests.	Percentage of correct replies	
					Right first.	Left first.
Two Clicks.	18σ	S	A 1	100	[85]	[77]
	"	D	J 24-26	"	[90]	[81]
	"	H	J 24-25	143	[59]	[81]
	"	"	J 26	105	[58]	[88]
	29	"	J 23-24	86	[84]	[81]
Two Shocks.	18σ	S	A 2	100	66	74
	"	Si	A 4	"	58	64
	"	H	A 2	"	62	64
	*29	S	A 8	40	90	75
	*"	H	"	"	85	80

The asterisks (*) indicate experiments omitted in making Table II below. A - August, J - July.

The experiments in which two clicks were used, bracketed figures in Group II, require a word of explanation. After experiments in this form were over, it was discovered that whenever the circuit of the right telephone was broken, a current was induced in the left circuit sufficient to cause a faint click

laboratory work, but had had little or no special training for this experiment.

Interval.	Subject.	Number of Tests.	Percentage of correct replies.	
			Flash first.	Click first.
13.7σ	S	200	58	55
	Da	100	52	38
	L	100	74	44
	B	100	54	50
	Do	100	66	56
	T	200	79	71
50σ	S	100	98	96
	Da	100	86	70
	L	100	66	68
	B	100	69	74
	Do	100	80	58
	T	100	94	92

in the left telephone. It might be supposed that this would tend to obscure the order of the clicks, especially when the right was given first, but this is believed not to have been the case. It is a well established fact that continuous sounds heard strongly by one ear and faintly by the other, are heard only on the side of the ear receiving the louder sound, and there seems to be no reason for thinking this untrue of clicks also. The fact that in the course of the experiments the presence of this faint induced click was never recognized is in accord with this hypothesis. A grain of evidence on the other side might appear to exist in the greater proportion of correct answers in the case of *H*, when the 18σ interval was used, but this is made doubtful by the non-appearance of the constant error with the 29σ interval. It seems a good deal more likely that with the shorter interval we have simply a case of well-marked constant difference. It is perhaps of interest to mention that both *S* and *H* hear more acutely with the left than the right ear, and *H* a good deal more acutely with either ear than *S*.

In order to facilitate comparison of the general results of this table, the intervals required to give 75% of right answers have been calculated according to the table in Cattell and Fullerton's work, "On the Perception of Small Differences" (p. 16), and are given in Table II. Since the object here is not

TABLE II.

Intervals required to give 75% of right judgments, compared with results of Exner, Tracy and Bloch.

	F-C	C-F	F-S	S-F	C-S	S-C	C - C		S - S	
							Left first.	Right first.	Left first.	Right first.
Si D H	32σ	37σ	48σ	28σ	(98σ)	(17σ)	16σ	12σ	19σ	30σ
	—	—	—	—	35	23	—	—	34	60
	—	—	—	—	—	—	14	9	—	—
	35	169	98	98	48 (98)	19 (98)	15	44	34	40
Exner.	160	63	71	53	—	—	64	64	—	—
Tracy.	44	67	F = flash ; C = click ; S = shock. Figures in parenthesis under C-S and S-C were gotten with a snap of an electric spark instead of the click of a telephone.							
Bloch.	28	36								

the exact record of results, but a picture of the phenomena under examination, some series of tests (those starred in Table I) have been omitted, four because the size of the interval used did not give results suited for calculation, and two because the number of tests was small; all other series of Table I have been included. With the same end in view, the results from tests made with different intervals have been combined. It hardly need be said that no importance is attached to the precise figures thus calculated, and no statements are made with regard to them that are not justified by the original records. The records of Exner, though made under different conditions and by a different psychophysical method, together with those of Tracy and Bloch are added for comparison.

The most noticeable thing in this table is the frequency of large constant differences; *S*, for example, in the combinations of flash and shock, requires a considerably larger interval if the order is F-S than if it is S-F, and *H* requires a much greater interval for C-F than for F-C. Some of these are evidently personal differences, one observer succeeding best with one order, the other finding no difference or succeeding best with the other. Reference to Table I shows also that some of the differences seem to depend on the general difficulty of making any judgment, and disappear, or even take the opposite sign when the interval is increased. This, at least, is the case for *H* in the C-C experiments and for *S* in the S-S experiments.

A tolerable unanimity, however, was found with the flash-and-click combination; the interval required for C-F was always larger than for F-C. In several series the difference is so slight that it may be accidental, but in others it is marked and in all there is no contrary case. In Tracy's twelve series there are only two instances of differences in the contrary direction, and these are slight in amount. In the experiments of Bloch, if the two studies are comparable, the same is to be observed. This is the more interesting as it is flatly opposed to the direction of the constant difference as observed by Exner in all seven of his subjects.¹ The relation is too constant in both cases to be set down as mere accident, and must be referred to some variation in the conditions of experimentation. The variations are so numerous that it is hard to fix with certainty upon the particular one, but three

¹ Gonnessiat also concludes from his experiments that the visual stimulus (the instantaneous flash of an artificial star) must precede the auditory (telephone click) by about 50σ , in order that they seem simultaneous, thus supporting Exner.

have a certain plausibility. In the first place, Exner's records are all naïve; in order to equalize the effect of practice, all practice was excluded and short series were taken on unpracticed subjects. In the long series of Tracy and the writer, the subjects could not remain unpracticed, and it may be that with increasing expertness changes in the direction of the constant differences occur. The records of the writer's experiments, while not very conclusive on this question, do not give evidence of such a change. In most of Exner's experiments, also, attention seems to have been given chiefly to one stimulus or the other, while in the writer's it was held in balance. It seems more likely, however, that the contrary direction of the constant difference depends on the fact that series of pairs of stimuli were used in one case and single pairs in the other.¹ In his experiments on personal equation, Gonnessiat finds rhythm a very important factor, and it may have been effective in these experiments of Exner's. Why Gonnessiat himself should have gotten such results as he did in trying to make the two stimuli seem simultaneous, is not easy to explain from data at hand, but it is highly probable that getting a coincidence of the two sensations is quite a different process from determining which comes first.

Whatever the cause may be, the fact remains that for single pairs of stimuli and for many observers the order F-C requires a less, or at most no greater interval than the order C-F, and this necessitates a revision of the explanation offered for the constant difference by Exner,² which makes the difference depend on the slower rise and greater duration of the visual sensation. The constant differences also that occur in the case of the click and shock, and especially those in case of stimuli to different organs of the same sense, cannot well be made to fit with an explanation based wholly on sensory inertia. It is more natural to refer the phenomenon, as Wundt does chiefly (*Phys. Psy.*, 4te Aufl., II, 392), to differences in the direction of attention, either habitual or depending on the nature and intensity of the stimulus, the sensation receiving preponderant attention appearing to come first. Habitual attention to what may be seen, in preference to what may be heard, is characteristic of many minds. The electric shock in the touch experiments, also, though not distinctly greater in intensity than the flash and click, would probably be regarded by most subjects as more intrusive.³

¹*Op. cit.*, pp. 430-431.

²*Op. cit.*, p. 425.

³A delay in the response of the telephones, if such there were, might explain some of the cases, but such a delay is practically out

II.

Experiments with Forced Attention.

Experiments on Voluntarily Directed Attention. The experiments were undertaken in full expectation of finding a definite relation between the direction of attention and the apparent order of the stimuli. Several short series of experiments were made with different pairs of stimuli, (a total of 360 for *S*, and of 278 for *H*), but nothing like a definite connection could be made out, and the experiments were broken off. So far as the figures showed anything, they showed that forced attention was as often a hindrance as a help, even when the leading stimulus was attended to, and that attention to the following stimulus was often advantageous. A good deal of support is given to the conclusion that voluntary attention is ineffective by the general observation that the best results were reached when the subject was in a state of indifference. Of this, more will be said presently.

Experiments on Involuntarily Directed Attention. Experiments with stimuli of different intensities were made in the expectation that the fainter stimulus could be so far reduced as to compel the subject's attention involuntarily to it through his fear of losing it. This seems to have succeeded with *H*; for the faint sound was often lost altogether, and she felt a straining of the attention towards the weaker stimulus. Yet, when heard, it usually seemed to lead and gave the subject 80% right when the weak sound led, to 32% when the loud sound led. The record of *S* showed the opposite tendency. He seems not to have felt the compulsion toward the weaker stimulus, but allowed his attention to balance indifferently between the two until one sound was made very faint, and then felt only an occasional tendency to listen for the weaker sound. When the stronger stimulus led, this subject had 92% of his judgments right, when the weaker led, only 42%. Differences like these seem to indicate an individual caprice of the attention. It is caught in one

of the question. With the apparatus at hand, it was not possible to measure the delay of the telephone plate when it moved as little as in these experiments. Chronographic tests, when a stronger current was used showed no appreciable delay. No regard was had in the experiments to whether the first movement of the telephone plate was toward the ear or away from it, but the differences in time introduced by this must be extremely small. This problematic influence of the apparatus was completely avoided, when the snap of an induction spark was substituted for the telephone click. With *H* this seemed to make a difference; with *S*, however, the constant error remained as before.

case by a loud sound, in the other by a faint sound. And the individual tendency is so persistent, that even when a subject knows that his attention has been partial to one stimulus, he cannot correct the tendency. All these attention experiments seem to show, in a single word, the helpfulness of spontaneous as opposed to voluntary attention.

Besides the experiments already mentioned, a few records were taken when three stimuli were given the subject. The click and the shock were sent at the same instant, and separated from the flash by an interval of 18σ . The subject recorded the apparent order of the three. Both subjects found that the click seemed to lag behind the other two stimuli, and noticed a growing tendency to ignore the sound altogether, making the experiment practically a repetition of the flash and shock tests, at a shorter interval.

A certain interest may attach to the following general observations:

When an observer is practiced the interval between the stimuli sometimes seems quite an extensive period of time. In judging the order of a flash and click at an interval of 18σ , *S* noticed several times that he distinctly waited for the second stimulus. When the flash came first he had a definite sensation of darkness before hearing the click. *H* noticed something similar at times. When listening to the two clicks at an interval of 30σ , this subject had a muscular sensation apparently intervening between the two sounds, as if the interval had been long enough for the attention to shift its muscular adjustment after receiving the first, before taking in the second.

The interpretation of the successive stimuli in terms of motion, noticed by Exner was frequently observed in these experiments. It was prominent in the two shock experiments and in those with the flash and shock, but with the two clicks it was much fainter or entirely absent.

III.

Conclusions.

During the progress of the experiments the chief subjects were on the watch for introspective suggestions, but introspection under the conditions of this experiment proved unusually hard, and not much was obtained in this way. A little light is furnished, however, by an observation, partly introspective, partly external, which was early made by Tracy and was repeatedly confirmed in the writer's experiments, namely, that the best results were reached when the subject assumed a certain indifference, awaited the stimuli

without strained attention and based his reply upon a "general feeling" of their order rather than on a clear recognition of it. The condition of mind may be most accurately designated as "alert indifference," a condition in which voluntary effort furnishes the general ground, but nothing more. The subject was always in an easy and unconstrained position, but the muscles of the head, of breathing, of the eye, ear, or hand, were somewhat innervated. This was occasionally shown by some slight muscular reaction; the hand would twitch in response to a very faint electric shock, or the eyes would wink when the flash came. But along with this degree of sensitiveness or alertness, there was a mental nonchalance. The subject was in a passive state, free from any sense of care or effort, and with no lasting memory of what was taking place. If a judgment was not recorded at once, it could not be given at all.¹

The hypothesis which the experiments have suggested with reference to the psychophysical mechanism by which such judgments of order are made is briefly as follows: The sudden entrance of any stimulus causes an immediate reflex response of adjustment in the organ in case of the eye and ear, and perhaps of withdrawal of the member in the case of the hand. What is really compared in judging the order of the stimuli, is not the special sense impressions, but the sensations resulting from these reflex movements. In order that these reflex responses may be prompt, the subject must be alert, but must not be voluntarily attentive, because voluntarily attention causes beforehand a more or less complete adaptation of the organs in question, and thus obscures the reflex response. The problem would then become one of the necessary length of the just observable interval between two sensations of movement, a problem on which, so far as the writer knows, no other data are extant.

This conception of the mechanism and the observations on which it rests, agree well with the first of Exner's several types of attention. The first is thus described: (p. 429) "We adjust our attention for the first stimulus that is to reach us, of course without knowing which it is; and not for this alone, but also — I cannot express myself differently — for the condition of the sensorium at the instant of this first stimulus. By this adjustment that instant is fixed in memory, and which of the two stimuli was the one adjusted

¹To S it sometimes seemed that there was a certain tendency to regard the sensation which seemed stronger in consciousness as the first, if the judgment was made from recollection and not from immediate sensation, but he is unwilling to attach much importance to this observation.

for can be recognized in the memory image; that one is then the first. The second stimulus is wanting in this memory image in so far as the image results from an exact adjustment. The limit of the discriminable is reached, when it is no longer possible to fixate the first stimulus alone." This method was noticed by Exner only in the case of the separate stimuli to the two ears, and, while he thinks it may possibly have occurred in the other cases also, he is inclined to believe that the close resemblance of the two-stimuli is an essential condition of its development. This restriction seems doubtful; at least, in the experiments of this study, no difference in type was noticed in the different combinations of stimuli. It is much more probable that this type is characteristic of experiments in which a single pair of stimuli are judged, and that the type which Exner found for stimuli to disparate senses belongs to the rhythmically recurring pairs of stimuli. As already explained, the two click experiments of Exner were the only ones where his apparatus seems to have allowed the production of a single pair.

The results of this study may be summarized briefly as follows:

1. The interval that must separate instantaneous stimuli addressed to disparate senses, or to different organs of the same sense in order that their order may be recognized, has been measured for single pairs of stimuli, and by a method as nearly as possible the same in all cases. The results of this measurement, besides indicating some changes in the figures commonly given for these intervals, make the explanation of the constant errors, found with the click and flash, by optical inertia apparently unnecessary.

2. The effect of voluntary attention has been examined, and so far as the experiments go, has been shown not to cause the stimulus for which attention is set to seem to lead in time. Throughout the experiments, on the contrary, the importance of spontaneous attention, or at least the spontaneous reaction of the psychophysical mechanism has everywhere appeared.

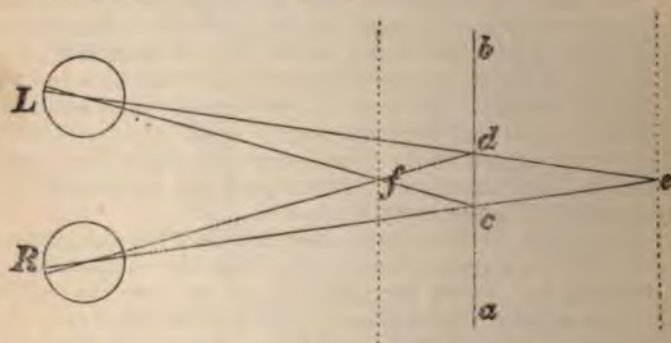
XI. NOTES ON NEW APPARATUS.

BY EDMUND C. SANFORD.

The Binocular Stroboscope. The purpose of this note is the double one of calling attention to a little known phenomenon of binocular vision and describing an instrument by which it

may be demonstrated. The phenomenon was long ago studied by Dvorák, but his paper is hidden away in the Proceedings of the Bohemian Royal Society of Sciences,¹ and has received little notice. The only contribution of the present writer is such a simplification of the instrument as makes the phenomenon demonstrable in any laboratory that has a vertical color-wheel and a mirror. The phenomenon in question is, as the title of Dvorák's paper indicates, a sort of "personal equation" between the two eyes. If the right eye receives a stimulus and a little later the left eye receives another, the two stimuli, if close together, may seem simultaneous and be credited to a single cause, or, if further apart, may be recognized as separate and credited to separate causes. Quantitative measurements of the time that must separate two such stimuli were made by Dvorák and some preliminary work in the same line has been done in this laboratory.

The point of present interest is, however, not this general case, but the particular one in which the stimuli are separate glimpses of a moving object. When proper conditions are observed such successive glimpses are united into an illusory perception of distance. The nature of this illusion will be made clear by the following diagram:

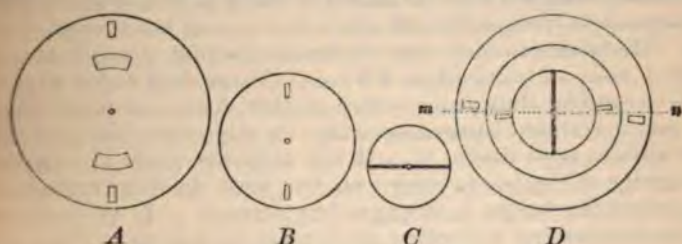


R and *L* represent the right and left eyes; the object moves in the line *a b*. Suppose in the first place that the object starts from *a* and moves upward, and that when it reaches *c* it is seen for an instant by the right eye and when it reaches *d*, for an instant by the left. The united perception will then be located at the intersection of the lines of sight, that is at *e*. If the order in which it is seen by the eyes is reversed (the

¹Ueber Analoga der persönlichen Differenz zwischen beiden Augen und den Netzhautstellen desselben Auges. Sitz.-ber. d. k. böhm. Gesells. d. Wis. in Prag; Jahrgang, 1872. Jan.-Juni, pp. 65-74.

direction of motion remaining the same), the left eye will see it at *c* and the right eye at *d*. In this case as before it will seem to be at the intersection of the lines of sight—this time at *f*. This is true for both direct and indirect vision, and gives a certain support — probably more apparent than real—to the old projection theory of visual localization.

The simplest means of getting such instantaneous glimpses of a moving object is to use a disk with radial slits as in the ordinary stroboscope. The instrument is easily made binocular by cutting separate slits for the two eyes, and both binocular and adjustable as to the order in which the eyes are used, by applying a smaller disk concentrically over the first. *A* and *B* in the diagram below are a pair of such disks.



The moving object in Dvorák's apparatus was a broad band of paper with vertical stripes, placed a little distance behind the disks and driven by the same means as they. In the simplified instrument the moving object is a figure of some kind painted either directly on the smaller disk or on a third still smaller disk placed upon the same spindle as the other two. Such a figured disk is represented by *C*, and the moving object is the black bar which it carries. In *D* the disks are shown combined for use.

In using the instrument the combined disks are placed on the color-wheel, and at a convenient distance before them a mirror so set that the observer, looking through the slits from behind into the mirror sees the reflection of the face of the disks. The bar of the smallest disk should be vertical when the diameter that halves the angle between the slots, *mn* in *D*, is horizontal. The instrument may be used without further addition, the eyes being brought close to the slits at about the height of the spindle. It is better, however, to place between the eyes of the observer and the slits a screen of black cardboard with a narrow horizontal slit (placed radially with reference to the disk) so as to prevent the observer from seeing through the slits except when they are immediately before his eyes. When the instrument is thus set up, and the disks are so

adjusted that the right eye sees first, and the direction of rotation as seen in the mirror is like that of the hands of a watch, the observer sees the image of the bar inclined toward him at the upper end and away from him at the bottom. When the left eye leads the inclination is reversed. The effect is most striking when the lead of one eye over the other is rather small, for otherwise the positions of the bar are too discordant for easy spatial interpretation; a lead of five or six degrees is sufficient.¹

The size of the disks must of course be varied to suit the machine upon which they are to be used. The following dimensions will probably be found convenient on most color-wheels: Radius of large disk, 15 cm.; of small disk, 10.5 cm.; of figured disk, 6.5 cm. Distance of outer edge of narrow slits in large disk from centre of disk, 14 cm.; of inner edge, 11.5 cm. Distance of outer edge of broad openings from centre of disk, 9 cm.; of inner edge, 6.5 cm. Distance of outer edge of slits in smaller disk from centre of disk, 9 cm.; of inner edge, 6.5 cm. Extent of broad openings in large disk, 40°; of narrow slits in both disks, 5°. If the slits are made too narrow the image of the bar is clear cut, but weak in illumination; if too broad the image is stronger but blurred. In other forms of the instrument it is often convenient to use more than two slits in each disk, but in this it is a disadvantage, for with more slits the bar is seen more frequently and in positions where the separate glimpses are not capable of a spatial interpretation. The result is a case of irreducible double images, as may be seen by using the disks when the bar of the figured disk is brought into the diameter that halves the angle between the slits.

Other distortions of the image of the disks in the mirror can sometimes be observed, but they can for the most part be easily explained on the principles already set forth.

A Model of the Field of Regard. The movements of the eyes and their effect on visual localization are an interesting, but somewhat difficult topic in the psychology of vision. One

¹Another satisfactory figure is one which consists of a couple of heavy black rings placed near where the ends of the bar now lie in C. When this figure is used and the right eye leads, the upper ring will look smaller and nearer, the lower one larger and further away. A still more interesting case, but one which requires an independent moving object, is that in which the object moves in a horizontal circle, a vertical wire, for example, moving upon a circle three or four inches in diameter. When seen through the disks its path seems to be elliptical, the apparent direction of the long axis of the ellipse and the direction of the motion depending, in part, at least, on the order in which the eyes are used.

difficulty is getting a notion of what the hemispherical field of regard looks like and what its relations are to the plane field on which experiments are generally made. To assist in removing such difficulties as these, the model about to be described was constructed. A stereoscopic picture of the part that represents the hemispherical field, and an ordinary diagram of the part that represents the corresponding plane field will be found in the appendix to the section of laboratory experiments on the Visual Perception of Space given below. A detailed account of what the model presents is given at the same place and may be omitted here.

The general plan of the model will easily be understood by reference to the stereoscopic figure. The framework is of wood, its most important portion being the face board—that carrying the letters—which is twenty-eight inches square—seven eighths of an inch thick, and has a twenty-four inch circle cut from its centre. To this are fastened at *A*, *B*, *C* and *D* semi-circles of brass of two-inch radius and three sixteenths of an inch thick. To these were first soldered the prime meridian *AB* and the equator *CD*. The wires used were of iron and about three thirty-seconds of an inch in diameter. The wire when bought was coiled with a radius nearly equal to that of the hemisphere to be constructed so that little bending was necessary. The remaining meridians were next soldered in their places upon *CD* and on the brass plates at *A* and *B*, and after them the parallels and the oblique circles *EF* and *GH*. The crosses, cut from tin, were then put in place and finally the small circles *IJ*, *KL*, *MN* and *OP*—Helmholtz's Circles of Direction—and the whole completed by painting the face board and wires a dull black.¹

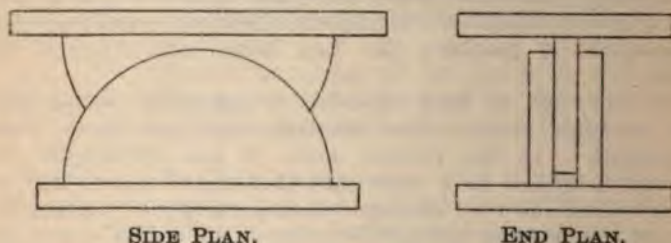
It was not at first intended to have the model show the relations between the hemispherical and plane fields of regard. In the present form of the instrument this has given place to a wide and thin board which slips into place behind the wire hemisphere and stands for a plane tangent to the latter at the middle point of the central cross. On this has been drawn a gnomonic projection of the wires and crosses of the hemispherical field, in other words, the figures that would be made by the shadows of these parts cast by a point of light in the centre of the lettered circle. These projections may be gotten mathematically by calculation or empirically by actually casting the shadows and tracing them. The diagram in the appendix was drawn by calculation, but

¹The white letters were made by tracing from a stencil on bits of cardboard and then painting over all other parts with black.

with the actual model the shadow plan was chiefly used as promising to accommodate itself more easily to small accidental irregularities in construction. By this combination of the plane and hemispherical fields the student can instantly satisfy himself that what he sees in one field is in reality an exact representation of what he sees in the other by bringing his eye to the centre of the hemisphere and seeing that the lines coincide.

The instrument as described is large and suitable for class demonstration. An instrument one quarter the size would answer equally well for individual inspection and would be much less cumbersome; indeed, the stereoscopic diagram, in connection with the corresponding diagram of the plane field, serves almost every purpose. The writer has had a few extra copies of both struck off, and will be glad to furnish them, as long as they hold out, to any one interested in them.

A Simple Adjustable Stand. This piece of apparatus is easily within the skill of any one that handles ordinary tools. Its plan will be clear from the diagram below. It consists of a base board twelve inches long, eight wide and seven-eighths of an inch thick. Lengthwise of this and seven-sixteenths of an inch on either side of its middle line, are placed two vertical semicircles of wood, of five-inch radius and seven-eighths of an inch thickness.



The upper part of the instrument is like the base board, except that it has a single semi-circle of wood along its middle line, and that it is a little larger—fourteen by ten inches.

When the instrument is put together, the semicircle of the upper part slips in snugly between the semicircles of the lower part, and the whole is fastened in any required position by means of an ordinary iron clamp, which squeezes the lower semicircles against the upper. The instrument made

in this way allows an angular adjustment of 90° in either direction from its middle position, and a vertical adjustment of three inches or more. When the clamp has just brought the sides together, the parts slide upon one another, and will retain any position given them, but when the clamp is screwed solid, the whole is as rigid as if made of a single piece. It is, of course, not necessary to use semicircular boards for connecting the base board and the top; almost any shape will serve, and some other shapes give even a greater range of adjustment.

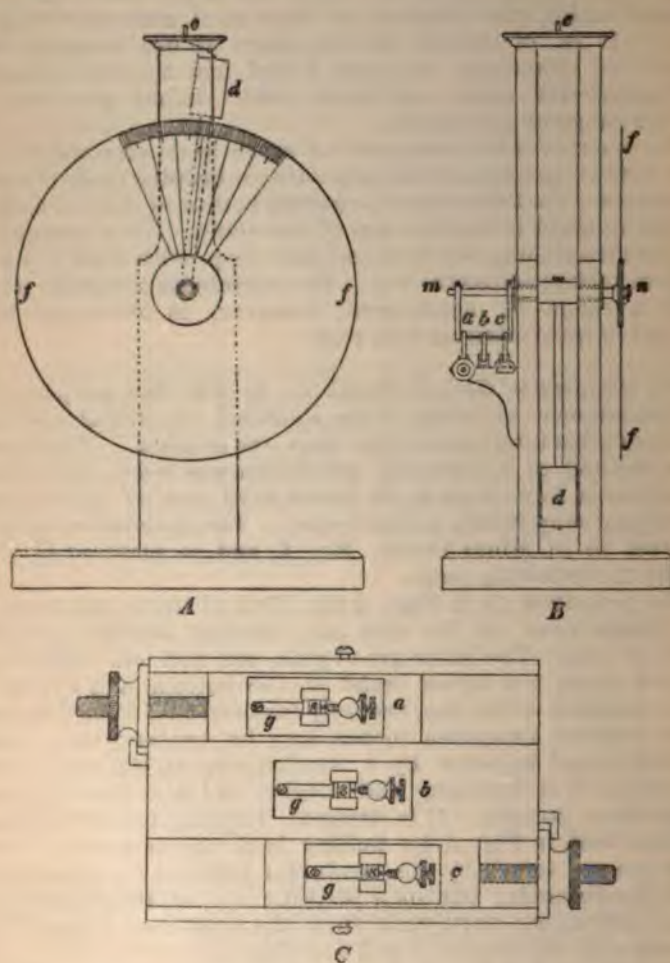
The stand was first designed to serve as an arm rest when it is desired to support the arm without keeping it absolutely motionless. For this purpose, a square bottomed, opened-ended trough of wood is used on top of the stand. This trough is twelve inches long, six wide and four deep, with a slack piece of cloth tacked across the top. The usefulness of a stand with such a range of adjustment, however, is obviously not limited to furnishing an arm rest.

The Pendulum Circuit Breaker. It was the purpose of this instrument to break three electrical circuits at known, and regulable intervals of time from one another. The familiar method of a swinging pendulum was used, but some adaptations have been made which have proved convenient in use and may justify a description. The instrument is that referred to in Minor Study, No. X, and is pictured in the cut on the following page:

The pendulum (d in Figs. *A* and *B*) is of brass and swings completely over on the axes mn , between the two upright posts of wood. The latter are of pine, one and one eighth of an inch thick, five inches broad at the bottom, and two and three quarters at the top, securely fastened into a base of the same material (eighteen inches long by twelve wide), and further braced together by a wooden cap at the top. The pendulum is a foot long and weighs, rod and bar together about three pounds. It is dropped from the nearly vertical position seen in Fig. *A*, by pulling back the release e . The keys are not struck by the pendulum itself as is common in such instruments, but by a striking bar or frame-work of brass, extending downward from the shaft mn , shown in contact with the keys a , b and c in Fig. *B*.

The other end of the shaft mn is finished like the spindle of a color-wheel for receiving a large disk f . As used in Study No. X, this disk was of tin and about seventeen inches in diameter. Three functions may be performed by the disk: It furnishes a surface that can be smoked over and used for timing the pendulum chronographically; it furnishes a means,

when pierced with a radial slot and illuminated from behind, of producing a very brief flash of light; and, by the scale of degrees scratched on its edge, it furnishes a means of setting the keys without the use of their micrometer screws. In



Study No. X, it was used only for the first and last purposes, the flash being produced in another way.

Fig. C gives a plan of the keys as seen from above. Keys *a* and *c* were movable, key *b* was fixed in a central position,

so chosen that the striking bar was just in contact with it when the pendulum was at rest in its middle position. The shelf on which the keys stood was of iron, and each had to be insulated from it; they were, therefore, set on pieces of hard rubber, *g, g, g*. The upright arms of the keys are lettered *s, s, s*, and each was so shaped at the bottom that when it was erect, it was held in place by the spring (near the *g*'s in Fig. *C*), and when it was thrown down it was prevented by the same spring from rebounding. On either side of the shelf was a set screw for fastening the keys in place when once adjusted. The micrometer screws attached to *a* and *c* could have been used for setting them, but it seemed better in Study No. X, to use the screws for making small changes in the position of the keys, and to do the setting directly from the disk as already mentioned.

The setting was accomplished as follows: The disk *f* being clamped tightly in place, a fixed point was fastened to the base of the instrument and brought close up to the lower edge. The keys *a, b* and *c* were connected with circuits in which were telephones or other apparatus for announcing the instant at which the respective keys were struck and their circuits broken. The pendulum was then lowered by hand and carried slowly by its middle position, and an exact reading obtained of the point on the degree scale at which the click announced that a key was struck. The whole degrees were shown by the scale, the tenths were estimated by eye. By chronographic measurement it had previously been determined that one degree corresponded to 1.6σ , and knowing this it was easy to set the keys to any required interval.

In using the instrument for the production of two nearly simultaneous stimuli in reversible order as required in Study No. X, the keys were so connected with the stimulation apparatus that the break at *b* gave one stimulus, *e. g.*, the flash of a Geissler tube, and that a break in either *a* or *c* (by parallel wiring) gave the other, *e. g.*, a click in a telephone. Whether the click should lead or follow the flash, was then controllable at will by a switch in the hands of the operator, all three keys being set up each time.

The value of such a piece of apparatus depends on its accuracy. The following records of chronographic tests with a Deprez signal and tuning-fork, giving approximately 149 vibrations per second, show a degree of accuracy very satisfactory for all purposes for which the instrument is likely to be used. The table gives the average number of vibrations of the fork for the 60° at the bottom of the pendulum's arc:

Table Showing Average Rate of the Pendulum.

Date.	Number of Trials.	Average. No. of vib.	Mean Variation.	Maximum.	Minimum.
J-7	22	14.13	0.06	14.25	14.00
J-22	34	14.15	0.07	14.30	13.95
A-3	14	14.13	0.09	14.30	14.00
A-3	12	14.08	0.05	14.20	14.00

Whether the keys were raised so that they were struck by the pendulum, or were turned down so that it swung free, made no appreciable difference in its rate. Of the two sets of tests on August 3, the first was taken after the instrument had been used for several days without re-oiling, the second immediately after fresh oiling. The oiling appears to have reduced the time for 60° by about seven one hundredths of one tuning-fork vibration (roughly 0.5σ), or about one part in two hundred.

The advantages of this instrument are the swinging of the pendulum completely over and the attachment of the disk for chronographic control of its rate and accuracy. The pendulum rises well up at the end of its swing, and is easily caught by hand and carried on till it rests again at *e* ready for another fall. The complete swing makes the apparatus compact by bringing the catch from which the pendulum falls above the axis instead of at one side, and also avoids the backward swing which is often a considerable inconvenience in the common form of the pendulum chronograph. The advantage of the disk over a plate fastened to the pendulum itself, is the greater ease of adjusting writing points to a surface with which they are constantly in contact.

A pendulum of this kind with the attached disk is probably the simplest and cheapest time apparatus at present attainable for reaction-times or any other brief time intervals. With a disk permanently scratched with lines corresponding to hundredths of a second (following an idea suggested by Bowditch for a similar purpose) or even thousandths, determinations could be made both rapidly and accurately, especially if one of the keys *a*, *b* or *c*, were used for giving the stimulus so that the time to be measured should always begin at the same place in the swing.

ON THE WORDS FOR "ANGER" IN CERTAIN LANGUAGES.

A STUDY IN LINGUISTIC PSYCHOLOGY.

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In such works as Darwin's "On the Expression of the Emotions in Man and Animals," Romanes' "Mental Evolution in Man and Animals," Mantegazza's "Fisionomia e Mimica," "Fisiologia dell'odio," etc., one looks in vain for a section devoted to the examination of the concepts of the various emotions as revealed by the terms used by the ruder and more primitive peoples to denote them. In connection with the theory of the emotions put forth by Prof. James, and as illustrations of the intimate kinship of psychology and philology, such investigations are of no little interest. Studies of the emotion of love from this stand-point have been made by Dr. D. G. Brinton (3), for certain American aboriginal tongues, and by Dr. Carl Abel for Latin, Hebrew, Russian and English (1).

So far as the present writer is aware, no attempt has been made to discuss the subject of anger, and this brief essay is intended a beginning in the sematology of that emotion. First, let us glance at our own language. *Anger*, which in Middle English meant "affliction, sorrow, wrath, pain, inflammation" — we still speak of "an angry wound or sore," and the familiar phrase of Shakespeare "more in sorrow than in anger," preserves traces of the kin of grief — is borrowed from Scandinavian, and, with its cognates: Icelandic *angr* (grief, sorrow); Danish *anger* (compunction, regret); Swedish *anger* (repentance, penitence, regret, compunction, sorrow) — the adjective *anger* signifies "afraid, sorry"; Latin *angor* (strangling, throttling, quinsy, bodily torture, anguish, vexation, grief, sorrow; *angere*, to compress, stifle, choke, strain, strangle, broil, twitch, gripe, trouble, torment, vex); Greek *ἀχος* (ache, pain, distress — used in Homer of the mind only; *ἀγχω*, "I mourn, am sad at heart, grieve, I vex, distress, make to grieve"; *ἀγχω*, "I press tight, press the throat, strangle, throttle," etc., go back to a primitive Indo-European radical *agh* (*angh*) expressive of the very physical idea, "to choke, to oppress, to constrict." Our common English phrase "choked with anger" is really tautological at bottom, for *anger* once meant *choking*. Other interesting words which have sprung from the same root are: *awe* (fear, dread), cognate with Icelandic *agi* (terror), Danish *ave* (check, control, restraint); Gothic *agis* (fear, fright, terror); and (probably) Sanskrit *amhas* (pain), *agha* (sin); *ugly* (frightful, hateful — compare German *ässlich*; we speak of "an ugly temper," and in American English *ugly* signifies "ill-tempered, gross-grained, vicious"); cognate with Icelandic *uggligr* (fearful, frightful, dreadful, to be feared; *uggligr*, terrible; *ygr*, fierce), *uggr* (fear), *ugga* (to fear), *ögn* (ter-

ror), *ōgna* (to threaten); Gothic *ōgan* (to fear), *ogjan* (to terrify); all of which are from a base *ag* or *og*, "to fear," itself a derivative from *agh* in the larger sense indicated above. Our *ache* is also related, a memory of which is yet present in the assertion of the school-boy preparing to assail his opponent, "I'm just *aching* to get at you." Another shoot from the same stock is German *Angst* (anguish, anxiety, fear, pang), a word apparently occurring only in the High German dialects — Gothic has, however, *aggwītha*, "anguish" — and related to Latin *angustia* ("narrowness, straitness, difficulty" — whence French *angoisse*, our *anguish*), and German *enge* (narrow, restricted), *bange* ("anxious, afraid," from *be+enge*), which latter word in Middle High German was also a substantive, with the meaning "sorrow, anxiety."

Another English word for "anger, indignation" is *wrath*, a substantive, derived from the Anglo-Saxon adjective *wrōð* (whence also our *wroth*, "angry, wrathful"), and cognate with Icelandic *reiði* (wrath). The correlation of *wroth* is with Icelandic *reiðr*, Danish and Swedish *vred* (angry, wrathful); Dutch *wreed* (cruel, hard, harsh), and *reit*, which in Middle and Old High German meant "twisted, curled." In truth, the Anglo-Saxon *wrōð* is but the past of *wriðan* (our *writh*, "to twist to and fro," Icelandic *riða*, Danish *vide*, Swedish *rida*, Old High German [the word is lost in the modern tongue] *ridan* "to wring, twist, turn, wrest, to wind"); going back to an Indo-European root *uert*, seen also in Latin *uertere*, "to turn, twist." A man *wroth*, then, is literally one whose mind or body is "turned, twisted, awry."

Our word *cross* (ill-tempered, angry), in Middle English, *crous*, finds its cognates in Dutch *kroes* (curled, crumpled, confused, cross, stubborn); Modern German *kraus* (curly, crinkled, crisp, etc.). The Low German proverb: "Krüse hār un kruse sin, dār sit de düfel drēmal in," brings out the same idea in curious fashion, and Martin Luther delighted to hurl at one of his opponents the significant taunt "Krauses Haar, krauser Sinn." To be *cross*, then, is to have a mind that is "curled, crooked." A similar turn of thought appears in the word "crook," and Lombroso and the anthropological psychiatrists may well be pardoned for maintaining that a "crook" is a man with a "crooked body" as well as a "crooked mind."

Zorn (anger, wrath, passion, rage, indignation, irritation), which in Middle and Old High German had the further meanings "quarrel, wordy encounter, brawl, dispute, violence, rage of the elements, affront, insult," is cognate with Old Saxon *torn* (anger, indignation); Dutch *toorn* (anger); *torn* (shock, strife, contest, tearing apart of a seam, ripping); Anglo-Saxon *torn* (anger, insult), and seems to be an old participle from the root *ter* seen in our verb to *tear*; Old High German *zēran* (to tear, destroy); Gothic *gatairan* (to tear); Russian *drate* (to tear); Lithuanian *dirti* (to flay); Greek *δέπειν* (to flay); Zend *dar* (to cut); Sanskrit *dri* (to burst, to burst open, to tear asunder); the Indo-European radical at the base of all being *der* (to burst, to tear asunder). We find also the verb *zürnen* and the adjective *zornig*. Judged by the word *zorn*, therefore, "anger" reveals "a *torn* mind"—we still say "distracted with grief" and "torn by conflicting emotions," and speak of "tearing around," "being on a *tear*."

Another word for "anger, fury, rage" in Modern German is *Grimm*, an indication of whose older signification is found in the compound *Bauch-grimmen*. In Old High German *grim* meant "anger, rage, hostility, fierceness, pain;" Dutch *grim* (anger, fury). In our "*grim* Death," we have preserved one of the many meanings of the corresponding adjective (fierce, angry-looking, etc.); cog-

nate with Dutch *grimmig* ("angry"—*grimmen*, "to foam with rage"); Icelandic *grimr* (grim, stern), Danish *grim* (grim, ugly); Swedish *grym* ("cruel, grim, furious"—*grymta*, "to grunt"); Old High German *grim*, *grimmi* (wild, fierce, hostile, terrible, violent, painful); Modern High German *grimm*, *grimmig* (enraged, furious, wrathful, fierce, violent, grim). Here again the kinship of "anger" and "sorrow" appears, for from the same root as *Grimm* comes *Gram* (grief, sorrow, etc.). The adjective *gram* (hostile) is cognate with Icelandic *gramr* (wrathful); Danish *gram* (wrathful); Gothic *gram* "angry"—seen only in the verb *gramjan* (to make angry, to excite to anger); Anglo-Saxon *gram*. The Anglo-Saxon *grimetan* (to rage, roar, grunt); Russian *gremiete* ("to thunder"—*grom*, "a loud noise"), and the distantly related *grin*, *groan*, *grumble*, indicate the ultimate origin which is from the Indo-European *ghrm* (to make a loud noise), derived from the more primitive *ghr* (to make a noise, to yell). In like manner we speak of a *grumpy* or *grumpish* man, meaning one who is crabbed or ill-tempered. Employing the same metaphor we speak of "howling with rage," "bellowing with anger," and "groaning in spirit."

A very common expression in German for "to be angry" is "auf einen böse sein." *Böse*, which now signifies "bad, evil, wicked, angry, sore, cross, ill-tempered, malicious," and of children, "naughty," is peculiarly a German word not being found in other dialects. In Middle and Old High German *bæse* and *bōsi* had the meanings "bad, worthless, evil, greedy, slanderous," and Kluge cites the Old High German *bōsōn* (to slander, to revile) as indicating that the original meaning of *böse* was "slandering, maliciously speaking."

In Middle English we meet with *wodewroth* (madly angry) and *wode* (mad, raging), the *wood* (mad, furious) of Shakespeare, cognate with Icelandic *óðr* (raging, frantic); Gothic *wods* (raving, raging, possessed). The corresponding substantive is seen in Dutch *woede* (rage, fury, madness); Modern German *Wut* (rage, fury, madness); the adjective is *wütig*, the verb *wüten*. The Teutonic radical from which all of these come is *woda* (mad, furious, frantic). In Lowland Scotch *wod* or *wud* means "raving mad, stark mad." Related are also Anglo-Saxon *wōð* (voice, song); Icelandic *óðr* (poetry, song); Latin *vates* (bard, god-inspired poet); Irish *fáith* (bard), the radical idea being indicated by the Sanskrit *vat*—"to stir up the mind, to incite the mind"—a bard is one whose mind is filled with divine frenzy. Here belongs also perhaps the god *Woden*, whose *wütendes Heer* is well-known in German mythology. We even yet speak of a man in anger as being "stirred up," "aroused to indignation," "moved to wrath," etc.

Ire, *fury*, *rage*, *indignation*, *choler*, *passion*, *resentment*, we have borrowed from Latin, through French. *Resentment* (from French *ressentiment*, ultimately derived from Latin *re*, "again," *sentire*, "to feel," like the verb "to resent," has changed from its original signification, "being sensible of, having a sensible apprehension of," to that of being aggrieved at, taking ill, being indignant at, getting angry at." *Passion*, which in English means "suffering, strong agitation of mind, rage," comes to us through French *passion*, from the Latin *passio*, "suffering," cognate with *pator* (I suffer, endure).

Indignation, "anger at what is unworthy," is derived through French *indignation*, from Latin *indignatio*, "displeasure, indignation, disdain," which comes from *indignor* (I consider unworthy—*indignus*—I am indignant, I am displeased at). We find also Latin *indignitas* (unworthiness, indignity, indignation). In English

the phrase "righteous indignation" indicates the general idea at bottom of the word.

Ire, a word somewhat more elevated in stylistic use than *anger*, comes to us through French *ire*; from Latin *ira* (anger, wrath, passion, rage, violence, fury, indignation), of which the ultimate etymology is doubtful. A derivative of *ira* is *iracundia* (proneness to anger, hasty temper, irascibility, anger, wrath, rage, passion, violence). Familiar phrases are: *ira inflammatus*, *ira commotus*, *ira amantium* (lovers' quarrels). From its derivative, *irasci* (to become angry) is derived the adjective *irascibilis*, whence through French, our *irascible* (given to anger, choleric).

Fury, "anger, rage, passion," is derived through Old French *furie*; from Latin *furia* (fury, rage, madness, passion), cognate with *furere* (to rage, to be mad). Skeat correlates with Sanskrit *bhuranya*, "to be active," and refers back to the radical *bhur* (to move about quickly).

Rage, "fury, violent anger," comes into English through French *rage*; from Latin *rabies* (madness, rage, fury). In French *rage*, like the Modern Latin *rabies*, is applied to a mad dog — hydrophobia — and to other animals as well. The verb *rager* signifies in French "to be in a passion, to be angry, to sulk," and *rageur*, "a peevish person." Latin *rabies* is from *rabere* (to rave, to be mad); cognate with Sanskrit *rabh* (to desire vehemently, to act inconsiderately, to seize); the radical of both being Indo-European *rabh* (to seize). From a Low Latin word *rabiare*, derived from *rabia*, a by-form of *rabies*, come Spanish *rabiar* (to rave); Old French *resver*, French *rêver* (to dote, speak idly, rave), Old French *râver*, whence *ravasser* (to rave, to dote, to talk idly), and English *rave* ("to be mad, to talk like a madman;") we have also the phrase "raving mad".

Choler, "bile, anger," through Old French *cholere* (chola, anger); Latin *cholera* (bile, bilious, complaint, cholera), goes back to Greek *cholēra* (cholera—from *cholē* [also *χόλος*], "gall, bile, rage, anger, wrath, bitterness, anything causing disgust or aversion). These Greek words are cognate with Latin *fel* (gall, bile, anger, rage, animosity, bitterness), and English *gall* (gall, bitterness, anger, bile). The physical basis of the idea is clearly the "bile, gall." From Latin *bilis* (bile, anger, wrath, choler, indignation); through French *bile*, comes our word *bile* (secretion from the liver, bitterness, anger, etc.). Here, again the physical basis of the idea is plain.

Skeat defines *spleen* as "a spongy gland above the kidney, supposed by the ancients to be the seat of anger and ill-humored melancholy," and we talk of "venting our *spleen* upon any one," and of a *splenetic* person—the word comes through Latin *splen* from Greek *σπλήν*, cognate with Sanskrit (*S*)*plīhan*.

In Nipissing, a typical Algonkian dialect of Canada, the words for "anger," *nickatisiwin*, "to be angry," *nickatis*, etc., come from the radical *nick*, which signifies both "angry" and "gland," showing clearly the physical basis of the concept. To Nipissing correspond the Ojibwé *dishkadisiwin*, *nishkadis*, *nishk* (5, p. 270).

Canon Farrar says (6, p. 197): "In Greek the diaphragm (*φρήν*, *renes*, reins) is used for the understanding; the liver for feeling; the breast for courage; the nostrils for contempt (cf. *μυκτῆρες*, etc.); the stomach and the bile for anger. Similarly in Latin the nostrils are used for taste and refinement; the nose for satire; the eyebrow for sorrow or disdain; the stomach for anger; the throat for gluttony. The Lithuanians use the same word for soul, heart, and stomach, and the same is probably true of many nations. Many of these metaphors have been transferred to English, and we also use the

blood for passion (hot or young blood), the phlegm for dullness, the spleen for envy; we say that a person has sanguine hopes; we talk of a melancholy man, which means properly a man whose bile is black; a man has a nervous style, or is nervous in the hour of trial; and we say of a bitter-minded critic that he has too much gall."

We speak of "fierce anger," and even use *fierce* in the sense of "violent, angry, wrathful." The derivation of the word is through Old French *fers*; from Latin *ferus* (wild, savage), cognate with *fera* (wild beast). Other phrases in use are "wild with rage," "savage resentment," etc. Here belong the comparisons: "Mad as a hornet," "angry as a bull," "cross as a bear," etc.

The same writer also says: "In Hebrew the heart, the liver, and the kidneys are used for the mind and understanding; the bowels mean mercy, like the Greek *σπλάγχνα*; 'the flesh' means lust; the loins strength; the nose is used for anger, so that 'long of nose' means patient, and 'short of nose' irritable; a 'man of lips' is a babbler (Job xi:2; the neck is the symbol of obstinacy; the head of superiority; thirst or paleness the picturesque representation of fear" (6, p. 196-7).

Shakespeare, in *1 Hen. VI*, iv, i, 141, makes the king say:

"How will their grudging *stomachs* be provoked
To wilful disobedience, and rebel?"

and in *Antony and Cleopatra*, iii, iv, 12, Octavia bids Antony:

"Believe not all, or if you must believe,
Stomach [i. e., resent] not all."

and in Elizabethan literature the word *stomach* had, as had *stomachus* in Classical Latin, the meanings "pride, courage, indignation, anger, resentment, ill-will." Hooker, in his *Ecclesiastical Polity*, says of Arius, that he "became through envy and *stomach* prone unto contradiction." The verb to *stomach* corresponds to French *s'estomaquer*, Latin *stomachari*.

Dr. Holder says: "The ancients made the spleen the seat of melancholy and other ills. Those people living in the malarial belt of the great Mississippi valley, with whom most of my life has been passed, charge to the liver all the ills from which flesh or mind may suffer, while the Indian declares me *spōr' kow-eké*, 'my stomach is bad,' and is truly nearer the right" (10).

In Greek *καρδία*, signifies "stomach," as well as "heart," just as *cœur* does in Modern French.

In the Kootenay Indian language of British Columbia the word for "angry" is *sānitlwinē*, which signifies literally "bad-hearted he-is," from *sānē* (is bad), *itlwi* (heart, mind) — the opposite is *sūkitlwinē*, "well-disposed, glad, happy," from *sūkinē* (is good), and *itlwi* (heart, mind). In analogous fashion are formed *sānitlqōnē*, "sick," literally "bad-bodied he-is," and *sūkitlqōnē*, "well, healthy," literally "good-bodied he-is" (4, p. 394).

According to Park, the African explorer, in the Mandingo, a language of Western Africa, the words "anger" and "angry" are expressed by *jusu bota*, literally "the heart (*jusu*) comes out" (12).

Of the Western Déné Indians of British Columbia, Father Morice remarks: "A single sentence, or periphrastic locution is all that the Carrier has at his disposal to give utterance to such varied movements as sorrow, melancholy, repentance, morosity, displeasure, etc. When moved by any of these, or cognate sentiments, he will never say but: *stāi ndāeta*, 'my heart is sick.' The expression *utzi-sāetsi*, literally "his heart is acrid," signifies "he is acrimoniously disposed" (11, p. 207).

The primitive Aryans seem to have located in the heart and the viscera the seat of the life of man, the soul, and the emotions, and the languages of their descendants bear many traces of these ideas. We find in Latin: *cordatus* (wise, prudent), *vecors* (senseless, mad, insane), *recordari* (to recollect, call to mind), *credo* ("I believe"—from *crd*-+*dh*), etc.; in German: *herzhaft* (dear, beloved), *herzlich* (cordial), *herzlos* (heartless, faint-hearted), etc.; in English: *heartly*, *heart-broken*, *dishearten*, *heart-rending*, *heart-whole*, *heart-felt*, *heartless*, *black-hearted*, etc.; in French: *sans cœur* (heartless), *au cœur dur* (hard-hearted, heartless), *de bon cœur* (heartily), *avoir le cœur fendu* (to be broken hearted), etc.

In Latin and Greek the *liver* (*jecur*, ἥπαρ) "was represented as the seat of the passions, especially of anger and love" (7, p. 265).

Of the Twaka Indians on the head-waters of the Princeapula river in the Mosquito Territory, Central America — although their neighbors the Mosquitos base their special vocabulary upon the word for heart (*kupia*), just as we do — Dr. D. G. Brinton tells us: "The Twaka Indians locate the seat of man's life and emotions, not in the heart, as most nations, but in the liver; and they have in common use such expressions as:

issing sawram, liver-split = angry;
issing pini, liver-white = kind;
issing santi, liver-black = unkind."

With these rude savages "kind" means "white-livered" and the gap between them and the cultured Englishman of to-day is somewhat lessened when we remember that in our own adjective *white-livered* (cowardly) we have preserved a memorial of that far-off past, in which the mind of primitive man failed to distinguish between "kind" and "cowardly." The English *white-livered* and the Twaka *issing-pini* lead us back in the history of mankind to a time when kindness to a foe was held to be cowardice.

Of the Térraba or Tiribi Indians of Costa Rica, Bishop Thiel is quoted by Dr. Gatschet as saying: "Many of the sensations and mental processes which we attribute to the heart are attributed by the Costaricans to the *liver*, *guo*, and hence such words as *to think*, *remember*, *forget*, *desire*, *sad*, *joyful* are compounded with the syllable *guo*" (8, p. 217).

In sixteenth-century English (the *Satires* of Bishop Hall) we meet with the expression "*liver-sick* of love" (sick at heart).

The Greek θυμός (spirit, courage, passion, anger, rage, wrath; soul, heart, life) is derived from θύω (I rush or dart along, storm, rage), and from the same root comes θύάσ, "a mad or inspired woman, a Bacchante." The word is cognate with Latin *fūmus* (smoke), and in English we still speak of "*fuming* with anger," "to get into a *fume*." Sterne even uses *fume* in the sense of "a passionate person." We also use the expression "*storm* of passion," "to *storm*," "a hurricane of wrath," etc. ὀργή (impulse, passion, anger, wrath, violent emotion), together with the verb ὀργάω (I swell with lust, am excited, passionate—the word is also used of fruit in the sense "to swell as it ripens," of soil, "to swell with produce," etc.), is derived from the root ὀργ (to swell). We also say "*swollen* with anger."

In the language of the Samoan Islands *huhu*, the word for "anger, rage" signifies literally "*swell, swell* (*hu*=swell), as we say "*swelling, bursting* with passion" (12a).

Μηνις (wrath, anger, malice) comes from the root *men* (to be excited in thought, to be inspired, raving, wrathful, etc.), whence also μένος (might, strength, spirit, courage), μανία (madness), μάντις (a diviner, a seer) — at the basis of all these lies the idea of "mental

excitement." As Latin *mens* (mind) is cognate, all derive ultimately from the Indo-European radical *man*, "to think."

In the language of the Pacific island of Tahiti "*riri*," anger, literally means "*he shouts*" (13, p. 89).

In the Stikkeen dialect of the Tlingit language of Alaska we meet with the following expressions: *K'ant-wa nuk*, "angry," *K'anraō*, 'cross,' *K'ān-qa-gaō*, "I am angry." Here *wa nuk* and *raō* are verbal suffixes, between which, as in the last word *K'ān-qa-gaō*, and the radical *k'an* (angry) the pronoun is inserted. *K'ān* (angry) bears a suspicious resemblance to *K'ān* (fire) (2, p. 65). To this category belongs our "incensed." We speak also, as do other peoples, of "*kindling wrath*," and "*smouldering anger*."

A most interesting word in Greek is *νέμεσις* (righteous indignation, anger, wrath, resentment), personified in *Némeis*, the goddess of divine wrath and just retribution, cognate with *νέμωσις* (a distribution), from the verb *νέμω* (I distribute, possess, etc.), all from the Indo-European radical *nem* (to pasture, to number, to allot).

In his dictionary of the Niskwalli language of Washington, Dr. George Gibbs gives the following interesting etymologies: "*O-het-sil*, 'to be angry,' *o-hēt-sil-chid-hwul-dug-we*, 'I am angry with you.' from *o-hēt*, 'why, what is the matter?' and *si-lus*, 'the forehead.' Derivatives are *ōd-het-sil-us*, 'to sulk, to blush,' *o-he-ha-hēt-sil*, 'to pretend to be angry.' The radical *o-het-sil* signifies also "to be ashamed," *o-het-sil*, "to be angry," being distinguished from *o-het-sil*, "to be ashamed," only by intonation (9, pp. 309, 310, 348, 296, 351).

If we arrange the words for "anger" discussed above according to the ideas upon which they are based, we have the following:

1. Physical idea of "choking, strangling." English *anger* and its cognates.
2. Physical idea of "writhing, twisting." English *wrath*.
3. Idea of "crookedness, curling." English *cross* and its cognates.
4. Idea of "bursting, tearing asunder." German *Zorn* and its cognates.
5. Idea of "hasty movement." English *fury* and its cognates. Greek *θυμός*.
6. Idea of "seizing upon, grasping after." English *rage* and its cognates.
7. Idea of "making a noise, yelling." German *Grimm* and its cognates. Tahitian *riri*.
8. Idea of "malicious talk, slander." German *böse*.
9. Idea of "mental excitement, excitation." Latin *vates* and its cognates. Greek *μανία*.
10. Idea of "swelling." Greek *ὄργη*. Samoan *huhu*.
11. Based upon the "heart." Kootenay *sānitlwinē*. Déné *stzi ndeta*. Mandingo *jusu bota*.
12. Based upon the "liver." Mosquito *issing sawram*. Terraba.
13. Based upon the "gall, bile," "spleen," "gland." English *choler*, *bile*, *gall*, *spleen*, and cognates. Latin *fel* and cognates. Nipissing *nickatisiwin*.
14. Based upon the "stomach." Latin *stomachus* and cognates.
15. Based upon the "nose." Hebrew.
16. Based upon the "forehead." Niskwalli *o-het-sil*.
17. Based upon the idea of "indignation at what is unworthy." Latin *indignatio*. Greek *Némeis*.

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A LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

BY EDMUND C. SANFORD.

(*Fifth Paper.*)

THE VISUAL PERCEPTION OF SPACE.

It is upon this field more than any other in physiological psychology, unless it be that of the psycho-physic law, that discussion has been most protracted and the accumulation of evidence most imposing. A complete treatment of the question involves arguments from surgery, pathology and other sources quite outside the possibilities of the laboratory. And even then, it is difficult, if not impossible, to establish one theory surely as against all others. Apart from the question of original sensations, however, there is a certain degree of harmony among investigators, and it is the commonly accepted experimental facts that this section of the Laboratory Course aims to gather up. The discussion of the ultimate matters may be followed in the works of Helmholtz, Hering, Stumpf, James, Wundt, and others. For the facts of spatial vision in general, see the works of Helmholtz, Hering, Aubert, Wundt, James and Le Conte. For special facts, special references will be given below. The subject is also more or less fully treated in the standard physiologies, Bernstein's *Five Senses*, McKendrick and Snodgrass's *Physiology of the Senses*, and other books of the same kind.

The ordinary seeing of space in its various aspects of distance, direction and size, rests on the retinal and kinæsthetic sensations of both eyes. And in every normal act of vision all of these sensations are either present themselves or by their reproduced images, and this fact must not be forgotten. For the sake of simplicity, however, it is necessary to separate the factors more or less completely, and to treat now of one and now of another. Those phenomena of which the presence of two eyes is an essential condition form a class by themselves, and will be reserved for a special section; the present one will be given to a portion of the facts of monocular vision, or rather to some cases in which the presence of a second eye is unimportant.

MONOCULAR PERCEPTION OF SPACE.

156. The Outward Reference of Visual Perceptions. The outward-reference of visual perceptions probably comes about through their co-ordination with those of other senses, especially those of tactual and kinæsthetic origin, but the matter is too complex for direct experiment. It is easy, however, to study the relations of the retinal image and the outer objects that produce it, and much has been written on the outward projection of retinal states. It

must, however, be kept clearly in mind that retinal states as such, are never perceived, and especially that retinal sensations are not first given a location in the eye, and then at some later stage transferred outward. Considered physically, the retinal image is reversed with reference to the objects that it represents. This has already been seen for the rabbit's eye in Ex. 99, and for the human eye with Purkinje's vessel figures and the phosphenes (Exs. 107 and 113). It can be shown also in the following experiment with retinal shadows :



Le Cat's experiment. Hold a pin, head upward, as close as possible before the pupil, and an inch or two in front of the pin, a card pierced with a pin-hole. Move the pin about till it comes into exact line with the hole, when there will be seen in the circle of diffusion representing the hole a shadowy inverted image of the pin-head, somewhat as appears in the accompanying cut. The rays of light from the pin-hole are too divergent to be brought to a focus on the retina, but enter the eye in a favorable state for casting a shadow. The shadow on the retina is erect like the pin that casts it, but is perceived inverted in its outward location. Observe at the same time the still more blurred, erect image of the pin through which the other things are seen. This is not a shadow but a true retinal image formed in the ordinary way by light reflected from the surface of the pin. When several pin-holes are used (three at the points of an eighth of an inch triangle, for example) an equal number of shadows will be seen.

The casting of the shadow can easily be illustrated with a candle, a double convex lens and a bit of card. Set the lens a foot or two from the candle and hold the card too near to the lens for the formation of an image, then introduce a finger or pencil close before the lens on the side toward the light and observe the erect shadow on the card.

Le Conte, *B*; Wallenberg, Laqueur.¹

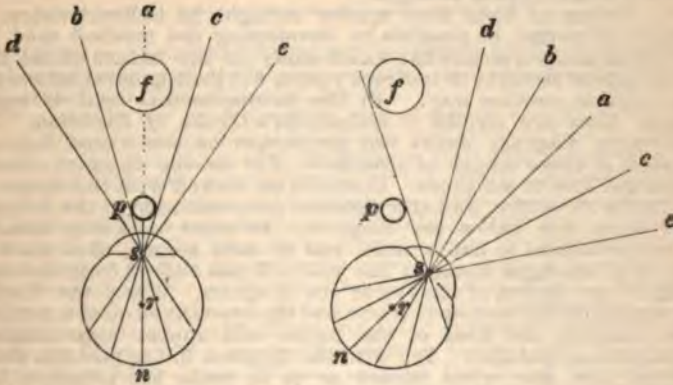
157. **Monocular perception of direction ; directions from the eye.** The perception of direction is ordinarily binocular, and the centre to which directions are related lies between the eyes, even when one is closed. This will be proved in the experiments on binocular perception of space. The binocular perception, however, must rest on a perception of the relative direction of points in the monocular field, and this will be considered in the next few experiments.

Two luminous points appear to have the same direction when one is exactly covered by the other, or, to state the matter in retinal terms, when the image of the one for which the eye is accommodated lies in the centre of the circle of diffusion of the one for which the eye is not accommodated, or, if both appear in diffusion circles, when the centres of these circles coincide. The lines

¹See bibliography at the close of the article.

drawn through points in this relation and prolonged to the retina are known as *sighting lines* (*Visirlinien*, *Lignes de visée*), and cross in the centre of the pupil, or rather, in the centre of the image of the pupil formed by the cornea, about 0.6 mm. forward of the true position of the pupil and 3 mm. from the summit of the cornea. These lines might well be called "lines of direction," had not this name been already given to another set of lines, namely those which are drawn from the points of external objects to the corresponding points of their retinal images. These have already been mentioned in Exs. 101 and 112, and they give with certain limitations the directions in which objects appear when the eye is exactly accommodated for them. Their point of intersection is about 7 mm. from the summit of the cornea. They are important for optical purposes, but for the general perception of direction are less important than the sighting lines, though for remote points and for points near the fixation point, the differences between the two sets of lines is very slight. For points remote from the fixation point, for reasons to be given in a later experiment, neither set of lines gives the direction in which objects are seen.

The position of the crossing point of sighting lines is found by inference from the optical structure of the eye. To make a sure empirical determination would be laborious, but it is easy, however, to show that the crossing point is considerably in front of the centre of rotation of the eye (about 10.6 mm.). Place a candle at a distance of a foot or foot and a-half from the eye. Look toward the flame with a single eye, but hold close before the eye a pencil or narrow strip of black cardboard. So long as the eye looks straight forward, the flame is entirely hidden by the pencil. When, however, the eye is turned strongly to either side, the flame instantly appears on the side toward which the eye has been turned. The explanation of this will readily be seen from the following diagrams in which *p* represents the pencil, *f* the flame,



s the centre of sighting lines, and *r* the centre of rotation. The lines radiating from *s* are sighting lines, *sa* being the principle one, which is practically coincident with the line of sight.

Helmholtz, *A*, F, 602 (539), 745 ff. (584 ff.); Aubert, *A*, 461 (on the sighting lines).

158. Monocular perception of direction; directions in the field of vision. The relative direction of points in the field of vision

cannot be changed, of course, without changing the direct points from the eye; it is easier, however, to experiment in the field.

a. Lines that appear straight in indirect vision. Lay a sheet of paper on the table and mark a fixation point in the center of it. Two or three inches to one side of the fixation point place a button or bit of black paper, and, a foot above and below, two more buttons or bits of paper. Then leaning over the table so that the eye is above the fixation point try to place the buttons in a straight line, holding the eye steadily upon the fixation mark. Examination of the buttons when placed will show that the middle one is too near the fixation mark, i. e., the attempt to make a straight line has resulted in a curve convex toward the fixation mark. Try also with the buttons in a horizontal line.

b. Actual straight lines in the periphery of the field. Lines that are actually straight should appear concave. On a large sheet of paper draw a pair of parallel lines three or four inches apart and three feet long. Place a fixation point half way of their length on the table, and observe as in a above. Try with the lines in vertical and horizontal and in oblique positions. The same thing, at least, may be noticed in the double images of a vertical thread seen binocularly.

In a spherical field of vision, the parallel lines of experience would be represented by great circles, the horizontal planes, for example, having their poles at the right and left ends of the horizontal axis of the field, their planes making equal angles above and below the plane of the horizon.¹

It is obvious that changes in direction which make straight lines appear curved cannot take place without introducing slight changes of distance also. The shortest distances for perception are the curves which appear straight.

c. Nature of lines that appear straight in indirect vision. It would, of course, be possible by developing the method used above to make a somewhat exact study of the nature of the lines that appear straight in indirect vision, but their general nature can be found in another way. In the hemispherical field of vision these lines are circles — Helmholtz's *Circles of Direction*. The following diagram shows the projection on the visual field of a system of these circles of direction. For use the diagram is enlarged five or six times. It should be viewed with the single eye opposite its centre, and at a distance proportionate to the length of the short line below the diagram. In order to fix this distance it is convenient to cut a small rod of such length, that when the eye is at the right distance the rod will just extend from the edge of the socket of the eye to the diagram. When the rod is brought into the proper position and the eye is fixed on the middle of the diagram, the lines of the figure will appear approximately straight and parallel. Try with the diagram in the position shown below, and also when turned so as to make the movement oblique. Especial care should be taken to avoid movements of the eyes, for a new interpretation of the curves is thus introduced. The checker-board seems concave instead of plane. Some

¹It should not be supposed from this that the native field of vision is hemispherical. It is neither definitely hemispherical nor definitely anything else, except as it is formed by the conditions and habits of vision. It is spoken of as a plane hemisphere for greater ease in exposition.

experiences of movement exercise a controlling influence on its perceptions. (For a fuller account of Listing's Law and the circles of direction, see Ex. 123, and the Appendix at the end of this section.)

d. Illusions of form in indirect vision. Radial distances, as the diagram of *c* shows, are more decidedly under-estimated than distances parallel to the margin of the field. This is easily shown by laying a large sheet of paper on the floor—a strip three feet long and a foot wide answers well, when the narrow side is next the observer—and standing so that when the eyes are directed horizontally forward, the paper will be seen at the lower edge of the field. When the eyes are turned from the horizon to the paper, the latter noticeably increases in width (*i. e.*, in a direction to and from the observer) when the eyes are again directed to the horizon, it suffers a corresponding contraction. Changes in the other direction are hardly noticeable. Disks of paper (three to six inches in diameter) when viewed in indirect vision appear as ellipses with their short axes directed toward the fixation point. For the part of the field near the fixation point such illusions as this and those of *a*, *b* and *c* are so slight that they may be neglected.

The whole field of vision itself appears narrower than it really is; it actually covers an extent of nearly 180° , and yet under favorable circumstances, as when looking at the dark field of the closed eyes, or at the sky in the absence of all landmarks, the extent may seem not much over 90° .

Helmholtz, *A. F.* 706-718, (551-561). Wundt, *A.* 3te Aufl., II, 112-114; 4te Aufl., II, 128-130. Hering, *A.* 309 ff., 535 ff.

159. Monocular perception of direction; directions in the field of regard. The observation that the perceptions of the eye at rest are modified by those of the eye in motion, is still further confirmed by the similarity of other phenomena of the field of regard and the field of vision.

a. Straight lines viewed with the eyes in secondary positions. Experiment with a single eye and a long ruler held horizontally before an even wall space or other uniform background. Hold the flat side of the ruler toward the face and about a foot distant from it. Try first with the ruler eight or ten inches above the primary position of the line of sight (*cf.* Ex. 123), running the eye freely back and forth along the edge, and observe that the edge appears curved upward, *i. e.*, concave below. Try with the ruler depressed a somewhat greater distance below the primary position and observe the contrary curvature. Try also with the ruler vertical and to the right and left. Little advantage will result from too extreme positions of the ruler. The curvature to be observed is not very great, but that it is due to the visual apparatus and not to the ruler, is easy to show by turning the ruler over, which would reverse the direction of curvature in the ruler, but not that of the curvature which depends on the eye. Change of position of the ruler from above to below the primary position of the eye, on the contrary, reverses the direction of the curvature due to the eye, but not a real curvature of the ruler. Compare the results here found with those in Ex. 158, *a* and *b*.

The occasion of the illusion is the rotation of the eyes when moved from point to point in secondary positions. (*Cf.* Ex. 123, and the Appendix at the end of this section.) When the eye is kept fixed on the end of the ruler, or moved slowly, the ruler may seem slightly tilted instead of curved.

b. The apparent vertical. For the single eye a true vertical appears to incline a very little inward, *i. e.*, to the left for the right

eye and to the right for the left eye. Place on the field of the cam-pimeter a large sheet of paper, and on it draw an exactly horizontal line at about the height of the eye when the observer is in position. Exactly above the point of this line to which the line of sight is naturally directed, set a tack and hang from it a black thread 60 cm. long with a weight at its lower end. At a distance of 57.3 cm. from the tack, draw a second horizontal line and paste along below it a bit of millimeter paper. Let the observer take his position and carefully push the weight to one side or the other (as may easily be done with a needle mounted in a short handle of wood) until the thread seems to be exactly vertical, and the angles that it makes with the horizontal line exactly right angles; then, let him hold the thread in place by sticking the needle into the wood and note the amount of the angle from the millimeter scale—on which, if the dimensions above have been observed, 5 mm. will correspond to 1° . In this experiment the eye may be moved up and down the thread as desired.

Repeat the experiment, keeping the eye fixed in the primary position. The amount of the inclination necessary has been found by Donders to be decidedly variable even in the same observer. The attachment of the eye muscles is such that with elevation of the lines of sight there is a slight turning outward, and with depression a slight turning inward. A line which the eye follows exactly in this upward and downward movement, *i. e.*, a line inclined a little outward seems vertical.

On *a* see Helmholtz, *A*, F, 699 (545); Hering, *A*, 536. On *b* see Helmholtz, *A*, F, 700 ff. (546 ff.); 716 f. (559 f.); Wundt, *A*, 3te Aufl., II, 122 ff.; 4te Aufl., II, 140 ff.

160. The tendency of the eye to follow lines and especially contours. This tendency is of importance in the seeing of form, because it results in clear vision of the object and because it complicates the whole matter by introducing kinæsthetic factors. It is a habit, however, that is not beyond conscious control, and for that reason is more difficult to demonstrate by overt experimentation than by casual observation. Any one that will take note of his own seeing when presented with objects with strongly marked lines, will easily find trace of the habit. In imagining geometrical figures, also (for example, an eighteen-inch hexagon drawn on the blackboard) something of the same tendency will often be found. The following experiment aims to give a laboratory means for making such observations.

Paste upon a piece of cardboard eight and one-eighth inches long and four inches wide, two four-inch squares of red paper in such a way as to cover all the card, except a white stripe one-eighth of an inch wide between them; cover the whole with a sheet of semi-transparent paper as for Meyer's experiment (Ex. 142 c). Examine the white stripe for the effects of contrast. After the examination has lasted a few seconds, suddenly lay across the middle of the diagram a bit of wire six or eight inches long, approximately at right angles to the white stripe. If the experiment succeeds, the white stripe will instantly show a strong increase in the complementary color. Before the introduction of the wire, the eye is chiefly engaged in following up and down the white stripe, and the contrast effects are confined to those of simultaneous contrast. When the wire appears, the eye changes to it and moves back and forth along it once or twice, and thus brings upon the white stripe the more powerful effects of successive contrast.¹

¹This experiment originates with Waller (Journal of Physiology, XII, 4, p. xlv), but is used by him for a totally different purpose.

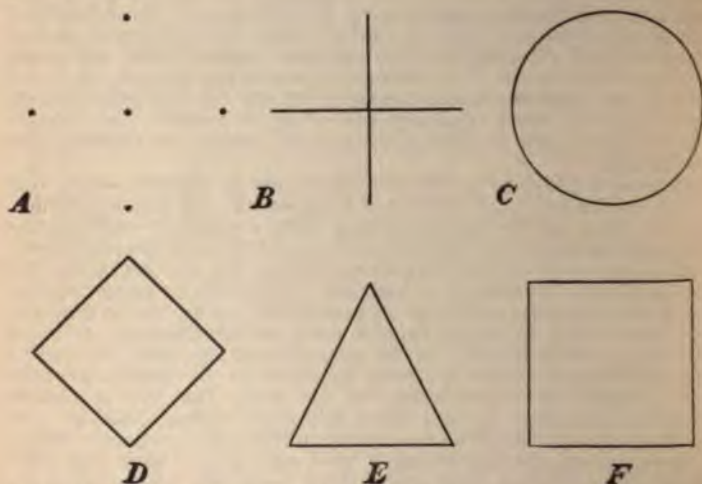
GEOMETRICAL ILLUSIONS.

Besides the illusions just considered, there are a large number of others that show in greater or less degree the influence of eye motions in the formation of visual perceptions. It is very likely that many of them — even those that appear simple — are the resultant of many other experiences than those of ocular motion, but in most of those that are to be given here, eye motions are certainly an important factor. They fall into classes according to certain empirical principles and have been so grouped below, but merely for convenience and without any intention of prejudicing their explanation. Typical examples only are given, for the number of variants of some of them is very large.

In all of them the student will do well to turn the diagrams about and to view them from different sides so as to separate the illusions that depend on position from those that are independent of it. In general, illusions are strengthened when the affected lines are made oblique in the field, corresponding with the less frequency and certainty of eye movements in such directions. For the most careful study of these illusions they should be separated from one another, and from the influence of all extraneous lines, *e. g.*, drawn singly on good sized sheets of paper.

161. Illusions in the perception of distances depending on their direction in the field of vision.

Vertical distances are over-estimated in comparison with horizontal distances. Lay off by eye on a sheet of paper placed vertically before the face equal distances, up, down, right and left, from a central dot, marking the distances by dots as in *A* in the accompanying diagram. Repeat several times and measure the distances found. In the diagram all the vertical and horizontal distances are equal, and in all cases, except in the circle, the vertical seems too long.



In *D* the over-estimation of the height entails an under-estimation of the angles above and below, and an over-estimation of those at the right and the left. The illusion is less in the line

figures and absent entirely in the circle, in Wundt's opinion (4te Aufl. II, 152), because in the case of these familiar figures, perception is influenced by knowledge of the geometrical relations of the parts. (For quantitative studies see among others, Kundt, and Münsterberg, 164 f., 175 ff.) A similar, though slight, difference is often found between horizontal distances to the right and left, when careful experiments are made with the single eye. (For quantitative measurements under various conditions, see Kundt, and Münsterberg, 160 ff.)

Distances in the upper part of the field are over-estimated as compared with those below them. This illusion may be tested actively as follows: Near the top of a strip of paper twelve or fifteen inches long and five or six wide, draw a horizontal line two inches long. Take this again as a standard and draw half an inch below it a second line of a length that seems equal to the first. Then cover the first line and taking the second as a standard, draw a third and so on, continuing this process till the strip is full. Then uncover and measure the first line and the last. With this Wundt associates the S's and 8's which seem a little smaller at the top than at the bottom when in their usual position, but a good deal larger above when inverted: ss.

For all of these illusions Wundt finds an explanation in the differences of effort required for turning the eye in different directions (3te Aufl., II, 119 ff.; 4te Aufl., II, 137 ff.). The superior and inferior recti are relatively weaker than the external and internal muscles of the eye. Furthermore in elevating or depressing the eyes, the oblique muscles partly oppose the superior and inferior straight muscles, and so render a portion of the effort ineffective for purposes of motion. Why lines in the upper part of the field should seem longer than those lower down, is not specifically stated, but by implication it also is to be credited to muscular differences.

For references see those given at the end of Ex. 167.

162. Illusions of interrupted extent. Interrupted intervals.

A



B



C



D

E

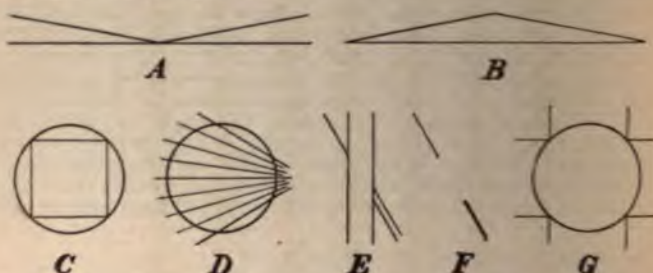
generally seem larger than free intervals. In the first two figures above, the interrupted extents seem larger than the free extents. In the last case, however, where the interrupted space has but a single dot in the middle, the principle suffers an exception. The

figure showing the filled angles should be regarded binocularly; monocular observation complicates the effect by introducing the illusion of Ex. 159, b. *C* and *D* are equal squares.

Here as before Wundt's explanation rests on variation in eye movements (3te Aufl., II, 126 f.; 4te Aufl., II, 144 f.). In passing over interrupted extents, the movement of the eye is made more difficult by the short stages into which its course tends to be broken, while it sweeps with relative freedom over the empty spaces. The fact that a single interruption in the middle of a space has an opposite effect, is explained by a tendency of the eye, when the middle of an extent is marked, to take in the extent simultaneously by fixation of the middle without motion. For other explanations see Helmholtz, *A*, F, 720 f. (563 f.).

For references see those given at the end of Ex. 167.

163. Illusions affecting the apparent size of angles. Small angles are relatively over-estimated. The figure with filled angles in Ex. 162 above might be classed under this principle as well as under that of interrupted extent, but in other figures the effect of small angles is seen by itself.



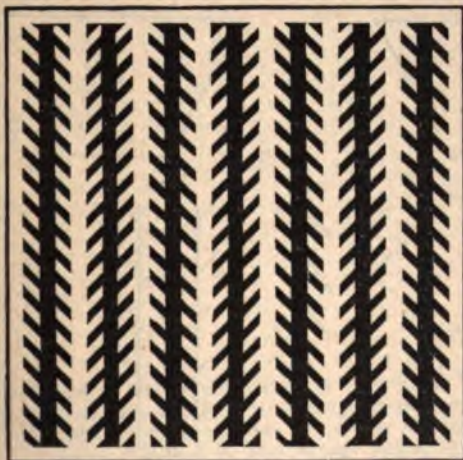
In *A* and *B* slight distortions are found in the horizontal lines. In *C* the circle is flattened at the corners of the square, and the sides of the latter are bent inward. In *D*, also, the distorting effects of the cross lines are unmistakable. In *E* the oblique line on the left is the real continuation of the lower line at the right, not of the upper as appears to be the case. This illusion is strengthened by viewing it from a distance, i. e., by reducing the size of its retinal image. In *F* it is shown that the presence of an actual oblong is not essential to the illusion, though here, as elsewhere, the place of actual lines may be supplied more or less consciously by imaginary ones, or by the lines of other figures, the edges of the page or any other prominent lines in the field. In *G*, however, the explanation by the over-estimation of small angles completely fails, for the effect is the same in kind as in *C*, when the reverse was to be expected.

The over-estimation of acute angles Wundt refers also to eye movements (4te Aufl., II, 146). In figure *A* above, for example, as the eye follows the horizontal line toward its intersection with the oblique lines, it suffers an increasing attraction, as it were, toward the oblique line nearest it, and from this results the wrong conception of its route. The fifth figure according to Wundt involves several illusions. The figure is estimated too large in the direction of its prominent (vertical) lines, and in this the habitual over-estimation of verticals helps. When the latter is

excluded by turning the figure on its side, the illusion that remains is to be explained partly by the over-estimation in the direction of the prominent lines and partly by over-estimation of the small angles. In explanation of the first three figures, Helmholtz again cites his principle "according to which acute angles, being small magnitudes clearly limited, seem in general relatively too large, when we compare them with undivided obtuse or right angles," but believes that this principle yields in importance to ocular movements, if indeed, it does not itself depend on such movements (*F*, 724, 725, 726, 727, (566, 567, 568)). Cf. illusions depending on motions of the eyes, Ex. 168. In the fifth figure he thinks movement is less important, and that irradiation may co-operate (*F*, 723, (565)). The over-estimation of small angles is rejected as a principle of explanation by Lipps.

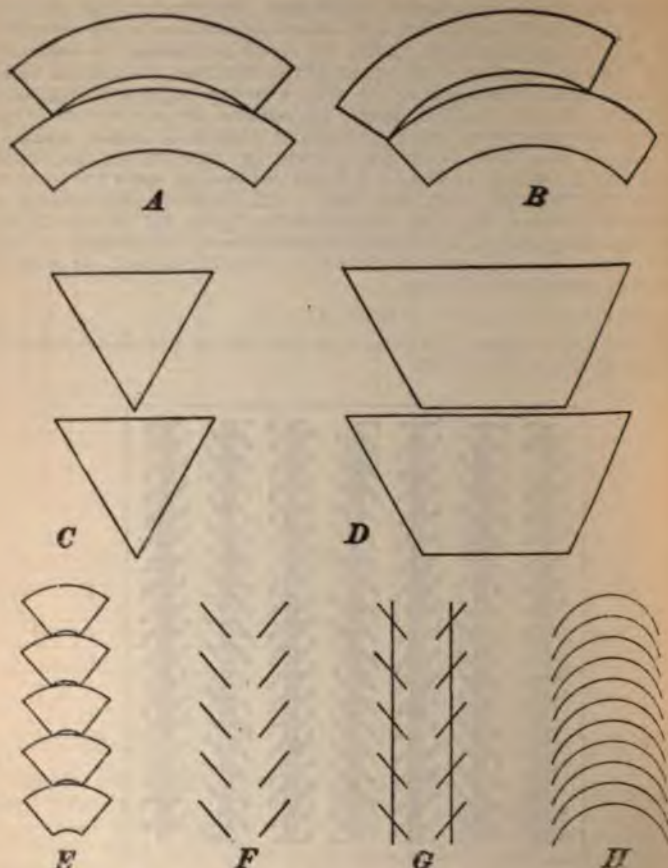
For references see those given at the end of Ex. 167.

164. Zöllner's figure. In principle this much discussed figure is the same as those of Ex. 163.



The oblique cross lines, making small angles with the verticals, cause an apparent dislocation of them in the same direction that they would be dislocated if the acute angle were enlarged. The greater effect in this figure would then be due to the multiplication of the small angles. For a partially concurrent explanation see Ex. 168; for divergent explanations see among others, Hering, Kundt, Lipps. The strength of the illusion depends on the angle of the oblique lines. For himself Zöllner found it greatest at about 30° . The short cross lines themselves show the illusion of *E* in Ex. 163, the right and left portions seeming not to be continuous.

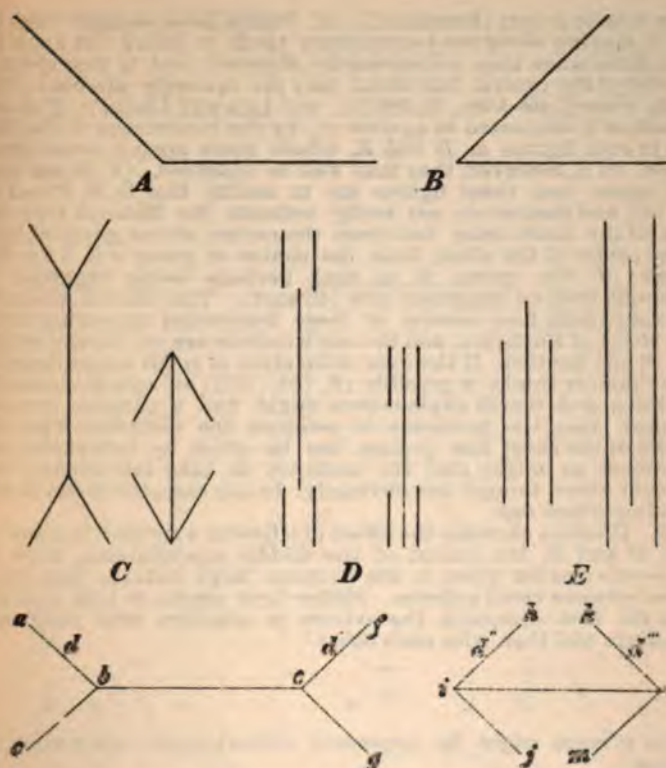
An interesting case in which Zöllner's figure is present, though masked, is that of the superposed segments of a ring. In *A* and *B* below, the upper one, in each case, seems smaller, though all are of precisely the same size. Müller-Lyer recognizes this as a case of the Zöllner's figure and suggests a transition to it by such figures as *E*, *F* and *G*. Wundt denies this explanation (4te Aufl., II, 151-152), asserting that if this were the case, the effect ought to



be the reverse, i. e., the upper segment should seem larger, and gives *H* in which the upper curves seem a little larger in proof, but he evidently has only considered the curves and not the straight line at the ends of the segments. His own explanation which traces the illusion to association with cases in which the segments are referred to the same centre is clearly imperfect, for it does not fit the cases of the triangles and trapezoids, *C* and *D* above, where the illusion though weakened is still present. The fact may be that the ring segments involve co-operating illusions depending on both principles, while the straight line figures involve only one. The illusion is even more striking when the segments are cut out of cardboard, and can be shifted about and actually superposed.

For references see those given at the end of Ex. 167.

165. Illusions affecting the length of lines in the presence of other lines and angles. In *A* and *B* the sides of the larger angle seem



longer than those of the smaller angle. Try with the diagram in different positions to avoid the tendency to over-estimate vertical distances noticed in Ex. 161 above. In *C* the central vertical lines are of equal length, but that in the longer figure appears distinctly longer. In *D* and *E* the same is true, though the illusion is somewhat weakened.

A number of explanations for these illusions have been advanced, no one of which appears so exclusively right as to exclude the rest. (1) It has been held that figure *C* is only a development of *A* and *B* and that the angles modify the apparent length of their enclosing lines (Müller-Lyer, Jastrow). (2) It has been suggested that we unconsciously take into account the spaces adjacent to the lines to be compared; *e. g.*, the trapezoidal spaces *abef* and *hikl* inclosed by the outwardly and inwardly inclined arms in the typical figure (Müller-Lyer and Auerbach). (3) Another writer considers that the tendency to over-estimate small angles and under-estimate large ones causes an illusory rotation of the short arms about their middle points, and thus a lengthening of the central line in one case and a shortening in the other. If, for example, *ab* and *ef*, were rotated in the sense required by this illusion about *d* and *d'*, their ends *b* and *e* would be brought farther apart. Similarly, but with opposite effect, if *gh* and *kl* were rotated about

their middle points (Brentano). (4) Others have thought that the eye in moving along the central lines tends to follow out upon the short lines when they are outwardly directed, and to stop short of the end of the central line when they are inwardly directed (Delbœuf, Wundt, 4te Aufl., II, 149 f.), and in a way Lipps.¹ This explanation is supported as against (3) by the persistence of the illusion in such figures as *D* and *E*, where there are no actual small angles. In *E*, however, they may well be imagined. (5) It has been held again that these figures are in reality like *D*, *E*, *F* and *G*, Ex. 167, and that we do not really estimate the distance from the apex of the short lines, but from the centre of one group of lines to the centre of the other, from the centre of group *a b b c* to the centre of the group *f e e g*, each perhaps being regarded as a triangle with an imaginary side (Brunot). The skillful observer will very likely find several of these tendencies appearing in his own study of the figure, and the explanations are not totally exclusive of one another. If the over-estimation of small angles depends as Helmholtz thinks is possible (F, 730, (571)) on eye-movements, the third and fourth explanations might find a common ground. Perhaps, also, the tendency to estimate the distances from the middle of the short line groups, has its effect by influencing the movement, as might also the tendency to take into account the adjacent areas, though less obviously. In any case, the motor factor is an important one.

166. Illusions showing the effect of adjacent extents.² In cases *A*, *B*, *C*, *D* and *E*, the extent of the middle quadrilateral, angle or line seems smaller when it lies between large extents than when it lies between small extents. Müller-Lyer seems to hold that the observer first compares the extents in question with their surroundings and then with each other.³

$\overset{\cdot}{a}$ $\overset{\cdot}{b}$ $\overset{\cdot}{c}$ $\overset{\cdot}{d}$ $\overset{\cdot}{e}$ $\overset{\cdot}{f}$ $\overset{\cdot}{g}$ $\overset{\cdot}{h}$

This process might be expressed schematically somewhat as follows :

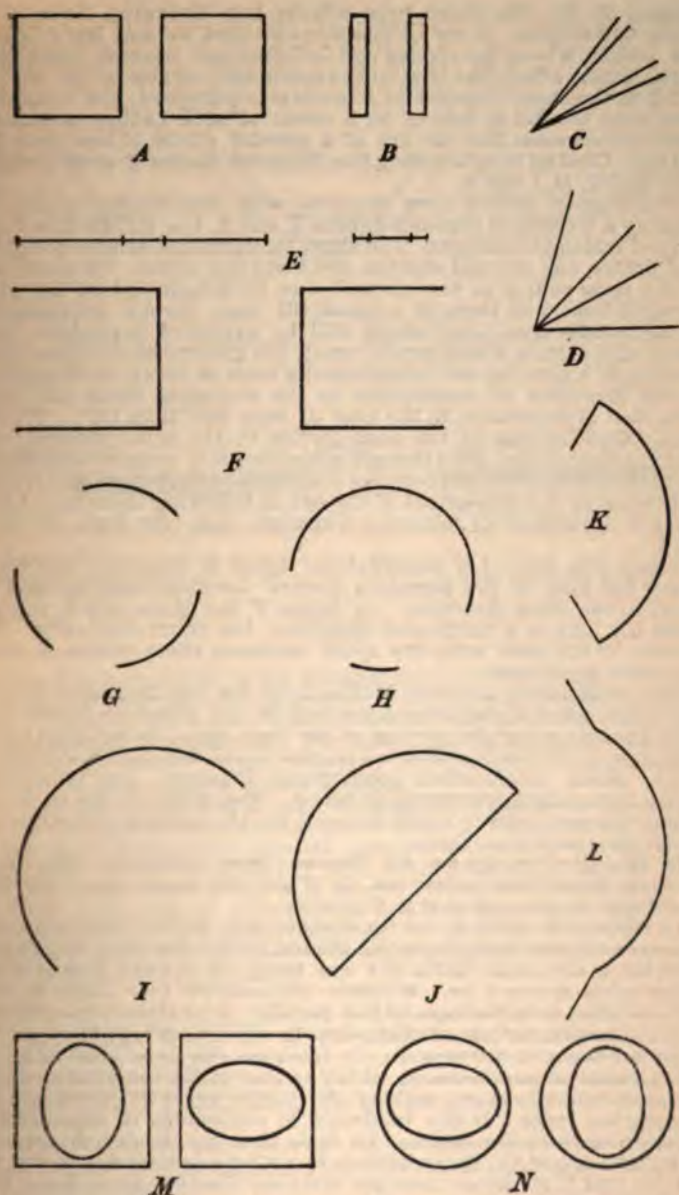
bc	in comparison with	ab	or	cd	is small.
fg	"	"	"	ef	or gh is large.
bc	"	"	"	fg	is small.

In a similar way the sides and bases of parallelograms are said to be mutually affected, and even parallel lines seem nearer together or further apart as they are long or short. In this case, which is the only one noticed by him, Wundt (4te Aufl., II, 146-147) explains the illusion by the strong tendency to move the eyes lengthwise along the long parallels, which results in an under-estimation of the figure in a vertical direction. In figure *M*, the squares are distorted horizontally and vertically by the inscribed ellipses. In *N*, however, the circles are distorted, if at all, in an opposite

¹Lipps in his explanations makes use of many picturesque expressions with reference to these figures and their parts such, as, liveliness, inner activity, upward-striving (*Lebendigkeit, innere Regsamkeit, Emporstrebens*), thus seeming to attribute different effects to "forces" inherent in the figures, and for this he is criticised by Wundt. It seems probable to the writer, however, that if he had undertaken an analysis of the "forces" that he finds, he could hardly have avoided agreement with the eye-movement party. Indeed in his *Grundriss der Psychologie*, p. 327, he makes use of eye-movements to explain illusions of the Zöllner type, though he regards them in the last analysis as psychic and not sensory illusions.

²To these illusions, Müller-Lyer gives the name of Illusion of Confusion (*Confusionstäuschungen*).

³Müller-Lyer, 266-267.



direction. In *M* we have a case of the influence of adjacent lines ; in *N*, however, this effect is overcome by the greater power of the

illusion of Ex. 163, which here affects the diverging lines of the circle and ellipse. It seems possible also that we may have here at the points where the circles and ellipses are nearest together a compressing effect like that in the concentric circles in Ex. 167.

When the circumference of a circle is interrupted, the remaining arcs seem too flat to belong to a circle of such radius; so also, a semi-circle seems like an arc of a greater circle of less than 180° extent. Closing it by drawing the diameter makes it seem smaller. Cf. Figs. *G*, *H*, *I* and *J*.

This illusion Müller-Lyer connects with that noticed in Ex. 165, making a transition through figures *K* and *L*, the middle line being curved instead of straight. The short straight lines tend to lengthen and flatten one arc and shorten and bend the other. If short arcs of the same radius as the central ones be substituted for the short straight lines, and then be rotated till they form a continuation of the middle arcs, they might still be expected to produce their usual effect, from which would result the generalization that every portion of a circular arc influences the form of every other portion, in the direction of contraction in the complete circle and large arcs, and of expansion in the case of arcs less than 180° . Wundt, also, refers the case of the semi-circles to the same general principle as that of Ex. 165 (though his principle is somewhat different from Müller-Lyer's), but believes that the small arcs in *G* and *H* seem flat, because the movement of the eye in following them is not very remote from that for following a straight line (4te Aufl., II, 149 f., 151 f.).

When one side of a quadrilateral figure is removed, the figure seems too long in the direction toward the side removed and too short in the other direction. In figure *F* the three-sided squares seem too long in a horizontal direction, too short vertically. The reverse is the case with the space between them which is also a square of equal size.

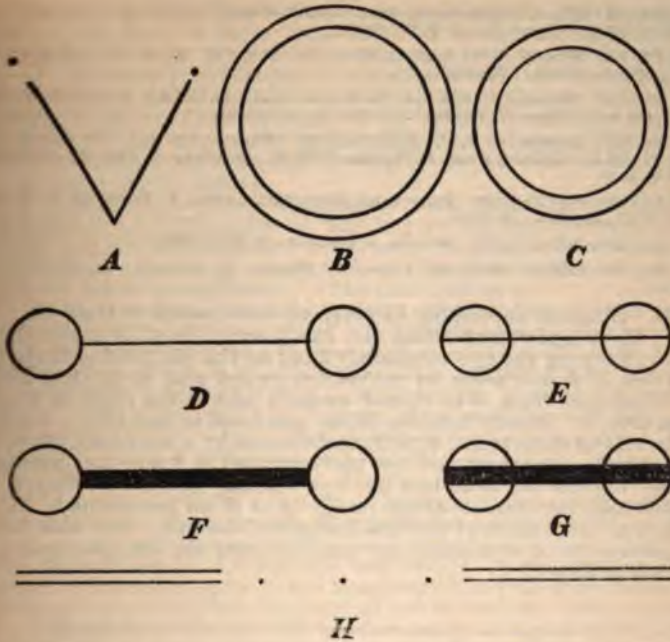
The explanation given by Müller-Lyer for this illusion is double. The open-sided square seems too long in the direction of the open side, because a certain portion of the free space is included in the estimate, and then it seems too narrow because it seems too tall.

167. Some unclassified geometrical illusions. An interesting figure from Łaska is given as *A* below. The sides of the angle are equal, but are made to seem unequal by the setting of dots at unequal distances from them.

In *B* and *C* is shown an illusion from Delbœuf. The inner circle in *B* and the outer one in *C* are the same size, but that in *B* looks larger and that in *C* smaller.

In Delbœuf's opinion, the illusion depends on the interference of the extra circles with the measurement of the diameter by the eye. In *C* the inner circle holds the eye back, as it were, and in *B* the outer circle draws it on. Wundt's explanation (4te Aufl., II, 146-147), is here, as in the case of the parallel lines already mentioned, the under-estimation of distances in directions opposed to the chief tendency to movement. In this case the eyes tend to follow the parallel circumferences which causes under-estimation of the distance between them, making the larger seem too small and the smaller too large. If this tendency to movement is opposed by a fixation mark in the middle, he finds that the illusion disappears, as in the case of the space broken by a single central dot in Ex. 162.

In *D* and *E* is shown another striking illusion, again from Delbœuf. The distance between the adjacent edges of the left hand pair of circles is the same as that between the remote edges of the right hand pair, though the latter looks considerably less. The tendency



apparently is to estimate the distance not between the points mentioned, but between the centres of the circles in each case. In *F* and *G* where the heavy lines interfere with this tendency, the illusion is a little weakened. For these figures, Delbœuf's explanation is like that for *B* and *C* above; in *E* and *G* the eye is restrained, in *D* and *F* it is drawn onward.

In *H* is shown an illusion from Mellinghoff. The three dots in the free space between the parallel pairs seem a little above the level of the lower lines of the parallel, though they are not actually so. The upper lines of the parallel pairs, according to Wundt (4te Aufl., II, 146), attract the eye as it is swept across the figure, toward a position intermediate between the upper and the lower lines, and to this position the dots are assigned.

In the parallel columns below is shown another illusion of common experience with printers :

In these two columns the type is of exactly the same size. On this side, however, it is set "solid" and looks smaller than on the other. According to Wundt (4te Aufl., II, 150), this is because the eye passes over the same number of letters in a shorter course.

Here the lines are "leaded," i. e., have greater space between them. Is it not possible that the illusion is based on the greater general whiteness in this case and blackness in the other ?

For a collection and discussion of a number of other illusions, some similar to those of preceding experiments and some different, see Lipps, *A* and *B*.

On the Geometrical Illusions in general see Wundt, *A*, 3te Aufl., II, 115-132, and 4te Aufl., II, 137-154. Helmholtz, *A*, F, 720-733 (563-573).

On Ex. 161: Kundt, Münsterberg, 164 f., 175 ff. Wundt, *A*, 3te Aufl., II, 119 ff.; 4te Aufl., II, 137 ff. Helmholtz, *A*, F, 697 (544).

On Ex. 162: Wundt, *A*, 3te Aufl., II, 134 f.; 4te Aufl., II, 142 f. Helmholtz, *A*, F, 720 f. (563 f.). Knox. Watanabe.

On Ex. 163: Wundt, 3te Aufl., II, 125 ff.; 4te Aufl., II, 145 ff. Helmholtz, *A*, F, 722 ff. (564 ff.). Lipps, *B*, 269, 291. Jastrow, *B*. Dresslar.

On Ex. 164: Helmholtz, *A*, F, 722-725 (564-568). Wundt, 3te Aufl., II, 125 ff.; 4te Aufl., II, 144 ff. Zöllner, *A* and *B*. Lipps, *B*, 267 ff. Jastrow, *A*, and the literature cited by him.

On Ex. 165: Müller-Lyer. Jastrow, *A*. Brentano. Lipps, *A*. Delbœuf, *C*. Wundt, *A*, 4te Aufl., II, 149 f. Brunot.

On Ex. 166: Müller-Lyer. Wundt, *A*, 4te Aufl., II, 149 f., 151 f.

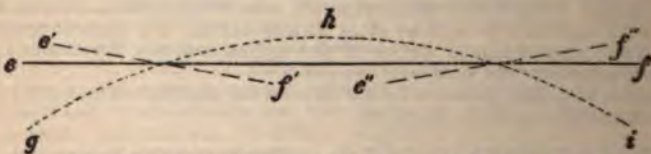
On Ex. 167: Láska. Delbœuf, *A* and *B*. Wundt, *A*, 4te Aufl., II, 146, 147, 150. Brunot.

168. Illusions depending directly on movements of the eye.

a. Move a pin head along the imaginary line *CD* in the figure below, keeping the eye constantly fixed on the pin head as it moves. The line *AB* will seem to move downward and to the left as the pin head goes from *D* to *C*, and upward and to the right as it goes from *C* to *D*. Steady fixation of the pin head is essential; a moderate rate of movement, which can be found by a few trials, gives the best result. The right and left movement of *AB* may be increased by moving the pin head in a line more nearly horizontal than *CD*. In this case movement of the image of *AB* on the retina is interpreted as movement of the line instead of the eye. For the fuller consideration of such cases see experiments on the perception of motion to follow.



b. An illusion affecting the direction of a line is to be observed when a compass point is made to draw an imaginary arc, cutting the line *ef* as the dotted arc *ghi* does in the figure below. As the point advances from *g* to *h* the line appears to take a position like that of *e'f'*; as the point traverses the region about *h* there is a sudden change, the line inclining now in the direction of *e''f''*. As before, constant fixation of the moving point is essential.



c. Something resembling *a* above is to be observed when the eye follows the movement of a pin head moved to and fro across the Zöllner figure in Ex. 164. As the pin goes from left to right the first, third, fifth and seventh black bands move downward, the second, fourth and sixth move upward. When the pin goes from right to left, the movement of the bands is reversed. The apparent inclination of the bands is also increased by the movement and the ordinary illusion intensified. The upward moving bands incline toward the side from which the pin starts, the downward moving incline in the opposite way. Moving the pin in a line parallel to the bands, decreases or abolishes the ordinary illusion. The illusion

of motion in the bands is evidently suggested by an illusory movement of the short oblique lines induced by the moving pin head in exactly the same way as the movement of the line *AB* in *a* above. Constancy of fixation is important here as before; some observers may find this easier to accomplish if the pin is held still and the diagram moved behind it. A certain moderate rate of movement which may easily be found by trial is best. The writer finds some help from bringing the diagram rather near the eye, i. e., within six or eight inches.

The apparent movement so strongly present in this experiment is regarded by Helmholtz as the key to the explanation of the Zöllner and similar illusions. That such an illusory motion might be a factor is strongly suggested by the "unsteadiness" of the figure on casual examination. (See also *d* below).

d. Near the edge of the table drive a couple of small nails about two feet apart and lay a sheet of paper between them. On the paper and against the nails on the side away from the edge of the table lay a meter-stick or other long ruler. Take a pair of compasses of large span, say a foot; set one point close to the rule at one side and bring the other point down to the rule about the middle of the paper. Short arcs drawn with the compasses in this position would not differ much from perpendiculars to the rule. Bring the compass point to a division of the scale, fixate the division mark and then move the compass point away, and slide the rule to the right or left, at the same instant, following the division mark with the eye. The movement in each case should be three or four inches. If the rule has moved to the right and the compass point away from it, the imaginary line traced by the latter will seem a good deal curved and inclined to the left. If the rule has moved to the left, the line will appear nearly straight and inclined to the right. If the compass point approaches the rule instead of leaving it at the instant of movement, the results will be reversed, movement of the rule to the right giving inclinations to the right, and toward the left inclinations to the left. The movement must not be so fast as to prevent clear seeing of the relation of the rule and compass point. Invariable fixation of the division of the rule is important.

When this experiment is compared with *c* above, it will be observed that the inclination in this case is the same as that of the bands in Zöllner's figure when the pin is moved over them. The movement of the eye in following the pin head corresponds to that of the eye in following the rule. The heavy dark bands correspond to the imaginary line drawn by the compass point. The illusion of Zöllner's figure would, therefore, depend on the apparent motion of the bands, which in turn depends on the movement of the eyes. The dislocation of the lines in the case of the Zöllner's figure is, however, considerably less than in that of the rule and compasses, for such an excessive dislocation would make the lines appear to cross, a state of things that could not be harmonized with other parts of the perception.¹

Helmholtz, *A, F*, 727-730 (568-571).

¹Helmholtz thus seems to give two explanations for the same illusion. In explanation of this, he says (*F*, 730, (571)): "It may be found surprising that I should derive the same illusion from two causes so different in appearance. But if it is recalled that in my opinion the knowledge of the measurements of the visual field in indirect vision, rests upon experience previously had by the aid of movements, and that the present movements of regard are accompanied by similar new impressions, it is seen that the two cases are not so different as they may seem in exposition; they do not differ, except as the memory and present aspect of similar circumstances."

169. The geometrical illusions viewed with unmoved eyes. Many of the illusions considered above are much weakened, and some entirely removed when eye movements are excluded. This may be done by fixation of the eyes, somewhat better by getting the figures as after-images, and most satisfactorily of all by instantaneous illumination. Try the effect of steady fixation on the figures of Ex. 163-164.

The after-image method may be tried on the Zöllner's figure (first figure under Ex. 164), which can be made to give a strong after-image as it stands, and on any of the other figures by cutting them in narrow slits in black cardboard, and then viewing them against a bright background.

The fact that many of these illusions are still present in a certain degree when movements of the eyes are excluded, does not demonstrate that any part of them is of non-motor origin. Says Wundt (4te Aufl., II, 139): "If a phenomenon is perceived with the moved eye only, undoubtedly the influence of movement upon it is proved; but it cannot be inferred in the other way, as is now and then done, that movement is without influence on a phenomenon, that persists [with the eye] at rest." As has already been shown in Ex. 158, the experiences of the eye in motion are retained and applied to its perceptions when at rest.

Helmholtz, A. F. 725 ff. (567 ff.).

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APPENDIX.

THE FIELD OF REGARD AND LISTING'S LAW.

Experiment 158 of this section requires a somewhat fuller understanding of Listing's Law than can be gathered from Ex. 123, where the subject was previously treated. It has, therefore, seemed best to attempt a fuller exposition of it here.

Listing's Law as stated by Helmholtz, is as follows: "When the line of sight passes from the primary position to any other position, the angle of torsion of the eye in its second position is the same as if the eye had come to this second position by turning about a fixed axis perpendicular both to the first and the second position of the line of sight."¹

On this principle rest two important corollaries: 1st. In movements from the primary position, there will be no rotation about the line of regard. 2nd. In movements from one secondary position to another, there will be some rotation about the line of regard.

The Hemispherical Field of Regard.

The usual way of putting the law to experimental test is to get a strong after-image of a rectangular cross on the centre of the eye, and then to observe the changes that its projected image undergoes as the eye is turned to one point and another of its field of regard. In the model from which the accompanying stereoscopic diagram is taken, an attempt has been made to show the changes

¹Helmholtz, *Optique physiologique*, p. 606, (466); Le Conte, *Sight*, 174.

that such an after-image would undergo when projected upon different parts of the hemispherical field. The primary meridian of this field is $A * B$,¹ and other meridians are shown at intervals of 20° . The equator of the field (that is the line of intersection of the plane of regard with the hemispherical field of regard, when the eyes are in the primary position) is $C * D$, and above and below it are shown parallels at intervals as before of 20° . The eye itself is supposed to be at the centre of the sphere, i. e., in the plane of the letters A, K, G, N , etc., and at the centre of the circle that they mark. When the eye is in its primary position, it is directed forward and fixed upon the central eight-rayed cross. Let us suppose that the eye takes a lasting after-image from the cross, but first from the horizontal and vertical rays only. If, now, the point of regard is elevated or depressed in the primary meridian, and there is no rotation about the line of regard, the vertical bar of the after-image cross will still be found to lie in the meridian; and if the point of regard be carried to the right or left in the equator of the field, the horizontal bar will still lie in the equator. This is shown by the slender crosses 40° from the centre on $A * B$ and $C * D$. The axes about which the eye turns are evidently in the plane of the letters A, K, G, N , etc., and coincide in the first case with the diameter CD , and in the second with the diameter AB . Suppose now, that the after-image has been taken from the oblique bars of the central cross, and that the movement of the eye has been oblique to the right and upward, and to the left and downward along $H * G$, and to the left and upward, and to the right and downward along $E * F$, but without rotation about the line of regard. As before, those bars of the cross which originally coincided with these lines will be found to coincide with them after the movement, as shown by the corresponding bars of the slender crosses in these positions. The axis for movements in $G * H$ lies in the diameter EF , and that for movements in $E * F$ in the diameter GH . For any intermediate directions of movement, the axes would have a corresponding intermediate position, but in all cases the axes would lie in the plane of the letters A, K, G, N , etc., perpendicular to the line of regard, both before and after its movement.

Since these after-images are always projected on a hemispherical surface there is no distortion of any of the crosses, and all of their parts maintain exactly the relations among themselves which exist among those of the central cross. It will be observed, however, that in the oblique positions the bars corresponding to the vertical of the central cross, do not quite coincide with the meridian passing through the centre of the crosses, but make a small angle with it, and that in the same way the bars corresponding to the horizontal in the central cross have no longer the same direction as the parallels above and below them. In other words the vertical and horizontal bars appear to have rotated, though the fact that the oblique bars have maintained their coincidence with the circles $E * F$, and $G * H$ shows that the rotation is not real, but as Le Conte says, "only an apparent rotation consequent upon reference to a new vertical meridian of space." This apparent rotation is known as *torsion*. The rules for this torsion are as follows: Movement of the eyes upward and to the right gives torsion to the right; upward and to the left, torsion to the left; downward and to the right, torsion to the left; downward and to the left, torsion to the

¹In naming the curves of the hemispherical field, the asterisk (*) is used for the central cross instead of a letter.

right—all of which can easily be observed in the stereoscopic figure.

So much for movements from the primary to a secondary position. Movements from any secondary position to the primary are evidently executed about the same axes as before, but in the contrary direction. It remains then to consider movements from one secondary position to another. Let us start with an after-image from the slender cross on $C * D$, 40° to the right of the centre, and move upward along the meridian. The vertical bar of this cross coincides with the meridian at the start; when we reach the position of the eight-rayed cross, however, it no longer does so, but has turned slightly to the right—this time owing to a true rotation about the line of regard, and not to reference to a new meridian. The amount of rotation is small, in this case about 13° . Movement downward along the meridian would have exactly the same result, except that the rotation would be in the opposite direction, and similar rotations would be found if the cross 40° to left of the centre on $C * D$ had been used for vertical movements, or the crosses 40° above and below the centre on $A * B$ had been used for horizontal movement along great circles.

If movements from secondary positions along great circles are attended with this deviation of the bar of the cross from the line in which it moves, are there any lines to be found along which the eye may move the after-image without finding such a deviation? There are such lines, and four of them are shown in the figure. They are the arcs IJ , KL , MN and OI . It will be seen that these are drawn through the sloping positions of the bars corresponding to the vertical and horizontal bars of the central cross, and are perpendicular to $A * B$ and $C * D$ like the bars of the side crosses. Along these lines, a short line or after-image can be moved without leaving the line, a peculiarity in which they resemble a straight line, and when seen with the eye at rest under proper conditions they do actually appear straight. These are the Circles of Direction or Right Circles, (*Cercles de Direction*, *Richtkreise*) of Helmholtz.¹ The verticle circles of direction have, it will be observed, somewhat greater curvature than the meridians through the same points, and the horizontal circles of direction somewhat less than the parallels near which they lie. These circles have the further peculiarity that they all pass through the *occipital point*, a point as far behind the eye as the primary point of regard is in front of it. Both of these properties are shared also by all the great circles passing through the primary point of regard, so that they also are circles of direction. Circles of this kind, great or small as the case may be, can be passed through any two points in the field, and are not limited to those shown in the figure.

The mathematical study of Listing's Law shows that the movement from one secondary position to another may, like those from the primary position, be conceived as rotations about fixed axes all of which lie in a plane, (though in this case the plane is not perpendicular to the line of regard), and that in every case there is also a line about which there is no rotation, the *atropic line*, though this does not coincide with the line of regard.

The Plane Field of Regard.

The experimental testing of Listing's Law is generally carried out with the plane, instead of the hemispherical, field of regard,

¹ *Op. cit.*, 636-637, 703-713, (492-493, 548-557.)



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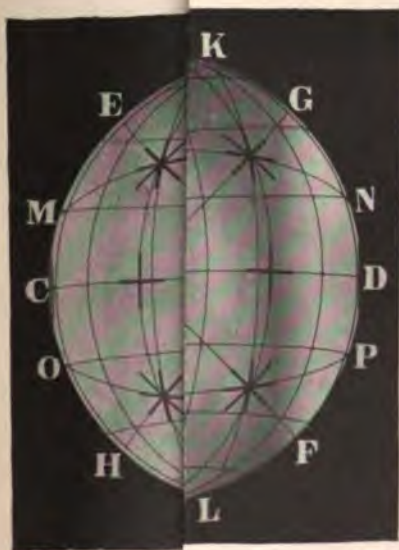
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council shall be brought before the association and decided by a majority vote.

ART. III. *Officers.* There shall be annually nominated by the council and elected by the association a president, and a secretary, and a treasurer, who shall perform the usual duties of these officers.

ART. IV. *Annual Subscription.* The annual subscription shall be three dollars (\$3.00) in advance. Non-payment of dues for two consecutive years shall be considered as equivalent to resignation from the association.

ART. V. *Executive Committee.* The president, the secretary and a member from the place where the meeting is held shall be a committee to make necessary arrangements for the annual meeting.

ART. VI. *Proceedings.* Such proceedings shall be printed by the secretary as the association may direct.

ART. VII. *Amendments.* Amendments to the constitution must be adopted by a majority vote at two consecutive annual meetings.

As prescribed by the constitution, a council was elected as follows:

Term expiring 1897:

Prof. G. T. Ladd, Yale University.

Prof. J. McKeen Cattell, Columbia College.

Term expiring 1896:

Prof. J. Mark Baldwin, Princeton University.

Prof. William James, Harvard University.

Term expiring 1895:

Prof. John Dewey, University of Chicago.

Prof. G. S. Fullerton, University of Pennsylvania.

Prof. J. McKeen Cattell was elected president, and Prof. E. C. Sanford, secretary and treasurer for the coming year.

An invitation was received from the American Society of Naturalists inviting the association to affiliate with it. The question was referred to the council with power to act. Invitations were received for the meeting of 1895 from Harvard University and from the University of Chicago. The decision as to place of meeting was left with the council, with the recommendation that the association meet, if possible, at the same time and place as the Society of Naturalists. It was resolved that the minutes should be printed in such journals as were prepared to print them in full.

The report of the treasurer is as follows:

RECEIPTS.

Balance on hand,	\$69 50	
2 dues 1893,	6 00	
38 dues 1894,	114 00	
Sales of proceedings,	1 60	
		\$191 10

EXPENDITURES.

Printing Proceedings for 1893, as per		
Messrs. Macmillan & Co.'s voucher,	\$55 93	
Postage, expressage and stationery,	8 00	
		63 93
Balance on hand,		\$127 17

The account was audited by the council and approved.

J. MCKEEN CATTELL,
Secretary, 1894.

ABSTRACTS OF PAPERS.

- (1) *The Knowing of Things Together*. Address by the President, PROF. WILLIAM JAMES, Harvard University.

The synthetic unity of consciousness is one of the great dividing questions in the philosophy of mind. We know things singly through as many distinct mental states. But on another occasion we may know the same things together through one state. The problem is as to the relation of the previous many states to the later one state. It will not do to make the mere statement of this problem incidentally involve a particular solution as we should if we formulated the fact to be explained as *the combination of many states of mind into one*. The fact presents itself in the first instance as *the knowing of many things together*, and it is in those terms that the solution must be approached.

In the first place *what is knowing?*

1. *Conceptual* knowing is an external relation between a state of mind and remote objects. If the state of mind, through a context of associates which the world supplies, leads to the objects smoothly and terminates there, we say it knows them. 2. *Intuitive* knowing is the identity of what taken in one world-context we call mental content and in another object. In neither 1 nor 2 is there involved any mysterious self-transcendency or presence in absence. 3. This mystery does, however, seem involved in *the relation between the parts of a mental content itself*. In the minimum real state of consciousness, that of the *passing moment*, past and present are known at once. In desire, memory, etc., earlier and later elements are directly felt to *call for* or *fulfill* each other, and without this sense of mutuality in their parts, such states do not exist. Here is presence in absence; here knowing together; here the original prototype of what we *mean* by knowledge. This ultimate synthetic nature of the smallest real phenomenon of consciousness can neither be explained nor circumvented.

We can only trace the particular conditions by which particular contents come thus to figure with all their parts at once in consciousness. Several attempts were then briefly passed in review. Mere synchronical sense-impression is not a sufficient condition. An additional inner *event* is required. The event has been described: *physiologically* as (1) attention; as (2) ideational processes added to the sensorial processes, the latter giving unity, the former manyness; as (3) motor synergy of processes; *psychologically* as (4) the thinking of relations between the parts of the content-object; as (5) the relating of each part to the self; *spiritually* as (6) an act of the soul; *transcendentally* as (7) the diminution (by unknown causes, possibly physiological) of the obstruction or limitation which the organism imposes on the natural knowing-of-all-things-together by an Absolute Mind. For transcendentalism the problem is "how are things known separately at all?"

The speaker dealt with these opinions critically, not espousing either one himself. He concluded by abandoning the attempt made in his *Principles of Psychology* to formulate mental states as integers, and to refer all plurality to the objects known by them. Practically the metaphysical view cannot be excluded from psychology-books. 'Contents' have parts, because in intuitive knowledge contents and objects are identical; and psychology even as a 'natural science' will find it easier to solve her problem of tracing the conditions that determine what objects shall be known together by speaking of 'contents' as complex unities. [The address will be printed in full in the *Psychological Review* for March, 1895.]

- (2) *Minor Studies and Notes on New Apparatus.* By DR. E. C. SARGENT, Clark University.

The four papers reported were on the following topics: 1. *Comparative Observations on the Indirect Color Range of Children, Adults, and Adults Trained in Color*, by Geo. W. A. Luckey. (This study was made in the psychological laboratory of the Leland Stanford, Jr., University.) 2. *A Study of Individual Psychology*, by Miss Caroline Miles. 3. *The Memory Span and Attention*, by Dr. Arthur H. Daniels. 4. *On the Least Observable Interval Between Stimuli Addressed to Disparate Senses and to Different Organs of the Same Sense*, by Miss Alice J. Hamlin; 5. *Notes on the Binocular Stroboscope, a Model of the Hemispherical Field of Regard, and Diagrams for an Optical Illusion* by E. C. Sanford. [All of these papers are printed in full in this number of the JOURNAL.]

- (3) *The Psychic Development of Young Animals and its Physical Correlation.* By T. WESLEY MILLS, Professor of Physiology in McGill University, Montreal.

As the comparative method of embryology and the doctrine of organic evolution have revolutionized biology, it must be expected that they or their analogues will at least greatly modify modern psychology. To learn how and when psychic processes originate is a long step towards understanding them; and as these processes in animals lower in the scale than man are presumably simpler, it is desirable that they be studied both in the mature animal and in the young developing one. Accordingly the writer has for some years been engaged in this task and has now made fairly complete researches on the psychic development of the dog, cat, rabbit, guinea-pig, etc. An attempt has been made to keep a record, in the form of a diary, not only of psychic but of contemporaneous physical changes. A special series of experiments has been made on the brains of young animals with a view of determining when cortical localization is established, in what order, etc. This work is not yet complete. Incidentally the subject of localization in the mature animal has been investigated and some generally accepted conclusions found unreliable, as well as others confirmed.

- (4) *On the Distribution of Exceptional Ability.* By Professor J. MCKEEN CATTELL, Columbia College.

A study of the mental traits, and of the works of great men forms an interesting chapter in psychology, and while we are undertaking to make psychology an exact science, it is an advantage to secure quantitative results. When anecdotes are published telling us that certain great men have inherited or bequeathed their talents, were insane, immoral, precocious, versatile, or the like, it is of interest, but we sometimes imagine that other examples might be quoted with opposite results or similar traits found in ordinary people. We need to be able to affirm that a man, who has accomplished work making him eminent, is more likely to be insane (according to a proper definition of insanity), than the average man, in a given ratio, and that this ratio varies in such and such a way for men whose work or character was of a given definable sort. And so in all cases quantitative results should be secured. We should be able to say that a man who is a great painter is just so much more likely to be a great poet as well, than is a great soldier or than is the average man.

The first requirement for such a study is a list of great men

secured by an objective method. The 1000 most eminent men have been selected by collating the space given to them in different biographical dictionaries and encyclopædias. The method secures impartiality and an assignable degree of accuracy, it being possible to give a probable error to each man. The list, of course, only gives a man's place in contemporary interest, but this would agree closely with the average verdict of the best judges as to his importance in history. The exact composition of the list is not, indeed, a matter of much importance for the end in view—an objectively selected list of great men being what is wanted. The list was shown at the meeting, curves were exhibited demonstrating the distribution in time and race of the 1000 men, and attention was called to some facts brought out by the curves.

- (5) *Sensibility to Pain by Pressure in the Hands of Individuals of Different Classes, Sexes and Nationalities.* By DR. ARTHUR MACDONALD, Bureau of Education, Washington.

TABULAR STATEMENT OF RESULTS.

No.		Total No.	RIGHT HAND.			LEFT HAND.		
			No. Requiring More Pressure in r. h.	Totals in Kilogrammes.	Averages, Kilogrammes.	No. Requiring More Pressure in l. h.	Totals in Kilogrammes.	Averages, Kilogrammes.
1	2	3	4	5	6	7	8	9
1	American Professional Men,	20	14	74.50	3.72	5	65.25	3.26
2	American Business Men,	14	6	85.25	6.08	6	87.75	6.05
3	American Women—non-laboring class,	27	13	93.25	3.45	6	91.83	3.38
4	English Professional Men,	17	9	88.50	5.20	6	87.25	5.13
5	English Women—non-laboring class,	7	4	43.00	6.14	2	44.25	6.32
6	German Professional Men,	6	5	31.25	5.20	1	29.00	4.83
7	Salvation Army members, London,	8	6	73.25	9.15	2	51.00	7.62
8	Slum Men in Chapel-Rouge, Paris,	9	3	122.50	13.61	2	119.50	13.27
9	Boston Army of the Unemployed,	34	16	332.50	9.77	14	333.75	9.81
10	Women in "Maisons de Tolance," Paris,	9	3	82.00	9.00	5	84.25	9.36
11	Epileptic Patients—laboring people,	3	1	28.00	9.33	1	27.00	9.00
12	Odd ones, men in Paris,	7	4	28.25	4.03	3	26.25	3.75
13	Odd ones, men in different countries,	18	10	96.25	5.34	5	89.50	4.97
14	Men in general,	142	76	1012.75	7.13	19	979.50	6.89
15	Women in general,	46	21	230.50	5.01	15	233.08	5.06

The preceding experiments were made incidentally upon different classes of people. Quite a number of university specialists interested in the subject were experimented upon. The middle of the palmar fossa was chosen and Prof. Cattell's algometer was employed.

Should these results be proved to be generally true by experiments on larger numbers of people, the following statements would be probable:

I. The majority of people are more sensitive to pain in their left hand. (Only exception is No. 10, cols. 4 and 7.)

II. Women are more sensitive to pain than men. (Nos. 14 and 15, cols. 6 and 9.) Exceptions are: comp. Nos. 4 and 5, cols. 6 and 9. It does not necessarily follow that women can not endure more pain than men.

III. American professional men are more sensitive to pain than American business men (comp. Nos. 1 and 2, cols. 6 and 9); and also than English or German professional men (comp. Nos. 1, 4 and 6, cols. 6 and 9).

IV. The laboring classes are much less sensitive to pain than the non-laboring classes (comp. Nos. 1, 2 and 9, cols. 6 and 9.)

V. The women of the lower classes are much less sensitive to pain than those of the better classes (comp. Nos. 3, 5 and 10, cols. 6 and 9).

VI. In general, the more developed the nervous system, the more sensitive it is to pain.

Remark: While the thickness of the tissue on the hand has some influence, it has by no means so much as one might suppose *a priori*; for many with thin hands require much pressure (Nos. 5 and 10, cols. 6 and 9).

(6) *The Freedom of the Will.* By BROTHER CHRYSOSTOM, Manhattan College, New York.

The positive results of the latest studies of the will, through introspection and experiment, are in striking accord with the teachings of the Schoolmen. The appetencies of Aristotle have been replaced by conation, which, if considered in the form of attention, is either *univocally conditioned*, and then corresponds to the sensitive appetition of scholastic philosophy, or is *equivocally conditioned*, and then does not essentially differ from the volition of earlier philosophers. But since equivocally conditioned attention may include among the objects attended to even the attending subject, it must be a spiritual action, for matter is incapable of such reflexive process. In other words, the attending mind is a rational soul. In this light, *apperception* may be characterized as the distinctive quality of conation. But apperception supposes at least such intellectual action as is contained in conception, and this in turn supposes sensation; and thus a point of contact is made with Münsterberg's theory.

Neither a purely autogenetic nor a purely heterogenetic theory of will accounts for all the facts. For conation is not a mere combination of sensations, nor a resultant of affection and sensation, nor does it consist in affection alone. Again, peripheral excitation fails to account for the active element of conation, while exclusively central excitation overlooks external influence. We must, then adopt a theory midway between these two extremes. Wundt, therefore, must be held to state rather the physiological correlate than the psychical fact.

The chief difficulty as to the freedom of the will is found in its connection with the law of causality, which law, however, belongs

to the domain of metaphysics, only indeterminism coming within the limits of psychology. *Cause* essentially connotes the inflowing of the agent upon some subject. But *free* and *uncaused* are not synonyms. All action of the will is voluntary, yet not all its action is free. For although the presentation of pleasurable or painful objects to the will, i. e., the motives, together with the agent's temperament and general subjective condition determine the spontaneous impulse of his will, yet it is a fact of conscious experience that he often can and does put forth at the same time an anti-impulsive effort. Only actions made under these conditions are rightly called free, and they imply essentially the power to will or not to will.

Yet the law of causality, even in that narrower meaning, which obtains in the physical sciences, also applies to free actions in the mass, for we can determine with more or less probability what men taken generally will do under given circumstances. In conclusion, Wundt's assertion that a free act is necessarily an uncaused one, is virtually an admission that the will is superior to material force, and is therefore spiritual.

- (7) *The Consciousness of Identity and So-Called Double Consciousness.* By Prof. GEORGE T. LADD, Yale University.

The questions in debate concerning the consciousness of identity and so-called double consciousness can not be intelligently discussed without a critical examination of the conceptions involved. What then do we mean when we speak of a thing or a mind as remaining "identical" or self-same, through various changes of states? To uncritical thought it doubtless seems as though some unchanging "core" of reality belonged to every being of which we feel ourselves entitled to speak in this way. But philosophical criticism seems rather to assure us only of the proposition: *The real identity of anything consists in this, that its self-activity manifests itself in all its different relations to other things as conforming to law, or to some immanent idea.*

From this it follows that change, in itself, is not inconsistent with identity being maintained. On the contrary, it is the very character of the actual changes observed or inferred, which leads either to the affirmation or to the denial of identity. This principle may be applied to whatever is popularly called a thing, and also to those hypothetical elements of all material things, the so-called atoms.

When we turn to consider the peculiar identity of mind, we find that the affirmation of such identity can never be taken as a denial of change. Indeed, the very real being of mind seems dependent upon change,—in the form, namely, of successive states of consciousness. So that the variety and greatness of the changes experienced *may* heighten, rather than diminish the reality and validity of the consciousness of identity, properly described and understood.

Now, if we inquire in what consists this conscious identity, we see that it is, and can be, nothing but that which is given to consciousness in all states of self-consciousness, of cognitive memory, and of reflective thinking about the Self. To have these states of consciousness is to be conscious of being identical and self-same. And degrees of the consciousness of identity, as it were, are connected necessarily with all real mental development.

In accordance with this metaphysical analysis, we may hopefully and even confidently venture upon the attempt to account for the phenomena of so-called double consciousness, in accordance with certain well-known psychological principles. Of these one may be spoken of as the principle of "psychic automatism." Under this

principle, we note in many of our most familiar experiences such a diremption of successive states, or of very complex present states into two-fold combinations of elements, as makes the full impression of *two* interacting personalities rather than of one person. Yet very subtle and unrecognized or dimly recognized influences of one upon the other, of the Self-conscious Ego upon the automaton, or the reverse, may be distinguished by psychology. All this is popularly expressed either by saying, "I have the automaton," or "the automaton has me;" "I am the automaton," or "the automaton is not-me." Illustrations of all this may be derived from the simpler or more complex bodily operations as under the influence of semi-conscious states, and in turn influencing them; from many deeds of skill and valor and even of a seemingly high order of intelligence; from the phenomena of artistic and religious inspiration, etc.

Closely akin to this is the most effective working of another principle, which we will call that of a "dramatic sundering of the Ego." We can more or less consciously and intentionally, or as forced by circumstances, so "put ourselves into" another character as virtually to divide the Self into two or more selves, whose appropriate states of consciousness either follow in rapid succession or seem to occur almost simultaneously. The phenomena of dreams, the plays of children, the experience of many actors, the phenomena of certain states of inspiration, the imaginative genius of certain writers, like Balzac notably, are instances in point here. Indeed, the very nature of ethical consciousness in its highest form of manifestation seems necessarily to involve such a dramatic sundering of the Ego. In not very infrequent cases, three interacting personalities become manifest in consciousness. These may be described as the tempter or bad angel, the good angel, and the self as the "torn one" between the two.

In fine, it seems fair to expect that by a further understanding and more extended application of these, and perhaps other cognate psychological principles, even the most extreme hypnotic cases of so-called double consciousness may finally be explained.

(8) *A Preliminary Report on a Research into the Psychology of Imitation.* By Prof. JOSIAH ROYCE, Harvard University.

This report first briefly described a collection of experiments now under way at the Harvard Psychological Laboratory, and then passed to some reflections suggested by these experiments, relating to the definition of the functions to be grouped together under the name of imitation. As the text of the report is to appear in the *Psychological Review* the present summary need not be extended. The experiments, which at present are only in their first beginning, have thus far been confined to the imitation of somewhat complex series of taps, given by an electric hammer, and arranged in rhythms. The subjects of the experiments imitate the taps, after hearing each rhythm, through repeating the hammer-strokes by means of an electric key. The rhythms as given and as imitated are recorded on the kymograph. The effects of habit in successive imitations of the same rhythm, the influence of speed and of other factors upon success in imitation are under study. The complexity of the rhythms studied in these experiments forms one special difference of this enterprise when compared with other experimental studies of rhythm; for the purpose is to study, not the rhythmic consciousness as such, but the imitative functions.

Notes of subjective experiences, taken down during or imme-

diately after each experiment, by the subjects concerned, have already given the suggestion for those considerations concerning the definition of imitation, with which the major part of the report was taken up.

- (9) *The Classification of Pain.* By Prof. CHARLES A. STRONG, University of Chicago.

This paper was a discussion of the current theory that pleasure and pain are always given as aspects of a content distinct from themselves—the feeling-tone, “*quale*,” or aspect theory. It sought to test this theory by considering its application to the case of cutaneous pain.

1. Neurologically, we know no facts in regard to cutaneous pain which decisively contradict the theory. For special pain-nerves are more than doubtful; and there is a symptom of locomotor ataxia, consisting of hyperalgesia to heat or cold without hyperalgesia to pressure, and even with analgesia to pricking and pinching, which seems to prove that some pains are distinctively pains of temperature. The condition of analgesia, moreover, while it implies distinct paths for pain in the spinal cord, may be reconciled with the aspect theory by holding that the sensation called forth through these paths, is a tactile or temperature sensation in painful phase.

2. But, introspectively, it is impossible in certain cases to carry out the analysis for which the aspect theory calls. Extreme pressure, heat and cold produce the same sensation—not of heat, or cold, or pressure, but simply of pain. This sensation (*Schmerz*) does not admit of analysis; it is impossible to separate it into a content and an accompanying feeling-tone. But it may call forth an emotional reaction in the shape of a feeling of the disagreeable or intolerable (*Unlust*).

In conclusion, the inference was drawn that pain, being a sensation, may be localized and may leave behind images. [The paper will be printed in the *Psychological Review* for May, 1895.]

- (10) *A Theory of Emotions from the Physiological Standpoint.* By Prof. G. H. MEAD, University of Chicago.

Professor Dewey having shown that it is possible to make a complete teleological statement of the emotions along the line of the discharge theory, it is interesting to see how far such a statement may be paralleled by a physiological theory. This would involve also a physiological theory of pleasure and pain. As pain can be differentiated from the sensations in connection with which it generally appears in consciousness, as it shows itself under circumstances in which the tissue of the end organs or the nerves themselves are affected, and as in the diseases, in which we find pain as a constant concomitant those parts are affected, which are richly supplied with blood vessels by means of supporting and nourishing tissues (Rindfleisch's *intermediärer Ernährungsapparat*), and as in those diseases which pass usually without pain (as in the catarrhs of the various mucous membranes) the tissues affected are poorly supplied with such blood-vessels, and enter into relation with the capillaries generally through the lymph, for the purpose of secretion, it becomes at least probable that, physiologically, pain may be considered as the interference through poisons or violence or otherwise with the process of nutrition as carried out in the finer arteries and blood-vessels. Pleasure must from this standpoint be considered as physiologically the normal or rather heightened process of nutrition in the organs, and the nerve paths

which connect these with the central nervous system, would be probably the sympathetic.

In the simple instinctive act that lies behind every emotion, the vaso-motor system is called into action by the enlargement of the small blood-vessels in the muscles and sweat glands. To maintain the blood pressure, the finer blood-vessels in the abdominal tracts are closed by the constrictors of that region, and the action of the heart may also be increased by the accelerators. The vaso-motor system thus is, in these simpler instinctive acts, in automatic connection with the senso-motor. The act must commence before the flow of blood can take place. It is in connection with this increased flow of blood, that we have to assume the emotional tones of consciousness arise according to the discharge theory. Within the act it would answer only to interest. It is in the preparation for action that we find the qualitatively different emotional tones, and here we find increased flow of blood before the act. We find also what we may term symbolic stimuli, which tend to arouse the vaso-motor processes, that are originally called out only by the instinctive acts. These stimuli in the form in which we can study them, seem to be more or less rhythmical repetitions of those moments in the act itself, which call forth especially the vaso-motor response. In this form they are recognized as æsthetic stimuli, and may be best studied in the war and love dances. It is under the influence of stimuli of this general character that the emotional states and their physiological parallels arise. The teleology of these states is that of giving the organism an evaluation of the act before the co-ordination that leads to the particular reaction has been completed.

- (11) *Desire as the Essence of Pleasure and Pain.* By Dr. D. S. MILLER, Bryn Mawr College.

Pleasure and pain, in the discussion now going forward, as to their classification and physical basis, are commonly treated as among our passive sensory experiences; at all events, it would seem to most psychologists a somewhat stupid paradox to assert that they were in any sense motor phenomena. Yet there is solid ground for holding this paradox; for maintaining, at least, that pleasantness (the quality which along with their specific differences of character marks all so-called pleasures) and painfulness (the quality which along with their specific differences of character marks all so-called pains) are essentially motor facts. A pain is an intolerable feeling; different as they are among themselves, all pains have this, at least, in common, that they are intolerable. No other feeling is intolerable; if it were we should call it a pain. It would then, not be easy to refute the proposition that painfulness is intolerableness; that so-called pains have no other common class-attribute. Now intolerableness is the quality of uniformly provoking a certain bodily disquietude or rebellion, issuing, where the nature of the case permits, in an attempt to escape from the offending irritant. And this is a motor phenomenon. The various *disagreeables* (a term with which "pains" in my meaning is convertible) a needle-prick, a headache, a burn, the numb internal ache of cold hands, the taste of quinine, the smell of assafœtida, the scratching of a slate-pencil, "gnawing pains," "shooting pains," muscular fatigue, disappointment, humiliation—these have no such intrinsic resemblance in sensational complexion as we find among different sights or sounds—between the members of the class of visual, or of the class of auditory sensations; they are similar only in the extrinsic fact that they all alike are accompanied by a bodily

reaction—some flinching or shuddering or convulsion, some restiveness or inner tension—which tends then and afterwards to pass into movements of avoidance, escape or repulse. Now, these movements and the tendencies to them are what we know as *aversion* in its various forms and degrees.

If painfulness is intolerableness, pleasantness on similar grounds, is the quality of being *welcome*. The bodily reaction of *gusto* is as characteristic, though not so obtrusive as that of intolerance; and it tends to pass into movements of retention or procurement. These movements and the tendencies to them are what we know as *desire* in its various forms and degrees.

- (12) *Pleasure and Pain Defined*. By Prof. SIDNEY E. MEZES, University of Texas.

It is necessary to find some fact or group of facts that is present whenever we experience pleasure and absent whenever we do not, and another fact or group of facts present and absent with pain. The frequent confusion of unpleasants with pains is very misleading. Unpleasants are of three kinds: memories and expectations; sensational unpleasants that are not pains—bitter tastes, *e. g.*; and sensational unpleasants that are pains—a toothache, *e. g.* We have here to define pleasure and the unpleasant. Attempts have been made to define pleasure-pains as sensations, as emotions, and as making up the genus of which sensations and emotions are two species. The fact that there is evidence for each of the first two theories shows that neither is exhaustive and competent. Besides the existence of pleasant and unpleasant memories, expectations, and fancies, invalidates all three. Many hold that pleasure-pains are ultimate ideas, simple and undefinable, like colors. There are strong positive objections to this theory, but negatively and for our purposes, it suffices that this theory is a last resort and that its supporters must overthrow all other theories before legitimately claiming it as established. This theory is valuable and true in so far as it points out that neither pleasures as a whole, nor unpleasants as a whole, have any properties in common. It overlooks the possibility that there may be something invariably co-present with pleasures, and some other invariably co-present with pains, and that these two may be the signs to us of the presence of pleasures and pains—what induces us to call a state pleasant or unpleasant. Now Plato, Aristotle, Hobbes, Kant and Schopenhauer agree that harmony or good adjustment is the mark of pleasure, ill-adjustment that of pain. Not all these writers point out the terms between which the adjustment is to obtain, but recently Wundt and Ward have held that the adjustment is of attention to its object. This immediately plausible suggestion of attention and adjustment must be examined. Clearly, what is not attended to is indifferent since uninteresting. Further, immediate attention to pleasures is not the same as that to pains; the former is easy and natural, the latter enforced and obstructed. Again, derived attention, always to unpleasants, is invariably obstructed by the more pleasant rivals to attention also present. May it not be that attention without obstruction is the mark of pleasure, attention with obstruction that of pain? The evidence for this view may be thus suggested: All states of intensely concentrated attention are pleasant, hard thinking, hard play, strenuous work; all states of *internal* conflict—hesitation, practical puzzle, co-present irreconcilable impulses, morbidly insistent ideas, etc.,—are unpleasant, and further, physical pains, owing to their great intensity, reverberate widely and naturally set up mutually obstructive reflexes.

- (13) *Emotions versus Pleasure-Pain*. By Mr. HENRY RUTGERS MARSHALL, New York.

Mr. Marshall reviewed his "genetic" argument in relation to the Emotions, emphasizing the contention that the typical Emotions are named, because (1st) they correspond to relatively fixed relations between the physical elements reacting, and because (2d) these reactions are immediate. Failure of these two conditions can be traced where "instinct feelings" have no emotional names. Emotions are in their nature irregular in recurrence, and to be of value must be forceful in reaction; hence Emotions are not usually lost to consciousness as many "instinct feelings" are, although, if these Emotions become rhythmical and weak, they act as other states do in relation to fixity of habit. Pleasure and pain relate to *organic*, while Emotions relate to *individual* or *racial* effectiveness or ineffectiveness; therefore their genesis cannot be considered to have been co-incident in time, nor to be of the same type.

The identification of Emotion and Pleasure-Pain in "Feeling" is dependent upon the validity of the tripartite division of mind; which is upheld by metaphysical postulation, but not by psychological evidence. Prof. Croom Robertson argued that the exhaustive categories, The True, The Good, The Beautiful, themselves proved the validity of the division. But the existence of the division is explicable in quite another way, as due to the search for Reality. In relation to mental experience in general this search gives us The True; in relation to impression it gives us The Beautiful; and in relation to expression it gives us The Good. If we are to discard this classical tripartite division, we should be able to account for its persistence. It results from an attempt to unify two diverse classifications, both bipartite; viz., 1st, the receptive-reactive classification, and 2d, the subjective-objective classification:—Sensation and Intellect (Knowing) being bound together on both the receptive-reactive and on the subjective-objective schemes; Pleasure-Pain and Emotion (Feeling) being bound together on the the subjective-objective scheme, the receptive-reactive quality being unmarked; Will being marked by a common and co-ordinate emphasis of the reactive and also of the objective qualities. The existence of this tripartite division, thus explained, can therefore no longer be used as an argument for the bond between Emotion and Pleasure-Pain, which states are distinctly separable, the relation between them being this: The emotions are complex psychoses which almost invariably involve repressions or hypernormal activities, either of which are determinants either of pleasure or of pain.

- (14) *Notes on the Experimental Production of Hallucinations and Illusions*. By Prof. W. ROMAINE NEWBOLD, University of Pennsylvania.

Prof. Newbold reported that in twenty-two out of eighty-six cases tried he had succeeded in producing illusions by causing the patients to gaze into a transparent or reflecting medium, such as water, glass, and mirrors. His most successful cases were found among young women under twenty years of age who were good visualizers, but as a majority of his subjects were young women and as the experiments were by preference made upon good visualizers, he was not inclined to lay much stress upon these conditions. The phantasm was usually preceded by cloudiness, flushes of color or of light in the medium, and varied from a dim, colorless outline to a fully developed and brilliantly colored picture. The images were

frequently drawn from the patient's recent visual experience, were sometimes fantastic and frequently unrecognised. The successive images were usually associated, if at all, by similarity, but frequently no relation could be discovered between them. Association by contiguity was excessively rare. The phantasm was frequently, but not always, destroyed by movements of the medium and by distracting sensory impressions and motor effort. Occasionally the phantasm was to a considerable degree independent of the medium, persisted for some time after the removal of the medium, and in one such case appeared to obey the laws of the after-image. The importance of such phenomena upon the question as to the value of the central component in the after-image is obvious.

No trace was observed of telepathic or other supposed supernormal agency. There seemed to be no reason for regarding the phantasms of the glass as any thing other than illusions of the ordinary types depending upon the glass as a *point de repère*. Their chief speculative importance, apart from the light which they may throw upon the after-image, lies in the fact that they present to us processes of association by similarity in concrete, sensible form, and in their possible relation to sub-conscious "automatic" processes. While the phantasms as such cannot be regarded as demonstrating the existence of such processes, it is probable that, if sub-conscious automatism exists, its products may be traceable in the phantasms of the glass. It is possible also that some specific relation exists between the hypnotic consciousness and the phantasm of the glass. Dr. Newbold found that images unrecognised by the waking consciousness were sometimes recollected by the patient when hypnotized, and, *vice versa*, experiments by Mr. F. W. H. Myers have shown that a tale related in hypnosis is sometimes presented in the glass externalise in dramatic form.

(15) *Experiments on Dermal Pain.* By HAROLD GRIFFING, Ph. D., Columbia College.

By means of an algometer transmitting pressure up to 15 kilog. the average pain threshold was found to be for forty college students, 5.5; for thirty-eight law students, 7.8; for ninety-eight women, 3.6; for fifty boys, twelve to fifteen years of age, 4.8. The palm of the hand was the place of stimulation. The most sensitive parts of the body are those where the skin is not separated from the bone by muscular and other tissues.

In eighty experiments on two observers the area was variable, areas of 10 mm., 30 mm., 90 mm. and 270 mm. being given. The corresponding average values of the pain threshold were 1.4 kilog., 2.8 kilog., 4.4 kilog. and 6.6 kilog. Thus the pain threshold increases with the area of stimulation, but much more slowly than in direct proportion.

The time in which dermal stimuli of different intensities cause pain was found by noting the time that elapsed before the appearance of pain after weights had been placed in a balance pan in such a way as to press upon the hand. The averages in seconds, based upon eighty experiments on two observers, are as follows: For 100 g., 230 secs.; for 200 g., 35 secs.; for 300 g., 10 secs.; for 500 g., 4.5 secs. Thus the time, as well as the area and intensity of stimulation, are factors in dermal pain. There is, moreover, an intensive limit below which pressure stimuli never cause pain. Above this limit the sensory effect of the time seems to be in direct proportion to that of intensity.

The pain threshold for falling weights was found to depend as much upon the height as the mass. As both the height and mass

are proportional to the kinetic energy of the moving mass, the stimulus for dermal pain in impact must be considered the energy of the striking object.

- (16) *The Normal Night-Blindness of the Fovea.* By CHRISTINE LADD FRANKLIN, Baltimore.

König's announcement in May, 1894, of the very close co-incidence of the curve showing the distribution of brightness along the spectrum for (1) the totally color-blind, and (2) the normal eye in a faint light, with the curve of relative absorption of different portions of the spectrum by the visual purple (and the obvious inference therefrom that the vision of the totally color-blind and that of the normal eye in a faint light are conditioned by the presence of the visual purple in the retina) made necessary some assumption to take account of the fact that no visual purple has hitherto been found in the fovea. Two assumptions were possible,—either that the cones (and hence the fovea) do contain visual purple, but of such an extremely decomposable character that it can never be detected objectively, or that the eye of the totally color-blind person, and the normal eye in a faint light, are actually blind in the fovea. As I had already made the prediction that total color-blindness consists of a defective development of the cones of the retina (*Ztsch. f. Psych. u. Phys. der Sinnesorgane*, Bd. IV, 1892) and also that the adaptation which renders vision possible after twenty minutes in a faint light is conditioned by the growth of the visual purple (*Mind*, N. S. III, p. 103)—both predictions being naturally suggested by my theory of light sensation. I was most anxious to put the latter assumption to the test, I therefore undertook to determine, in the dark room of Prof. König's laboratory, the threshold for light sensation for different parts of the retina and for different kinds of monochromatic light (the full results of this investigation will appear later). The blindness of the fovea for faint light did not at once reveal itself; the act of fixation means holding the eye so that an image falls on the part of the retina best adapted for seeing it, and hence it would involve keeping the image out of the fovea in a faint light, if the fovea were really blind in a faint light. But after the total disappearance of the small bright object looked at had several times occurred by accident it became possible to execute the motion of the eye necessary to secure it at pleasure. It was then found that the simple device of presenting a group of small bright objects to the eye of the observer was sufficient to demonstrate the "normal night-blindness of the fovea" (as it may best be called), without any difficulty—one or the other of them is sure to fall into the dark hole of the fovea by accident. It was only by means of this arrangement of a number of small bright spots that the total blindness in the fovea of the totally color-blind boy could be detected—he had, of course, learned not to use his fovea in fixation. Prof. König then proceeded to demonstrate the total blindness in the fovea of the normal eye to blue light of wave-length about λ 470.¹ [These experiments upon the normal eye were exhibited at Princeton.] It was shown that König's proof that the pigment epithelium is the only layer of the retina which is affected by red, yellow and green light is not wholly conclusive. The interpretation of the new facts, and their bearing upon the several theories of light-sensation were discussed. [This paper will appear in full in the *Psychological Review* for March, 1895.]

¹Prof. v. Kries is said to have shown that the experiments in question do not establish the blue-blindness of the fovea (*Berichte der Naturforschenden Gesellschaft zu Freiburg*, IX, 2, S. 61). I have not yet had access to this criticism.

- (17) *The Muscular Sense and its Location in the Brain Cortex.* By PROF. M. ALLEN STARR.

[This paper was presented in the absence of Prof. Starr. It may be found in full in the number of the *Psychological Review* for January, 1895.]

- (18) *Psychology in the University of Toronto.* By PROF. J. G. HUME, University of Toronto.

In the University of Toronto, we begin the work in psychology, etc., in the Sophomore year. Up to that time the students are engaged in language studies, Mathematics, English History, Chemistry, Biology, etc. After the Sophomore year, they still continue some of this language study as supplemental to the philosophical course. The latter (beginning with Psychology, Logic and Theory of Knowledge in the second year; Psychology, Logic, Theory of Ethics, History of Ethics and History of Philosophy in the third year) keeps extending until in the fourth year those who have selected this course give *all* their time to the subjects of the course without any supplemental work, taking in the fourth year, Psychology, Ethics, History of Philosophy, Special Reading in the original of various selections from the whole period of modern philosophy, giving special attention to Kant and Lotze.

In Experimental Psychology: Second year, second part of the year: Demonstrations from the director, explanations of methods and practice. In the third year during the whole year, the class divided into groups, is under the charge of the director in the laboratory. In the fourth year, they are supposed to be able to undertake experiments of an independent character. Some of the enquiries started in the fourth year are continued in post-graduate work. In the present fourth year, there are sixteen honor students, conducting four sets of experiments, that is in four groups with four in each group: I. On Time-reactions (mechanical registration instead of the chronoscope). II. Discrimination of Geometrical Figures and Letters in the Field of *Indirect Vision*. III. Discrimination of Color-Saturation. IV. Discrimination and Reproduction of Rhythmic Intervals. In post-graduate study, there are two enquiries being continued from last year: I. Estimation of Surface-Magnitude. II. On Certain Optical Illusions. The director of laboratory, Dr. August Kirschmann, has in the press a recently finished investigation upon the nature of the perception of *metallic lustre*. [This account was presented in the absence of Prof. Hume.]

PSYCHOLOGICAL LITERATURE

I.—NEUROLOGICAL AND COMPARATIVE

By C. F. HODGE, Ph. D.

The Insanity of Over-exertion of the Brain. The Lecture given before the Royal College of Physicians of Edinburgh, 1876. By J. M. TUKE; pp. 66, 9 Figs.

"To approach the treatment of the insanities that psychology is hopeless; we have gained nothing in the past, and can hope for nothing in the future. Nothing has been gained, but great harm has been done. The subject of the cause and treatment of insanity has been beset by serious misconceptions which it must take a long time to clear up, though these words appear late in the book, they are the author's treatment of the subject. The only hope of curing insanity from the side of anatomy and physiology is concerned is primarily the brain. Chapter I is therefore a description of a cerebral convolution, and here we see the results of the Golgi-Cajal methods utilized to the study of the only nervous structures but the membranes and the blood and lymph mechanisms are clearly brought out. The competent physician forms mental pictures of the condition of the brain as it is called upon to treat. These for the brain specialist are as true to nature as it has long been attempted to represent the disease of kidney or heart—as perfect as our knowledge of anatomy and histology can give.

Chapter II follows logically with the recent advances in the study of brain or nerve physiology. It is in this chapter that the author finds, "Causes of Implication of the Apparatus." The question is first discussed both as to quantity and quality of the blood and lymph. Here Mosso's work naturally comes in connection with the hyperæmia during mental work, and that of others the anæmia of sleep. These facts are further discoveries of Mosso that brain work was increased when blood from a fatigued animal is transfused into one

the neurological laboratory of Clark University. While fatigue is within physiological limits, although extreme, a comparatively rapid recovery may occur. Under this head Dr. Batty Tuke takes occasion to remark, and without doubt truly, that the fatigue in Hodge's experiments, in which the effects of five hours stimulation required twenty-four hours complete rest, had overstepped the normal. Possibly for the ganglion cells of a cat this is true. Still even more pronounced evidences of cell-fatigue can be demonstrated in birds and honeybees at night, and these probably do not exceed the normal. Recovery in these animals undoubtedly takes place much more rapidly.

The area of the cortex found to present changes similar to those pointed out in fatigued nerve cells, is generally confined to the central region, and the large pyramids are first to show a change from the normal. Connected with the hyperæmia which accompanies excessive fatigue we find over this area the pia oedematous, thickened and milky, and the Pacchionian villi often hypertrophied. Contrary to the opinion of many on the subject, Batty Tuke does not believe that any other disease, or the disease of any other organ can act directly as the cause of permanent insanity. Even their indirect influence in worry, pain, loss of sleep, etc., he would limit more strictly than is usually done, and cites in support of his view that many of the most painful and distressing diseases, calculus, fistula, rectal or uterine cancer, stricture, etc., are not specially inimical to brain health. Between extreme conditions of fatigue prostration and insanity it is often hard to distinguish, so hard, in fact, that it is frequently said to be impossible, that sanity and insanity shade into one another by imperceptible gradations. We are glad to have stated by so high an authority that "between the two" (normal and insane conditions), "there is a distinct line of demarcation. This he finds in the individual's reaction to external circumstances. As soon as external impressions begin to lose their influence in determining judgements, normal wear has been exceeded and the mechanism of sensation or association has become seriously impaired."

The most hopeful part of the book is the line of treatment laid down for cases of over-exertion-insanity. Foreign travel, change of scene, etc., so often recommended are as clearly contra-indicated as mountain-climbing would be in pneumonia. Not change of stimuli but cessation of all stimuli is demanded. Rest, rest in bed, should be insisted upon. The patient should be carefully secluded. Possibly the other members of the family may be required to leave the house. Sleep must be promoted by all natural methods, hygienic, dietary and by massage. With Dr. Cowles, the author insists that in conditions of the brain suffering from over-strain drugs are too apt to do harm and delay recovery. They should be used only as a last resort, in any case. In hypnotism, Batty Tuke has no faith. Treatment for from three to six weeks are necessary to produce convalescence and one to three months thereafter for complete recovery. It is during this period that recreation and change may prove beneficial. To make the method of treatment available for the poorer classes each city hospital should have a ward set apart for such cases. Patients should be admitted on a physician's recommendation, and a few week's care at the right time might in no small number of cases restore a man to his work and save the state the expense of years of asylum treatment. No one can have read Dr. Cowles' book on *Neurasthenia* without being impressed by the generally hopeful convergence of view that it is possible to prevent the development of insanity, if nerve-fatigue is recognized and treated before it is too late.

There is but one thing more to be desired and that is that we gain and rationally apply to the conduct of daily life a sufficient knowledge of brain physiology to guard even against an amount of over-exertion which may not be wholly recovered from by the sleep of a single night.

Eine neue Theorie über die Ursachen einiger Nervenkrankheiten, insbesondere der Neuritis und der Tabes. L. EDINGER. Leipzig, 1894. pp. 116.

This paper is one of a series of clinical lectures and consists chiefly of citations of clinical points taken from the daily experience of the author during the last three years. Its main interest is to be found in its general agreement, although covering a much wider field of nervous diseases, with the general trend of Batty Tuke's argument. All nervous diseases must be referred to functional rather than to structural causes. They can all be explained by Edinger upon the theory that nutritional regeneration fails to equal the destruction of substance occurring in the course of functional activity. One and the same exciting cause anæmia, syphilis, et. al. will thus produce disease in that part of the nervous system which is most severely taxed by the work of the individual. In officers, railroad employees and foresters we have tabes of the lower extremities; while from the same general physical condition we find cases of progressive paralysis cropping out in those who are engaged in mental work.

Untersuchungen über den feineren Bau des centralen und peripherischen Nervensystems. CAMILLO GOLGI. Translated from the Italian by Dr. R. Teuscher. Gustav Fischer, Jena, 1894. Quarto 272 pp. 30 quarto plates.

An epoch in the knowledge of the nervous system was marked by the appearance in 1885 of Golgi's book, 'Studi sulla fina Anatomia degli Organi Centrali del Sistema Nervoso.' The Italian edition has long been exhausted, as some of us have occasion to know who have had orders for the book placed for four or five years. The present fine edition of Fischer's will thus fill a long felt need. In it we have Golgi's most important communications between 1871 and 1893. His figures and descriptions of nerve cells with their processes, protoplasmic and nervous, have become so familiar that they need no explanation. But since Golgi first outlined his main positions a number of questions have come to be of immense importance to neurologists and we naturally turn to this latest edition of his works to find his present position clearly stated. Possibly first comes the question: What is the function of nervous and protoplasmic processes? Long before Golgi's work and the discovery of his staining method, histologists had recognized a difference between the processes which arise from the body of a nerve cell; but the methods of Deiters, Gerlach and others failed to demonstrate these to any great length. Moreover M. Schultze demonstrated with apparent clearness that the structure of both axis-cylinder and protoplasmic processes are alike in possessing ultimate nerve fibrils, and that these fibrils may enter a cell by one process and pass out by another without any branching or break of continuity in the cell. The very natural supposition then arose that the cells were connected by their protoplasmic processes, dendrons, with one another and either received sensory impressions or discharged motor impulses through their neurons, which were then supposed to be unbranched. A reflex arc might thus consist of the following parts: First, a sensory neuron, entering the cord through the dorsal root and passing to its sensory cell; second, the dendrons of this cell connecting with those of a motor cell; and third, a neuron passing from a motor cell to a muscle. Golgi succeeded in following out these processes much further than former histologists, to what would seem to be their ultimate terminations, and in no case did they unite with the dendrons of other cells. They did, however, show a general tendency to grow out towards the blood vessels and glia cells in the neighborhood and this fact led Golgi to advance the theory that dendrons are closely connected with the nutrition of the nerve cell. This position Golgi finds no reason to modify. The evidence which has been brought to bear

upon this point chiefly by Cajal and Van Gehuchten is, according to Golgi's view, purely theoretical and is not derived from any new facts discovered by them in nerve histology. The answer to this question is to be found where the reader is least likely to look for it, viz., in the last chapter and upon almost the last page of the book. This chapter is entitled, "Upon the origin of the fourth cranial nerve and a general question of cellular physiology which is connected with it." The particular point here is that the cells which give origin to the fourth nerve have a single neuron and no dendrons whatever. If on Cajal's hypothesis, that the dendrons are the organs of the nerve cells by which impulses are received and that the neuron furnishes the path for the discharge of the nerve impulse, how do cells like these having no trace of dendrons, receive stimuli? In order to bring the cells of the spinal ganglia into his system, Cajal has been obliged to suppose that the neuron to the skin is in reality to be considered a dendron. This, according to Golgi, is seriously straining facts to make them agree with theory.

The second important question upon which we desire to have Golgi's present opinion touches the relation of nerve cells to one another. Do their processes actually unite or do they merely come into contact? Golgi replies to this question with an entire chapter describing "The diffuse nervous network of the central organs of the nervous system and its physiological significance." In his former book he advances the view that the branches of the neurons unite to form a close-meshed network throughout the entire central gray matter. The great complexity of this structure made difficult the demonstration of actual union of processes from different cells; but Golgi now claims to have made preparations which leave no room for doubt. The contact theory has been so ably advocated of late by Ramon y Cajal, Kölliker, Van Gehuchten and others that this word from Golgi is most opportune. Golgi also insists more strongly than ever, if that is possible, upon the characteristic difference between neurons and dendrons, and to the objection of Obersteiner, that the Golgi method does not enable us to distinguish with certainty between these two kinds of processes, Golgi replies that this only proves that Obersteiner has never been able to obtain good preparations.

The bearing of Golgi's view on his conception of cerebral localization may be gathered from the emphasis which he places upon the fact that we have absolutely no subdivision of the cerebral cortex corresponding to the so-called "centres" of the localization school. No anatomical divisions exist, and in histological character the entire cortex is of essentially the same structure. In this there is no denial of a certain degree of localization. Regions, not sharply defined, into which a nerve enters directly or from which it most immediately springs, are naturally more distinctly concerned with its special function. But the presence of a diffuse nervous felt-work including the entire central gray matter must tend to bring us back toward something like the old position of Flourens, viz., that the entire brain, being a unit in structure, is also a unit in function.

Ueber ein neues Eintheilungsprincip der Grosshirnoberfläche. P. FLECHSIG. *Neurologisches Centralblatt*, XIII, p. 674, Leipzig, 1894.

The new division of the cerebral surfaces suggested by Professor Flechsig is the natural result of his long and eminently successful studies upon fiber systems in the brain and the order of their development in the child and human embryo. By these fiber-systems the cerebral hemispheres may be divided into two grand divisions. The first includes those areas which receive, or give origin to sensory or motor fibers (the sensory and motor areas of the localizationalists) besides a few

association fibres. The second great division of the brain has no direct connection whatever with the corona radiata, but contains only association fibres. For sake of brevity Flechsig designates the first class of areas as sensory centres, "Sinnescentren," and they include the optic area around the calcarine fissure, the auditory area in the posterior part of the first temporal convolution, the olfactory area in the hippocampal gyrus and the posterior part of the inferior surface of the frontal lobe, and last the great central motor region about the fissure of Rolando, including the posterior portions of the frontal convolutions.

The second great class of areas, the association centres, "Associationscentren," occupy the four great tracts, terra incognita, not accounted for by the localizationalists. These are the anterior portion of the frontal lobe, the island, a large part of the temporal lobe, and a large region in the parieto-occipital lobe, including præcuneus and the posterior portion of the parietal lobe. The extent of these areas can best be determined in the brain of a three-months old child. At this age almost the entire corona radiata is medulated, and these streams of medulated fibres spray out to distribute themselves solely in the sensory centres above described. Scarcely one-third of the cortex is thus supplied with medulated nerve fibres, and the large association areas comprising more than two-thirds of the entire brain surface are either entirely destitute of medulated fibres or contain only a few scattering fibres which come to them, for the most part, from the sensory centres. It would thus seem at this stage of development each sensory centre possessed its own sensory mechanism distinct from every other. Later, at what age it is not stated, each association centre develops association fibres which unite it with two or more sensory centres, and these fibers are much more numerous than fibers of association which unite sensory with sensory centres. The greatest difference, according to Professor Flechsig, between the brain of man and that of other animals is found in the enormous development of the association centres. Their development, in fact, determines the type of brain and the form of the skull.

Beitrag zur Lehre von der absteigenden Degeneration in Gehirn und Rückenmark und Bemerkungen über die Localization und die Leitungsbahnen der Krämpfe bei der "Absynth-Epilepsie," ROBERT BOYCE. *Neurologisches Centralblatt*, Bd. XIII, p. 466, 5 figs.

Boyce has carried out a long series of experiments upon cats to determine first, the exact descending degenerations connected with the different operations, and, second, the locus, or loci, of origin for the convulsions in, "absynth-epilepsie." The following observations were made: 1. Extirpation of motor areas of one side. This is followed by degeneration of the corresponding pyramid, no other tract being implicated in the least. 2. Extirpation of one hemisphere, or, what amounts to the same thing, hemisection of the mid-brain. After this operation degeneration occurred in the descending root of the fifth nerve, in the posterior longitudinal bundle, both on the same side, and Meynert's and Forel's bundles of the opposite side. These degenerations were studied by Marchi's method and are made very clear by a well selected series of drawings.

Either directly after the operation, or after the animal had recovered, absynth was injected and a record was obtained upon the myograph from the extensor muscles of both fore legs. Asymmetrical epileptiform cramps were found to occur. If contractions had been wholly absent from the side corresponding to extirpation of the motor areas, this would have proved that these areas are the sole loci of origin for the convulsions. It was found, however, that centres exist in the medulla and cerebellum which are capable of originating epileptiform cramps of the typical clonic character. The rhythm is, however, much slower than

upon the uninjured side. Hemisection of the cervical cord stops all contractions upon the operated side, which proves that it is not possible to stimulate the cord in this way. In this connection it will be remembered that Goltz notes the occurrence of epilepsy in dogs from which the cerebrum has been removed, and this without the use of absynth. The most valuable result of Boyce's experiments lies in demonstration of the fact that epilepsy may be looked upon as a reaction of certain centres in the brain to a poison which may pervade the whole system. Its maximal affect is produced, when the cerebral cortex is intact; but centres in the medulla and cerebellum are sufficiently sensitive to be affected. Many cases of epilepsy in man are doubtless due to similar sorts of intoxications, and the fact that the convulsions begin in centres of irritation, *i. e.*, foci of highest sensitiveness, is further support for the generally accepted views. It would seem reasonable, however, that treatment should begin with the toxic substances in the blood rather than with extirpation of sensitive parts of the brain.

A Microscopical Study of the Nerve Cell during Electrical Stimulation. C. F. HODGE. *Journal of Morphology*, Vol. IX, pp. 449-463, 5 Figs. Boston, 1894.

Changes in Ganglion Cells from Birth to Senile Death; Man and Honey-Bee. C. F. HODGE. *Journal of Physiology*, Vol. XVII, pp. 129-135, Plate IV. Cambridge, 1894.

Die Nervenzelle bei der Geburt und beim Tode an Altersschwäche. C. F. HODGE. *Anatomischer Anzeiger*, Band IX, Su. 706-710, 4 Figs.

The first of the above papers forms a new chapter in the nerve-cell-fatigue-work, reports of which have been given in this JOURNAL since 1889. By means of specially devised apparatus the spinal or sympathetic ganglion cells taken from the same frog were kept for different lengths of time in a gentle stream of salt solution upon the stages of two similar microscopes. Comparable cells were sought out in each preparation and electrical stimulation was then applied to the one and not to the other, and drawings, by means of the camera lucida, as well as careful measurements, were made of both preparations at regular intervals. Thirty-three experiments were made in all with the fairly uniform result that the nucleus could be seen to gradually shrink in the cells to which stimulation was applied. This decrease in size may amount to as much as 58% in twenty minutes but never exceeded a loss of 75%. The cell as a whole did not shrink perceptibly, but after treating with osmic acid the stimulated cells could be seen to be pervaded by irregular light spaces, representing probably the vacuoles figured and described in former papers. The greatest shrinkage of the nucleus observed in the control cells was 19%. Experiment 3 continued for six days, during the whole of which time it was possible to distinguish nuclei, nucleoli and the granulation of the cell-protoplasm. Active changes however, in the nucleus ceased to be discernable after six hours. Curves of nerve-cell fatigue obtained by plotting the shrinkage of nuclei differ somewhat from the curves which were formerly derived from cells while in the body. In the latter case the nuclei shrank rapidly at first, then very slowly, or gained a little, and finally decreased in size quite rapidly again to a condition of apparent complete fatigue. When the cells are removed from the body and placed in a non-nutrient solution, as might be expected, no such intermediate recovery occurs. Thus curves derived from measurements of nuclei during stimulation show a much more rapid decline than in case of cells in contact with their normal nutritive supply in the body. They come, in fact, to closely resemble fatigue curves for an excited muscle. With a stream of saline solu-

tion continually bathing the cells, it is difficult to conceive that fatigue in this case is to any extent due to accumulation of decomposition products. It was possible also to observe changes in the nucleolus. In general this decreased in size during stimulation, and the reason for this shrinkage could be seen by close observation to lie in the fact that granules were extruded from the nucleolus into the nucleus. If a little potassium tartrate, 0.1% be added to Ringer's solution, the nucleoli in the stimulated cells undergo active, apparently amoeboid, changes of form and move about from place to place in the nucleus. They very soon fragment, however, and dissolve. By this method changes resembling those occurring in fatigue can be demonstrated much more quickly than with stimulation of the ganglia while in the living body.

The last paper is an abstract of the second with addition of four figures in the text. The purpose in this investigation is to determine so far as possible the characteristic differences between young and old nerve cells. Especially good material for demonstrating the extreme phases in the process of ageing was supplied by portions of the nervous system from a man dying at the age of ninety-two and of old age apparently uncomplicated by any disease, and to compare with these similar preparations from a male foetus, killed by accident of birth. The brains of twenty-one old bees were also compared with the same number of young bees' brains. They were in every case caught as they emerged from the brood cell and the old bees were selected by age signs, abraded hairs and frayed wings, etc.

In the cerebrum of the old man no abnormality has been as yet detected by methods thus far employed. The cells appear normal in size and number, so far as this can be determined without special counting, and the nuclei and the nucleoli are in all respects normal. The cells of Purkinje were 25% fewer by count in the cerebellar cortex than in a similar preparation from the cerebellum of a man killed by accident at the age of forty-seven but this difference may be nothing abnormal. Both protoplasm and nucleus of these cells also appear considerably shrunken. The most marked abnormality was found in the cells of the spinal ganglia. Perhaps the most important difference here between young and old cells is a failure of the nucleoli in the old cells to stain with osmic acid. The nuclei, also, in the old cells are considerably shrunken and present irregular outlines and the cell protoplasm is filled with pigment. These differences may be most readily gathered from the following table.

	Volume of nucleus.	Nucleoli observable in nuclel.	Pigment much.	Pigment little.
Foetus	100%	53%	0%	—
Old Man	64.2%	5%	67%	33%

Nucleoli, comparable to those of vertebrate nerve cells, are not present in the bee's brain, nor is anything resembling the pigment granules of vertebrates to be seen. Aside from these features, however, comparison of young and old bees yields results quite similar to those obtained from human material. The cell protoplasm in the old bees is much vacuolated and the nuclei are shrunken, often almost beyond recognition. In all the young bees the protoplasm is dense and the nuclei are so large in proportion to the size of the cells that they often are pressed into polyhedral forms. In all the sections, one is also impressed by the much greater number of cells in the young brains. An actual count of comparable groups of cells gives one cell in the old to 2.9 cells in the young. Age changes in nerve cells are so marked and in so many respects resemble the changes produced by fatigue that in making experiments on fatigue, the age of animals compared should be taken into account.

Histological Changes Induced in Sympathetic, Motor and Sensory Nerve Cells by Functional Activity. GUSTAV MANN. *Journal of Anatomy and Physiology*, Vol. XXIX, pp. 100-108, Pl. I.

Dr. Mann's experiments as described in the present paper deal with changes occurring in sympathetic ganglion cells of the rabbit caused by electrical stimulation of from thirty minutes to nine hours; with the retina and occipital lobes of dogs, in which one eye had been exposed to light, while the other remained covered, and with the motor areas and motor cells in the spinal cord of dogs after considerable fatigue and in the resting condition. The method yielding most definite results are methyl-blue or toluidin-bluestaining after hardening in mercuric chloride. The author's conclusions are as follows: 1. "That during rest, several chromatic materials are stored up in the nerve cell, and that these materials are used up by it during the performance of its function." 2. "That activity is accompanied by an increase in size of the cells, the nuclei and the nucleoli of sympathetic, ordinary motor and sensory ganglion cells." 3. "That fatigue of the nerve cell is accompanied by shrivelling of the nucleus and probably also of the cell, and by the formation of a diffuse chromatic material in the nucleus. As far as our author's fatigue changes are described, there is a rather close agreement with results of my own experiments. It is further quite possible that there should be an initial swelling of the cell on beginning stimulation, due, as Dr. Mann suggests, to a flow of lymph into the cell. My experiments were directed chiefly to the demonstration of extreme fatigue and do not cover this point.

The Microscopical Examination of the Human Brain. EDWIN GOODALL, M. D. London, 1894. pp. 186.

We have here presented in a clear concise manner several hundred methods for the microscopical preparation of the brain. The book is at once a compendium for ready reference to all sorts of devices of treatment and a critical statement derived from experience as to the practical value, difficulties, etc., of each method. It is English, and of course we are treated on the same page with formulae calling for drachms and ounces, and grammes and kilos. The metric system is followed in the main however. An appendix of fifty pages is devoted to museum methods.

Schema vom Faserverlauf im Rückenmark. E. VILLIGER. Basel, 1894. 19 pp. with large colored diagram.

In the preparation of the above schema Villiger has made full use of recent results of v. Lenhossék, Pierre Marie, Ramon y Cajal, Déjérine, L. Edinger, and A. Strümpell. The result is a convenient diagram, drawn in perspective, giving all the different kinds of cells, including those of the spinal ganglia with the course of their respective neurons within the cord. Each type of cell has its own color and this is continued into the neurons arising from it. The normal direction of the nerve impulse in each fibre is also indicated with the direction in which degeneration occurs after injury. The plate is almost as good as a model.

Zur Physiologie des Labyrinths, Mittheilung; Das Hören der labyrinthlosen Tauben. J. R. EWALD. *Pflügers' Archiv* Bd. LIX, Sn. 253-275. Bonn, 1894.

By over ten years active experience upon this line of work Prof. Ewald has arrived at a degree of skill which has made him complete master of his difficult and extremely delicate operations. The writer is personally under great obligation to Professor Ewald for a most careful and thorough demonstration of not only the operated animals but of his method of removing the membranous labyrinth. Objections have been raised (by Bernstein and Matte) against Ewald's conclusions, on

the ground that portions of the labyrinth were overlooked in the extirpation, that these parts remaining intact might continue to mediate some degree of audition. The writer is glad of an opportunity to return Prof. Ewald's great courtesy by recording the testimony that any oversight of the kind indicated is out of the question. Without the shedding of a single drop of blood to obscure the anatomy of the parts in the least the entire labyrinth was removed and writer could plainly see the empty bony capsule and at Prof. Ewald's request actually counted the five stumps of the branches of the auditory nerve as they had been torn from their end-organs. There is no possibility of any doubt as to completeness of extirpation. Passing next to the operated animals, there is certainly no more possibility for doubting that they react to certain sounds than that a normal dog reacts to a whistle. The fact that Matte's pigeons did not respond to the report of a percussion cap, and this was the only test according to Ewald that Matte had recourse to, proves little or nothing; since normal birds seldom react to a noise of this kind. The writer has often shot wild pigeons one after the other out of a tree without the remaining birds paying any attention to the report of a gun. We are thus confronted by the fact that animals, wholly deprived of auditory sense organs, are still able to react to certain sounds. Possibility of contact with anything which could vibrate, the method so well known by which the deaf are enabled to appreciate sound vibrations, was guarded against by suspending the cage from the ceiling and supporting the pigeon on a pad of cotton several centimeters thick. The sound is now conducted through a rubber tube twelve feet long from the mouth of the observer to within 10 cm. of the pigeon. The sound which Ewald generally used is a long drawn "Uh" made during inspiration. Disturbance of the air in the neighborhood of the bird would thus be exceedingly slight. In fact, quickly drawing a full inspiration through the tube never caused any response on the part of the pigeon. In his book on the eighth nerve Ewald advances the theory that the stumps of the nerves are capable of being stimulated by sound vibrations. He has now applied arsenical paste to kill the nerves but is still unable to obtain a bird which does not react to sound. For the present, therefore, the author contents himself with a statement of the facts and does not attempt to frame any theory as to the mechanism by which his birds are enabled to appreciate sound vibrations.

Zur Frage nach der Vererbung erworbener Eigenschaften. REH. Biologisches Centralblatt, XIV. Bd. (1894), s. 71-75.

The author, after referring to the views of Hæckel, Darwin, Weismann, Haacke, and stating his belief that the doctrines of Hæckel and his followers are not absolutely antagonistic to those of the school of Weismann, expresses his own view as follows: The question is not one of the inheritance of "operative mutilations," but of "acquired characters." This undoubtedly exists, but it presupposes a fixed *Anlage*, innate (*innewohnend*) and given by the systematic position. To have shown this, is the great merit of Weismann.

A. F. C.

Untersuchungen über die Folgen der Zucht in engster Blutsverwandtschaft. DR. RITZEMA BOS. Biologisches Centralblatt, XIV. Bd. (1894), s. 75-81.

As the result of experimental breeding of the descendants of a tame albino female rat from October, 1886, to 1893, Dr. Bos concludes: 1. Continual interbreeding of close relations decreases the power of reproduction, and may even cause at last complete infertility. 2. It appears, also, after many generations to induce a

decrease of size of body. 3. It is possible, but in no way proved, that the continual interbreeding of close relations causes a greater predisposition for diseases and the occurrence of malformations.
A. F. C.

II.—ANTHROPOLOGICAL PSYCHOLOGY.

By A. F. CHAMBERLAIN, Ph. D.

Suicide among Primitive Peoples. S. R. STEINMETZ. Amer. Anthropol. (Washington), Vol. VII (1894) pp. 53-60.

It has been assumed by many authorities that insanity and suicide increase in the ratio of the civilization of the races. In this article the author of the excellent "*Ethnologische Studien zur ersten Entwicklung der Strafe*," gives us the result of his examination of the literature relating to primitive peoples in the matter of suicide. "It seems probable from the data I have been able to collect that there is a greater propensity to suicide among savage than among civilized peoples, and that its frequency may be owing to the generally more positive faith in the future life existing in the former races which enables them to meet death with greater calmness and a slighter resistance of the instinct and other natural motives tending to conservation of life, and finally the question suggests itself that if suicide is one of the positive symptoms of moral degeneration, as Dr. Winkler suggests, is it possible that moral degeneration is taking place among the primitive peoples?" The motives leading to suicide are generally the same as those active in all civilized societies, a fact which controverts the opinion of Morelli.

African Fetishism. CHATELAIN HELI. Journ. Amer. Folk-Lore. Vol. VII (1894), pp. 303-304.

This is a clear statement, in brief terms, by one who can speak with authority on the subject of African religion. The author's conclusion is worth reproducing here. "The more I ascertain and compare original facts, the more am I impressed with the fundamental unity of the religious conceptions of Chinese, Hindoos, and American Indians, as well as of nominal Moslems, Jews and Christians, with the African negro. They all have a dim notion of a supreme being; they all serve him far less than they serve the spirits, the mysterious forces of nature, and the souls of deceased persons (ancestor worship, etc.), and put their trust in amulets, talismans, incantations, quacks, priests, soothsayers, spirit-its, and the thousand and one manifestations and paraphernalia of the one universal disposition of mankind known as superstition."

African Races. CHATELAIN HELI. Journ. of Amer. Folk-Lore, Vol. VII (1894), pp. 289-302.

After all the books and magazine articles on the "dark continent" this essay comes with refreshing simpleness of statement and lack of racial bias or theoretic askewness. The author, and his researches entitle his opinion to the greatest respect, holds a much higher opinion of the African negro than is wont to be entertained in psychological and anthropological circles, and he is probably right in so doing. Interesting to the psychologist is Mr. Chatelain's declaration: "The four main causes of the cultural inferiority and of the miseries of the African negro's life can be reduced to four heads namely, first, the lack of a written literature; second, the institution of polygamy; third, that of slavery; fourth, and chiefest, the belief in witchcraft. The development of the race and the happiness of the individual depend on the healing of these sores." The author evidently anticipates the adoption

of Christianity by the negro, with a native literature, and the development of a great negro civilization, for he scouts the idea of any final spoliation of the continent by the whites. Another race of importance from a psychological stand-point is the Hamite, of the western branch of which, the Berbers, who have occupied their present habitat from time immemorial, Mr. Chatelain says: "The great civilization of their Egyptian cousins, the luxury of Carthage, the power of ancient Rome, the fire of Islam, have past by or over them, and left them almost unchanged. Never daunted, scarcely influenced, they have, however, adopted Islam but without sacrificing their own individuality. Fierce tribal Independents, they have not even allowed the formation of a national government. Here we have a branch of the white race, naturally the equal of any other, showing no sign of degeneration and from the first in contact with the best civilizations, yet proudly stationary on a level of culture but slightly superior to that of the Central African negro, who for thousands of years has had no civilization within his sight or reach," (p. 294).

Technogeography, or the Relation of the Earth to the Industries of Mankind. O. T. MASON. Amer. Anthropol., Vol. VII (1894), pp. 137-161.

The author of this interesting essay defines technogeography as "the study of the relationship between the earth and human arts and inventions," a sub-division of the broader subject of anthropogeography, "the consideration of the earth in its broad connections with the whole science of man, including his body and his mind, his arts, languages, social structures, philosophies and religions." Prof. Mason proceeds to discuss the earth as the producer of mankind, as a storehouse of materials, as a reservoir of forces, as a teacher of processes, the earth as a whole, as an organized structure, the culture-areas of the earth, the earth as a single culture-area, the earth in relation to the higher artificial life. His paper is a useful contribution to philosophical anthropology.

Migration and the Food-Quest. O. T. MASON. Amer. Anthropol., Vol. VII (1894), pp. 275-292.

This paper, which has as sub-title, "A Study in the Peopling of America," is devoted to a discussion of that problem from the migration-motive of the food-quest. The author "disclaims any reliance upon continents that have disappeared, upon voyages across the profound sea without food or motive, the accidental stranding of junks, or the aimless wandering of lost tribes. When the continent of America was peopled, it was done by men and women purposely engaged in what all sensible people are now doing, namely, trying to get all the enjoyment possible out of life for their efforts." The author is able to see a closer relation between the peoples of America and those of the eastern Asia, than is seen by Brinton and other authorities, but the chief proposition he defends is "this close connection between the two continents has existed for thousands of years, during which the contact between western America and eastern Asia was more and more close, and extended, and unbroken, as we proceed backward in time. Or, to put the matter in another shape, there never was known to history a day when the two continents were not intimately associated."

The Half-Blood Indian, an Anthropometric Study. FRANZ BOAS. Pop. Science Monthly, Vol. XLV (1894), pp. 761-770.

This valuable study is based upon material collected for the department of ethnology of the World's Columbian Exposition, the charge of the section of physical anthropology having been given to Dr. Boas.

The principal facts disclosed by the investigations, of which the author gives a brief summary are : (1) the mixed race is more fertile than the pure stock, contrary to the opinion generally entertained regarding hybrid races ; (2) the statures of Indians and half-bloods show differences which are in favor of the half-bloods. The latter are almost invariably taller than the former, the difference being more pronounced among men than among women. The white parents of the mixed race are mostly of French extraction, and their statures are on an average shorter than those of the Indians ; (3) the facial measurements of the half-bloods are intermediate, the average value being nearer the typical Indian measurement, and remote from the white measurement ; (4) the half-blood has a narrower nose than that of the Indian, with thinner alæ ; (5) the measurements of length of head of the Ojibwa and métis show a gradual increase in length from the full-blood, through the three-quarter-blood to the half-blood.

The Anthropology of the North American Indian. FRANZ BOAS. Mem. Intern. Cong. Anthropol. (Chicago, 1894), 1893, pp. 37-49.

This is a concise account of the general results of the measurement of some 17,000 full-blood and half-breed Indians from all over the North American continent, with the exception of the Arctic coast and the Mackenzie basin. The facts brought out of greatest importance are : (1) The average number of children of Indian women is high, and therefore, the decrease in their numbers can only be explained by the fact that there exists a very high infant mortality ; (2) on an average the breadth of face of the Indian is 1 cm. more than that of the American white (the latter, however, is exceeding narrow, as compared with that of some Europeans) ; (3) on the whole, the North American Indians may be called a tall people ; (4) in the areas where deformation of the head has not obtained, Dr. Boas recognizes four well characterized types of skull which cannot be combined or considered as closely related ; (a) the mesaticephalic (index approximately 79) population of the whole Mississippi valley ; (2) the long-headed Eskimo of the eastern Arctic coast ; (3) the exceedingly short-headed types of the North Pacific coast, and in isolated spots further down the coast ; (4) the long-headed type of southern California. He is inclined to admit an early contact of the Eskimo and Micmacs to explain the low indices of the latter.

Grundzüge der Anthropologie für höhere Lehranstalten, Lehrer-Seminare und Lehrer, sowie zur Selbstbelehrung für jedermann. A. SPROCKHOFF. Revidiert durch Geh.-Rat Prof. Dr. Rud. Virchow in Berlin. *Der Körper des Menschen. Gliederung, Bau und Thätigkeit seiner Organe mit besonderer Berücksichtigung der Gesundheitslehre, sowie der Krankenpflege und der ersten Hilfe bei Unglücksfällen nach Prof. Dr. von Esmarch in Kiel. Zweite, vermehrte und verbesserte Auflage mit 153 instructiven Abbildungen.* Hannover, 1892, XXIX, 290, s., So.

This is an excellent treatise—intended for use in the higher institutions of learning—on anthropology, in the narrower, physical and somatological sense of the term. Brief and clear expositions of the body and its members and organs, their anatomy, physiology and hygiene are given. There is besides a special section (s. 211-266) on the elements of hygiene, and the first procedures in case of sickness and accident. An appendix (s. 267-275) deals with psychic life. The book is provided with a good index and a glossary of scientific (Latin and Greek) anatomical and other terms with their equivalents in German. The ethnological portion of the book (s. 196-210), which is provided with the usual set of race portraits, is the least satisfactory part. The

American Indians are, as usual, classed as Mongols. For a German textbook, however, the work is a vast stride in advance.

The Origin of Sacred Numbers. D. G. BRINTON. Amer. Anthropol., Vol. VII (1894), pp. 168-173.

In this brief paper the author deals with "holy or sacred numbers as observed in the early civilizations, and among tribes living in what we call primitive conditions." His conclusions are: (1) The sacred numbers are preëminently 3 and 4, or derived from these; (2) these numbers represent contrasting or antithetic symbolical notions, and arise from wholly opposite mental perceptions; (3) the number 3 derives its sacredness from abstract subjective operations of the intelligence, and has its main application in the imaginary and non-phenomenal world; (4) the number 4 derives its sacredness from concrete and material relations from external perceptions, and has its application in the objective and phenomenal world; (5) the associations which attach sacredness to these numbers arise in the human mind of the same character everywhere and at all times, so that no theory of borrowing is needed to explain identities or similarities in this respect; (6) ethnic character, however, tends potently to develop especially the one or the other, either the abstract symbolism or the 3 and its derivatives, or the concrete symbolism of the 4 and its derivatives; and conversely the preponderant development of the one or the other of these reveals, with instructive precision, the ethnic character of tribes and nations. In the "three series," we have the various trinities of time, space, position; creation, preservation, destruction; birth, life, death; three worlds; divine triads—the trinities of Buddhism, Christianity, etc. Derived from 3 are 9 and 33, numbers of significance in Teutonic and Hindu mythology. In the "four series"—derived directly from the relations of the human body to the external world about it—we have the four cardinal points, Janus, the four-faced Roman year-god, and the world-wide occurrence of the number four in myth, ritual and ceremony. Derived from 4 are the numbers 7 and 13, sacred or tabu'd numbers in many lands and among many peoples—the 7 planets; the 7 "ancient spaces" of the Zuni Indians; the 7 caves of Aztec legend, etc.; the 13 islands of which the earth consists according to Hindu cosmogony; the 13 months of the North Asiatic and primitive Aryan solar year, etc.

As to ethnic facts, Dr. Brinton states that "the American and Mongolian races revere almost exclusively the 'four series,' for which also the ancient Babylonians had a decided preference; while triads and trilogies are Egyptian and Greek, Teutonic, Keltic, Slavonic, Indic. Dr. Brinton's paper is another valuable addition to the literature of anthropologic psychology.

A Primer of Mayan Hieroglyphics. D. G. BRINTON. (Publications of the University of Pennsylvania; Series in Philology, Literature and Archæology, Vol. III, No. 2), Boston, 1895, VI, 9-152. pp. 80.

The object of this primer is "with the greatest brevity to supply the learner with the elements necessary for a study of the native hieroglyphic writing of Central America," and Dr. Brinton is of all students of this, the problem of American archæology and linguistics, best qualified to perform such a task, combining as he does a terse and vigorous style with a directness of thought, and an instinctive grasp of the essentials of the subject under discussion, that are not often found among those who have treated of this *crux* of Americanists. the graphic system of the Mayas and their congeners in ancient Central America. After a brief introduction concerned with the general character of the Mayan hieroglyphics, the manuscripts and the various theories of interpretation, "alphabets" and "keys," that have been put forth from time to

time, since the effort of Bishop Landa, in 1570, the author proceeds to discuss "the mathematical elements" (pp. 18-36)—numeral system, rhetorical and symbolical use of numbers, methods of counting time, the ritual calendar, the astronomical knowledge of the ancient Mayas; "the pictorial elements" (pp. 37-77)—religion and cosmogony, pictorial representations of divinities, quadrupeds, birds, reptiles, occupations and ceremonies; "the graphic elements" (pp. 78-126)—the direction in which the glyphs are to be read, the composition of the glyphs, analysis of various graphic elements, hieroglyphs of the days, months, deities. The fifth and last section of the book is devoted to reproduction of some seventeen specimens of Central American hieroglyphic texts with appropriate comments and explanations. There are three indexes: (1) Index-vocabulary of Maya words; (2) index of authors; (3) general index; and the typography and general get-up of the work are beyond reproach and worthy of the æsthetic subject with which it deals. As an interpreter of these hieroglyphics, Dr. Brinton takes a position intermediate between the German writers, who maintain that they are mainly or wholly ideographic, and the French school (followed by many Americans), who look on them as largely phonetic, holding "that while chiefly ideographic, they are occasionally phonetic"—*ikonomatic* is the term the author has applied to this system of writing, which at times is practically a rebus. The manuscripts or codices of the ancient Mayas, of which four imperfect examples are preserved, the author thinks are much more astronomical in character than even Dr. Förstemann, who has gone furthest hitherto in this interpretation, believes: "they are primarily and essentially records of the motions of the heavenly bodies; and both figures and characters are to be interpreted as referring in the first instance to the sun and moon, the planets and those constellations which are most prominent in the nightly sky in the latitude of Yucatan." This contention, Dr. Brinton ably supports by evidence that can scarcely fail to convince. It is interesting to learn that Dr. Förstemann's explanation of the 24th page of the Dresden Codex, with which Dr. Brinton agrees, indicates "that it was intended to bring the time covered in five revolutions of Venus into relation with the solar years and the ceremonial years, or *tonalamatl* of 260 days; also to set forth the relations between the revolutions of the moon and of Mercury; further to divide the year of Venus into four unequal parts, assigned respectively to the four cardinal points and to four divinities; and finally, to designate to which divinities each of the five Venus-years under consideration should be dedicated." What a wealth of astronomical and mythological ideas these old Americans possessed, we are only now really discovering.

The sections on the religion of the ancient Mayas, their cosmical conceptions and the representations of their gods and goddesses are supremely interesting, and here the author's acute interpretative instinct is seen at its best. Of the 950 figures of deities in the four codices, 638 (more than three-fourths) have been recognized, of which no fewer than 196 are of Itzamna, the long-nosed, snake-tongued god of life and medicine, who has many manifestations. In his exposition of the graphic elements, Dr. Brinton adds not a little to our stock of knowledge, the ingenious exposition of the "drum" signs being noteworthy. The evolution of the "hand" and "eye" signs is also well brought out.

Altogether, this primer is a contribution to the study of American palæography, which does credit to the distinguished Americanist from whom it emanates, and saying this is to pass the best judgment upon the work. Psychologists cannot fail to find in this little volume, concerned as it is with the beginnings of literature and alphabetic writing, many things of profound and lasting interest.

Se vi sono Donne di Genio. G. SERGI. Torino, 1894 (*Estratto dagli Atti della Società Romana di Antropologia*, Vol. I, Fascicolo 20, 1894), pp. 18, 8vo.

As text for his essay, "Are there Women of Genius?" Prof. Sergi puts forward the following statement: "Morphologically and functionally, woman fails to reach the normal male development, remaining generally behind, as if there were a general arrest of development." Woman in many ways presents traits of childhood, which have disappeared in the adult male. The author's conclusion is: "That woman has not genius like man, is easy to demonstrate; yet, it cannot be doubted that there are many women gifted with high intelligence and energy in literary productions and in the fine arts, but such women are not geniuses." Though there are no woman geniuses, Prof. Sergi writes: (1) "That without being a genius, woman may be the mother of geniuses, for these have received superior characters found in her; (2) being of modest or low intelligence she can give birth of offspring of equal value; (3) being without high intelligence she can be the means of transmitting superior faculties by the paternal or atavistic line; (4) the 'genius' of woman when it exists, is rudimentary, latent, and remains so as a sexual fact; and no milieu or other favorable factor is ever able to develop it to the degree of male genius; (5) there is sometimes an apparent genius, a superiority over the average, but really a male heredity which develops with physical male characteristics in the same woman, as Lombroso has observed; this is an abnormality, an heredity imperfect by lack of corresponding selection in the secondary sexual characters.

Die Seele des Weibes. Versuch einer Frauen-Psychologie. DR. FERD. MARIA WENDT. Korneuburg, 1892, pp. 130.

Brain anatomy shows no inferiority in the brain of women. The specific gravity of the gray matter in woman's brain is greater than in man's. Her brain is absolutely smaller but relatively larger. The weight of man's brain is to the weight of his body as 1:36. Woman's, 1:35. The nerve tracts are shorter enabling impressions to reach the brain sooner. This accounts for the greater mental activity and quickness of women and small men. On the other hand, there is less oxygen in the blood in the case of woman, not favorable to mental activity. This is not compensated for by frequency of heart beat and respiration. To the advantage of woman is her greater sensibility. The threshold, both as regards stimulus and time, is lower. Her finer sensibility is shown particularly in the sense of touch but also in temperature, taste, smell, sight and hearing. Woman has greater receptivity and more rapid and accurate perception. The rapidity of her perceptions and representations is shown in her greater power and rapidity of speech. Her memory is better, particularly for colors, tastes, caresses and for concrete pleasant and unpleasant experiences. She excels in imaginative power and in phantasy, and it seems a pity, says the author, that she has not excelled in music, painting, poetry, and in the other arts, depending upon this faculty. The book is childish throughout and of no psychological value.

X.

La Psychologie des Sexes et ses Fondemens Physiologiques. ALFRED FOUILLEE. *Revue des Deux Mondes*. Vol. 3, 1893, pp. 33.

It has been said that minds have no sex. This might be true if we were pure spirits, but in our present condition our characters receive the impress of our organisms. In psychological problems

concerning the moral and social relations of the sexes, biological considerations have not been sufficiently regarded, but biologists are now introducing elements of great value into the study of the sexes, by an exact characterization of the physical and mental traits of each sex. If it is true that morality and science should follow nature, the origin, characteristics and purpose of the sexes in the evolution of life cannot be disregarded. To entirely obliterate the differences of sex would be, as Geddes and Thompson have said, to "begin evolution upon a new basis."

Since the earliest antiquity philosophers have maintained that woman is an example of arrested development. This idea has been proved false by recent science. It is now shown that the embryo receives a mathematically equal part of maternal and paternal substance, and that the sex is determined by the amount of nourishment provided. Insufficient nourishment tends to produce males, and more favorable conditions of nutrition females, so that the feminine sex, far from being the result of arrested development demands the most favorable conditions of nourishment to determine it. M. Armand Sabbatier had already found that the characteristics of the female are concentration, unification, cohesion,—of the male, division and dispersion. Carrying this method further we observe that the female represents solidarity, inwardness, dependence; the male, differentiation, outwardness, independence. The female represents receptivity, economy; the male, motion, activity, expenditure. In the female the temperature is lower and the consumption of vitality less. The female is calm, tender, altruistic; the male, restless, explosive, egoistic. The female excels in finesse, cunning. It is her forte to wait, observe, divine. Her brain is more refined. The male excels in intellectual effort, in attention, in penetration. His brain is larger. The female represents beauty, the æsthetic element, passivity, impressionability. Her temperament is phlegmatic or lymphatic. The male represents force, the dynamic element. His temperament is choleric or bilious. The female is more apt in particulars, details; the male, in generalization and abstraction. The female is intuitive and, when not ruled by feeling, sees more justly. The male is deductive, analytic, and sees farther and deeper. With the female the association of ideas is in space, by contiguity; with the male, in time, by causality. The female has a better memory, is more imaginative, more positive and practical, not given to Utopias, is reserved, circumspect and prudent, is more conservative and has more common sense.

As we rise in the scale of animal life these sexual differences become more marked. Darwin and Spenser have endeavored to explain them by natural selection, but that explanation is incomplete. An internal, not an external determinism fixes the primary characteristics of the two sexes. The explanation of moral differences should likewise be sought in fundamental organic differences. These result in woman in the complexity and preponderance of the affections. Some have cited the conjugal and maternal love of woman as an evidence of inferiority, because the same is found among the lower animals. But we might apply this mode of reasoning to masculine superiorities, as for example, to courage. The fact that courage points back to early stages of evolution is not a reason for depreciating it.

In considering the defects in woman's intelligence it is difficult to determine those that belong to her nature and those that are the result of inferior instruction during past ages. It is certainly evident that reform is needed in the instruction provided for women,

and her economic and judicial advantages are far from what they should be. Identity of function in man and woman in society is impossible but there should be an equivalence of duties, better regulated by law. To find the just balance that shall insure an equality between duties and rights in the family and in social life is one of the great problems of the future.

Toullée's work is certainly the best that has recently been done on this subject. Nowhere is the want of accurate detailed work in psychology so much shown as in the scattered efforts that have been made thus far to outline a psychology of woman.

Y.

Ueber die Frauenemancipation. Von DR. GUSTAV TEICHMÜLLER. Dorpat, 1877, pp. 95.

The term "Emancipation of Woman" implies her present condition to be one of slavery. The facts warrant this implication. Is this a necessity such as the partial subjection of children is admitted to be? Is the difference between the sexes of such a character as to warrant the assumption that the final aim of woman differs from that of man? All views with regard to woman can be classed under three heads. The first view is that of Aristotle which assumes that the actual condition of woman realizes the design of nature. The second is that of Plato who holds that her individual existence as a moral and intellectual being entitles her to the enjoyment of a like freedom with man. Important as are the reproductive activities they are only a means to the continuance of the race, and can never be to either man or woman an end in themselves. The final end of every human being must be the development of his mental powers, in the possession of which high gifts women are equal sharers with men. To this sound philosophic view the third, that imposed by social necessity sets its seal. The only adequate alleviation of the miseries resultant upon overpopulation is for the state to fit everyone of its children, regardless of sex, to earn a livelihood.

C. H. S.

III.—MISCELLANEOUS.

Entwurf einer ontologischen Begründung des Seinsollen. Von GUSTAV ENGEL. Berlin, Wm. Herty, 1894, pp. 212.

The author is a vigorous philosophical writer, who for nearly forty years has been thinking Hegel's thoughts into both more condensed and more modern form. This is his masterpiece. It is Hegelism complete, and more or less atoned with Darwinism. Duty, being and evolution are one. They comprise all psychologically possible forms of a beginning. What *ought to be* is traced from the lower spheres of number, quantity, time and space to art and morals, till in the union of the inner and outer, conduct and religion, the problem of the possibility of the higher world is answered. The quintessence of Hegelism has never been more succinctly stated.

I Misteri della scrittura. Lettura tenuta al Circolo sociale Trevigiano il 16 Aprile, 1893. GIUSEPPE STUCCHI. Treviso, 1890, pp. 92, 12mo.

In this interesting little book, the author, who is professor of philosophy in the Liceo di Treviso, treats of the "mysteries of writing," graphology—that universal belief in the existence of some connection between the writing of a person and his character,

a belief so common and so wide-spread that it cannot be entirely groundless. After noticing very briefly Severino's "*Vaticinator*," Descuret's "*Medicina delle passioni*" (in which is to be found a study of the writing of Silvio Pellico by the Abbé Flandrin), Henze's "*Chirogrammato mancia*," Delestre's "*Mystères de l'écriture*," and the later works of Michon, "*Crepieux-Jasmin*," "*Deschamp*," etc., Prof. Stucchi remarks: "Graphology, which ought to be a most valuable auxiliary of psychology, has, like this, its essential basis in a third science, physiology;" and proceeds to outline the nature and practice of graphology. His conclusions are: (1) That graphology, like any other science, has a theoretical and a practical part, and from the exact and sure application of theoretical principles comes, with long and patient exercise, special ability; (2) that in order to establish an exact correlation between certain graphic signs and the moral and intellectual qualities of one's self and of others, a better knowledge of one's self and of others is necessary in order to avoid falling into grievous error; (3) that not all the graphic manifestations have the same value for graphological inquiry; (4) that a single writing is insufficient to reveal the nature of a given person.

A. F. C.

Schmerz und Temperaturempfindung. Von PROF. DR. Z. OFFENHEIMER. Berlin, 1893, pp. 128.

This thoughtful and important paper takes a step beyond Bouller, Dumont, Mantegazza, Vel, and scores of other writers on pain, a subject which has been under investigation at Clark University the past year. Pain affects the course of disease, and, indeed, fills the history of medicine, which wars on it. It is a degree, not a kind of sensation. The fact that saponin kills touch and not pain, while chloroform kills pain, but not touch, shows that their centres or conductive fibres or both are different. Pain is not the maximal sensation a sense-organ, but the most intense sensation which follows the strongest stimulus in the vaso-motor nerves. Besides, the interruption of pain conductivity and of vascular innervation, the increase and reduction of the sensations of temperature, have been noted in all fully recorded cases of syringomyelia, or degeneration of the posterior horn of the spinal cord. Touch nerves do not pass here and have no known connection with the horn, hence, so long as temperature sensations were thought to be mediated by tactile nerves, this was inexplicable. Temperature sensations are unique in being composed of simultaneous action of sympathetic and of tactile nerves. What has been called the sympatheticus is composed of two quite distinct groups of fibres, viz., the splanchnicus and the sympathetic system proper. The latter is peculiar among all nerves in that it has centripetal and centrifugal conductivity by its connection with anterior and posterior roots, and also by forking at the peripheral end a second arrangement for centripetal and centrifugal conductivity is provided, of which the latter innervates the nerves, and the former mediates the stimuli which proceed from the tissues. A constant excitation goes from the anterior roots to preserve the vascular tonus, which may be inhibited by an opposite pain current from the periphery, causing relaxation of tonus and hyperæmia.

A Review of Evolutionary Ethics. By C. M. WILLIAMS. Macmillan, New York, 1893, pp. 581.

The first 268 pages are devoted to well made digests of thirteen leading writers on evolutionary ethics, beginning with Darwin and embracing Wallace, Haeckel, Spencer, Fiske, Ralph, Barrett,

Stevens, Carneri, Höfdding, Gizycki, Alexander and Ree. Digests are hard to make, but it is indispensable, in these days of rapidly accumulating literature, that they shall be made, and made systematically and thoroughly. Mr. Williams has acquitted himself pretty well here. Part II. is his own and is devoted to end, will, relations of thought, feeling and will, egoism and altruism, conscience, progress, results, and the ideal, and the way of its attainment. Christianity is defended as a "comforting belief." The discussions are practical and treat of such themes as the labor question, luxury, machinery, Bellamy, education, the status of women, rights of universities, capital punishment, altruism, change of heart, slavery, sacrifice, golden age, democracy, habit, health, want of rest, pleasure, end, law, etc. The length of the discussion is atoned for by frequent summaries. The highest joy of human association is the love of noble characters. The final destruction of the race need not trouble us. A far greater source of present pain is the loss of faith in personal immortality. It leaves death a harder sorrow, but it lends life a new meaning. The good we strive for lies here. We must, therefore, draw closer in sympathy and by mutual kindness render loss less bitter. We must bow to the inevitable and strive to "join the choir invisible of those immortal dead who live again in minds made better by their presence," to scorn the "miserable aims that end with self, in thoughts sublime that pierce the night like stars," and thus enkindle generous ardor, feed pure love, and make the "music which is the gladness of the world."

Psychologie du Militaire Professionnel. Par A. HARMON. Paris, 1894, pp. 216.

This is one of the "social psychology studies," and has excited great interest and opposition. The author's main theme is that armies are a source of crime, and he has striven to give us a work of science and not a collection of scandals. He finds that army life depresses mentality, breeds contempt of human life and physical suffering, causes brutality and grossness, both within and without the profession, and provokes sexuality and legal criminality. Physical analgesia, moral anæsthesia, the fact that all is supported by an *esprit du corps*, the distaste for useful labor, the substitution of brute force for respect for right, — these cause the demoralization, misery, alienation and suicide which statistics show to be so prevalent among military men.

Apperception and the Movement of Attention. G. F. STOUT. Mind, Vol. XVI, 1891.

In this analysis of the thinking process, Stout uses the term "Apperception" in the Herbartian sense. Attention is a motor-process, a muscular action which cannot be sharply marked off from that which produces physical change in external things. It involves actual movement, muscular strain, or at least motor impulse. It is not an occasional act. In the clearness and strength of presentations which successively become salient, there is merely a difference of degree; but between the salient presentation at any moment and the out-zone constituents of mind, there is an unbridged chasm. This unique salience must be due to a specific process which is called attention.

Mental elements, like social elements, group into systems. So long as the system lasts, it prevents its elements from acting in any other system or independently. It may break up and set its components free, or may unite with other systems and thereby

limit the action of each element. "The process by which a mental system incorporates, or tends to incorporate a new element," is apperception. In this synthesis, attention aids apperception by focussing a presentation until the apperceptive system has succeeded or failed in assimilating it. Such assistance is needful where the complexity of an apperceptive group or the novelty of a presentation retards incorporation; it is unnecessary where incorporation has grown easy.

In its activity, the apperceptive system is not isolated; it excites other systems and these tend in turn to act (Coöperation). At the same time, it weakens all those groups which are not capable of combining with it in the same systematic activity (Competition). The more a system coöperates with others, the less it is able to compete with them. A presentation which may be apperceived by different systems, is grasped by the strongest, i. e., by the one whose action has been most recent and intense, has been freshened by repose or stimulated by organic sensation. Intrinsically, the promptness of a system to apperceive depends upon its symmetrical organization, its comprehensiveness, the cohesiveness and sense-character of its parts.

This strength is tested in the *conflict* which arises when "one system in assimilating a new element tends to wrest it from its preformed connection with another." If the attempt succeed, the result is *positive* apperception; if it fail, *negative* apperception; and at times the issue may remain in suspense, as when we are left hopelessly in doubt.

To illustrate this normal inter-action by contrast: In hypnotic suggestibility, a dominant system excited by the hypnotizer, and exercising unlimited tyranny, prevents that mutual competition and coöperation which would correct or dispel hallucination.

Attention is fixed upon a particular presentation by the *feeling* which accompanies apperception. This feeling plus apperception, constitutes *interest*. Among several presentations brought up by sensory impression or association, that one excites interest and is selected by attention which is congruent with the most excitable apperceptive group. To this congruence, the likelihood of apperception is directly proportioned; the likelihood of accompanying attention, inversely proportioned; for when the presentation is well conformed to the group, it glides into place without a ripple of attention.

The presentations successively attended to form the train of ideas. As distinguished from this, the *train of thought* implies that each idea be apperceived by the same persistently dominant system, and that the relation linking each idea to its predecessor, form also a source of the interest through which it attracts attention. This distinction rests upon the organization of mental systems. Thought involves the activity of *proportional* systems, i. e., of systems whose constituent elements are united according to an analogous plan or type, and which apperceive objects otherwise diverse, "merely because they agree in being capable of entering into certain relations."

There is a consequent distinction between reproduction by simple association and *proportional* reproduction. In this latter, it is not the similarity or other special character of the presentations *in se* that determines the revival, it is the analogy of constituent

relations. (Let $\frac{a^a}{b^a} = \frac{a}{b}$; then a^a will call up, not a or b , but b^a ;—though it resemble a .)

A final characteristic of thinking is the reversion of attention to previous links in the train of ideas, giving rise to a modified repetition of it, and avoiding that conflict which previously hindered the incorporation of a presentation in an apperceptive system.

Le Sentiment et la Pensée. Par ANDRÉ GODFERNAUX. Paris, 1894, pp. 224.

This essay in comparative and experimental psychology is dedicated to Ribot and Dr. Magnan. Five chapters are first given to the description of mania, melancholy, hypochondria, ecstasy and chronic delirium. Normal psychology is treated from the stand-points of excitation or depression, and the emotions and association of ideas are treated as muscular tendencies and coördinations. The general conclusion is that the work by which the effort of a tendency, while yet vague and undecided, to specialize itself into more or less complex groups of motor phenomena, corresponds, in the last case of definite muscular coördinations, to the work by which an emotion takes concrete form and creates a definite synthesis of the elements of consciousness.

Lowell Lectures on the Ascent of Man. By HENRY DRUMMOND. New York, T. Potts & Co., 1894, pp. 346.

The last few lectures of this interesting course are omitted, and instead is a long introduction of fifty-six pages. Probably there was never a book that admitted being condensed so completely into a few sentences. Evolution is a grand drama approaching its last act, man. This is the age of the evolution of evolution. The beginning must be interpreted from the end. Darwin too much ignored man. The struggle for the survival of others began with the first care for the egg. The seventy vestigial structures which Weismann enumerates in man show his evolution from lower forms. The arrest of the body came with tools. Now this is a psychical universe. Soul growth begins with feelings which we share with the lower animals. Old age and death show traces of devolution. The evolution of motherhood stands for altruism and love. The father comes later and stands for justice. The family was very slowly unfolded, and is the root of all sexual institutions. The world's history is a love story. Nutrition and reproduction are the roots of selfishness and unselfishness respectively. All is progressive. God does not live in gaps, and love is the consummate blossom of all evolutionary processes. It is the old Pauline charity. The book is a pleasant and very popular summary of the world processes from the atom to the saint. On the whole very liberal and progressive, and to be most warmly commended to all who still feel the old sense of opposition between science and religion, all trace of which the author himself has, however, by no means escaped.

Basal Concepts in Philosophy. An inquiry into Being, Non-Being and Becoming. By ALEX. T. ORMOND, PH. D., Professor of Philosophy in Princeton University. New York, 1894, pp. 308.

Contemporary thought is chiefly marked by its weakness in respect to fundamental philosophical conceptions. This causes sensationalism in psychology and phenomenism in philosophy, and hence comes agnosticism on one hand and monistic pantheism on the other. Intermediate between these the author would ground the world of reality in an Absolute, possessed of supreme intelli-

gence, goodness and love. The author acknowledges "the great debt I owe to my honored teacher, the venerable McCosh, to the spirit of whose realistic philosophy I hope my own work will be found loyal." The author states that he is also indebted to Plato, Aristotle and others. The author is essentially right in his main positions, but only in a sense like that in which the Old Testament prophets of Christ were Christian. He dimly sees the re-revelation of his own doctrines in the newer directions of science, but says very little that might not have been said fifty years ago. The book is an honest, earnest, old-fashioned plea for old doctrines on good old grounds.

The Synthesis of Mind, the Method of a Working Psychology, by Corydon Ford (J. V. Sheehan, Ann Arbor, Mich., 58 pp. 8vo.), is a book full of wild and unintelligible verbiage, and absolutely without value, except as a study in abnormal psychology. H. A. A.

The Reality of the Self, by W. L. Courtney, Esq., M. A., LL. D. (Being a paper read before the Victoria Institute, 25 pp. 8vo.) is a very brief and superficial recapitulation of some of the spiritualistic arguments. It is modest and clear, but neither the paper itself nor the discussion following contains any contribution to rational psychology. H. A. A.

To the growing list of psychological periodicals is to be added a psychological annual—*L'Année Psychologique*—under the management of the distinguished directors of the Psychological Laboratory of the Sorbonne, MM. Beaunis and Binet. With these gentlemen are associated as collaborators, Ribot (College de France), Flournoy (Geneva), Delabarre (Brown University), Weeks (Harvard), V. Henri (Leipzig), Philippe (Paris), Courtier (Paris) and Bourdon (Lille). The four parts of the annual will be devoted respectively to: 1. A full account with abstracts, tables and diagrams of all the important psychological literature of the year just past. 2. A bibliographical index of the literature of 1894—covering more than twelve hundred titles. 3. Original contributions by Binet and Passy, Binet and Henri, Flournoy, Delabarre and Weeks—Prof. Delabarre's paper being an extensive description of the present state of psychology in America. 4. News and Notes of psychological interest. The regular price of the volume, the risk and cost of which are born by MM. Beaunis and Binet without expectation of profit, will be 10 fr. The work will be ready in March and may be had from F. Alcan, 108 Boulevard St-Germain, Paris; or from M. A. Binet, à la Sorbonne, Paris. The names of those in charge of the new annual assure both its character and success.

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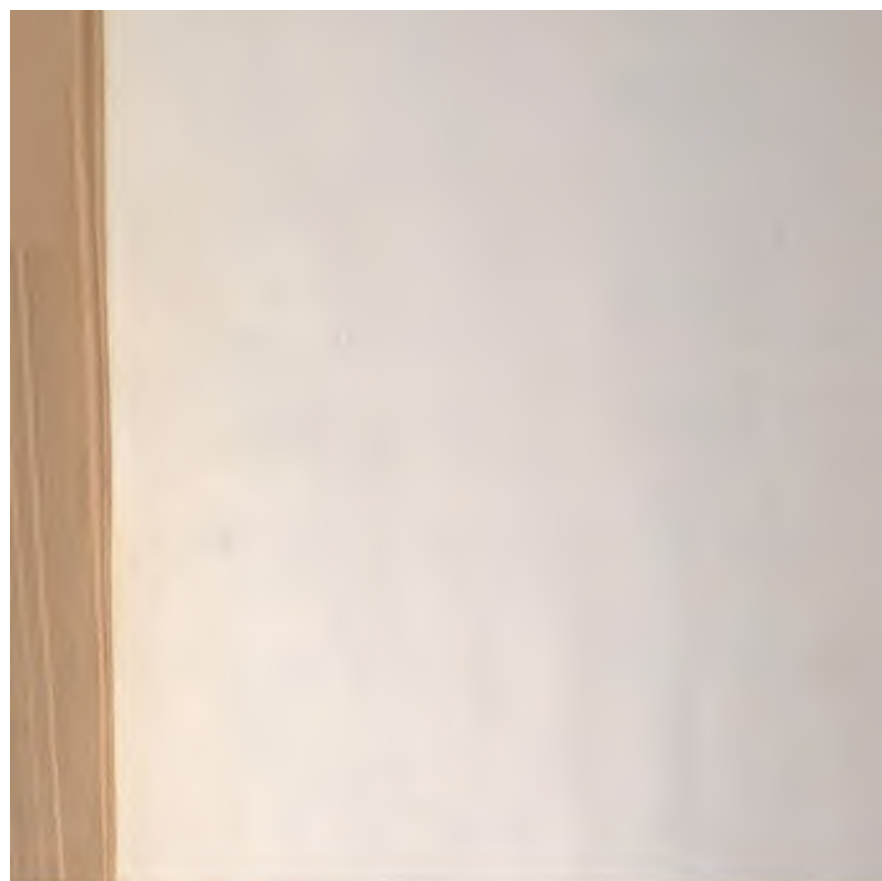
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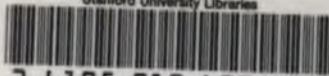
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